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THE LOGIC OF THE NORMAL LAW OF ERROR IN MENTAL MEASUREMENT

By Edwin G. Boring, Clark University

No amount of practically successful "mental measurement" in laboratories, school-systems, factories or the army can relieve us, if we do not wish to waste time, of the necessity of stopping, every so often, to take account of first principles.

Psychophysics, with more than half a century of history to its credit, has repeatedly found the need to eliminate its logically unfit and reorganize its forces: it is a long cry from the principle of the just noticeable difference to the principle of the psychometric function. The mental test as a newcomer had first to prove its right to consideration. Now that it has been accepted it must pass under the critical eye and learn to conform. And what needs to be said, in way of admonition, applies especially to the mental test, although for no other reason than that the mental test is the lustiest form of mental measurement that one meets today. It, especially, merits a discriminating encouragement.

1. The Nature of Probability

We can get nowhere in an understanding of mental measurement unless we appreciate the relation of the unit of measurement to the frequency of occurrence of the different measures. In statistics we deal repeatedly with frequency-distributions; and we know, very often, the frequencies before we are assured as to what they are frequencies of. It is not unusual to attempt to calibrate a scale of measurement in terms of frequencies,— an attempt that always demonstrates the necessity for a definite understanding of the relation between the two. Frequency-distributions, however, have a way of absorbing some of the mystic power that is commonly supposed to inhere in the normal law of error, the 'probability-curve,' which is itself a frequency-distribution. Into the mystery of this function, a mystery connected with its supposed a priori nature, we can pry only by studying its origins, logical and historical. On the logical side we must ask what, in scientific usage, is the nature of a probability. In such a beginning we are setting ourselves no mean task, but a necessary one if we are to avoid muddling.

The early history of the theory of probabilities is a history of the solution of problems arising in games of chance.[1] An applied mathematics of probability preceded the pure. For us the most significant principle that this period brought forth is one that is implicit in the famous theorem of James Bernoulli (1713).

This theorem holds that events tend to occur with relative frequencies proportional to their probabilities. If *a* is twice as likely as *b* to happen, then in the long run it will happen twice as often. But how, we may well ask, are we to know the probabilities of occurrence for *a* and *b*? We must exclude the inverse use of the Bernoullian theorem. We can not say that *a* is twice as likely to happen as *b* because it happens twice as often, for this is simply to reason from frequency to probability and ultimately to identify the two. Our problem was to predict frequency from given probability. How then do we achieve this given probability?

The case of the coin appears at first glance fairly obvious.

> If we may regard an ideal coin as a uniform, homogeneous circular disc, there is nothing which can tend to make it fall more often on the one side than on the other; we may expect, therefore, that in any long run of throws the coin will fall with either face uppermost an approximately equal number of times, or with, say, heads uppermost approximately half the times.[2]

Thus we read in a modern textbook of statistics. The thesis is: the probabilities are equal when there is no good reason for their being unequal. Von Kries has called this law the principle of insufficient reason ("*das Princip des man-*

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[1] I. Todhunter, *History of the mathematical theory of probability*, 1865, is the standard historical work. He begins with Pascal and Fermat in 1654 and continues to Laplace: 624 pp.

[2] G. U. Yule, *Introduction to the theory of statistics*, 4th ed., 1917, 256.

gelnden Grundes"); [3] Boole speaks of it as "the equal distribution of ignorance." [4]

Against an indiscriminate use of the principle of insufficient reason von Kries inveighs. We are asked to consider before the days of spectroscopic analysis the question of whether there is iron in Sirius. We are completely ignorant, but we can conceive of no greater reason for the presence of iron than against it. The probability for the existence of iron in Sirius is therefore ½, on the principle of insufficient reason. Similarly the probability for the existence of gold is ½. It follows algebraically that the probability for the existence of both iron and gold in Sirius is ¼, of iron without gold ¼, of gold without iron ¼, and of neither of these metals ¼. There were, when von Kries wrote, 68 known earthly elements. The probability of the existence of all these elements in Sirius is the 68th power of ½; *i. e.* the chance of all the elements existing in Sirius, as well as the chance of no one of them existing there, is about three in one-hundred-billion-billions. But if we had raised in the first place the question of the existence of earthly elements in Sirius, we might, being ignorant and applying the principle of insufficient reason, have said that it is just as likely that there are no earthly elements in Sirius as that there are some. The discrepancy between the three one-hundred-billion-billionths and one-half leads von Kries to declare that "die Aufstellung der gleich möglichen Fälle muss eine in zwingender Weise und ohne jede Willkür sich ergebende sein." [5]

Plainly it must. Shutting our eyes reduces our faith in what we see to less than one-half. If we would know the nature of our surroundings, we must open our eyes; and, if we would acquire a tenable belief of the chemical constitution of Sirius, we must await the spectroscope. There is no alchemy of probabilities that will change ignorance into knowledge. Expectations must be founded upon cogent rather than insufficient reasons. Nevertheless we can not, it would seem, push this antithesis too far. We can never be completely informed of all the conditions. Thus Westergaard: [6]

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[3] Joh. von Kries, *Die Principien der Wahrscheinlichkeitsrechnung*, 1886, 6. The book is "eine logische Untersuchung." See also A. Kaufmann, *Theorie und Methoden der Statistik*, 1913, 43ff. C. Stumpf, in *Sitzungsber, d. k. bayr. Akad. zu München* (*philos.-philol. Cl.*), 1892, 37-120, in arguing against von Kries, appears to defend the principle of insufficient reason; but in so doing he appeals to a wider analysis of the situation that is not wholly unlike a principle of cogent reason. See especially 41, 61-79.

[4] G. Boole, *An investigation of the laws of thought*, 1854, 370.

[5] *Op. cit.*, 10f.

[6] H. Westergaard, *Grundzüge der Theorie der Statistik*, 1890, 4.

> In every well-arranged game of chance . . . the balls are of the same size, of the same wood, of the same specific gravity, *etc.* They are mixed with the greatest care and every ball is apparently subject to the same forces. Yet such is not the case. In spite of all effort the balls are different, and depart, even though quite insignificantly, from spherical form. One approaches this and another that mathematical form. Every one has a weight and size different from all the others. No ball is absolutely like any of the others. Moreover they can not possibly lie in the bag in exactly the same manner. In short there are a multitude of apparently insignificant differences which determine that exactly this ball and none other shall be drawn.

In the face of such complexity it is hopeless to seek cogent reason for the drawing of the particular ball.

Moreoever, there is a logical as well as a practical reason for our inability to predict in the individual case. Westergaard seems to see this when he implies—it amounts to this—duplicity on our part when we arrange the game of chance. We attempt to make the balls all equal so as to equalize chance, we assume that we have succeeded for all practical intents and purposes, and yet in the same breath we expect to be able to draw some particular one of the balls. However, there must be inequalities, including of course difference of spatial position within the bag, in order to make the drawing possible.

> Did no such inequalities exist, then one of two things would happen: either all the balls would turn up at once or they would all remain in the bag.[7]

In more general terms, we may say that the problem of probability exists only in the face of ignorance. Given all the conditions, we deal not in probabilities but with the certainties of cause and effect.[8]

Here then it would seem we are landed in a dilemma. If we are in total ignorance of the conditions of our problem, all argument is foolishness. If we know all the conditions, we may argue to an effect but not to a probability. Have we to deal then either with a practical impossibility or with a logical absurdity?

The solution lies in the adoption of a definite intermediate position. Fisher lays down a first principle in the case:

> It thus appears that a rigorous application of the principle of cogent reason seems impossible. However, a compromise between this principle and that of the principle of insufficient reason may be effected by the following definition of equally possible cases, viz.: *Equally possible cases are such cases in which we, after an exhaustive analysis of the physical laws underlying the structure of the complex of causes*

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[7] *Loc. cit.*

[8] Thus J. Venn, *Logic of chance,* 3rd ed., 1888, discusses probability as "amount of belief," where "ignorance of the individual is presupposed" and "causation . . . denied, within considerable limits:" p. 122.

influencing the special event, are led to assume that no particular case will occur in preference to any other.[9]

But Fisher, in attempting a compromise, neglects to tell us definitely how much ignorance and how much knowledge make the proper conditions from which we may proceed.[10] We are scientists engaged in very practical work and we wish to know what we may and what we may not do in our statistics.[11]

We can get at the nature of probability by taking a simple instance. The event in which we are interested is, let us say, the toss of a penny. We wish to know the probability that it will come heads. The law of insufficient reason says that, since we can not know that the penny is more likely or less likely to come heads than tails, the probability of heads is ½. The extreme demand for cogent reason would urge that, since we can not know the principal determining factors, this case is insoluble. The 'compromise' that Fisher proposes is that we shall analyze the underlying physical laws. We shall find that one side of the penny is heads and the other tails, that the penny is homogeneous, that it spins many times in the toss, and so on, until we are "led to assume" that neither heads nor tails will be preferred. Now the crux of the whole matter lies in the mechanism by which we are led to this assumption. In the case of the penny no knowledge of physical constitution and of the complexity of tossing would ever be valid ground, in itself, for believing that the penny would come heads as often as tails. In the initial instance this fact must be observed.[12] Thereafter we may reason that other homogeneous, one-headed, well-tossed pennies will in the long run, come heads fifty per cent of the time, and we may even go farther and reason that a homogeneous, six-faced, well-tossed die will in the long run come up equally often on every face. What we do is to assume probability on the grounds of observed frequency and its conditions.

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[9] A. Fisher, *Mathematical theory of probabilities*, 1917, vol. I, 9. An interesting book, written with historical and philosophical perspective for practical purposes. It emphasizes the work of Continental statisticians in a way that English writers do not. Volume II seems not yet to be published.

The position is approximately Kaufmann's, *loc. cit.*, in distinguishing between *mangelnder* and *zwingender Grund.*

[10] It will not keep the statistician from error to tell him that his analysis must be "exhaustive" or that it must be so exhaustive as to 'lead him to assume . . .' Not only do investigators differ in natural thoroughness, but the problem may be restated, as we shall see, so that analysis of the properly known conditions may be pushed to the limits of cogency.

[11] The present paper in its inception is an attempt to answer certain questions that arose in the Office of the Surgeon General of the U. S. Army with respect to the ratings of recruits on intelligence tests. Actually logic is not a far cry from practice, but the technologist upon occasion can be deaf.

Let us state the case in roughly syllogistic form.

> The tossing of a penny gives, when indefinitely repeated, 50% heads;
> The event in which we are interested is a tossing of a penny;
> The event in which we are interested gives, when indefinitely repeated, 50% heads.

This leads us to predict what will happen to our particular penny, if it is tossed repeatedly; and, what is more important, it defines for us the place of cogent reason and the sphere of ignorance. We must know what a penny does when tossed again and again, either directly from observation or indirectly by inference from other observations. The major premise must be based upon cogent reason and there is no limit to the exactitude of the knowledge. Ideally the major premise may be considered to be indubitably established. Moreover we must know that the event in which we are interested is the toss of a penny in the sense of that term in the major premise. Our minor premise, by "an exhaustive analysis of the physical laws underlying the structure of the complex of causes influencing the special event," must also be established and without limit of accuracy by cogent reason. Of what then do we remain ignorant? Of the manner in which the particular, in the middle term, is subsumed under the general. In other words we know that our event is such an event that if repeated it will yield in the long run heads and tails equally, but we do not know whether this particular event, unrepeated, will be heads or tails.

Of an equal distribution of this ignorance, of a law of insufficient reason, we are saying nothing. But have we attained to a statement of probability by saying of a single event that it is such that, if repeated, it would yield a given frequency of particular cases?

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[12] "The assumption that any probability-constant about which we know nothing in particular is as likely to have one value as another is grounded upon the rough but solid experience that such constants do, as a matter of fact, as often have one value as another:" F. Y. Edgeworth, *Mind* O. S. 9, 1884, 230. K. Pearson approves this view: *Grammar of science*, 3rd ed., 1911, pt. i, 146. See also Edgeworth in article on Probability, *Ency. Brittanica*, 1911, xxii, 377, 391. There have been many attempts to supply "the rough but solid experience" empirically; *e. g.* Edgeworth, *Cam. Philos. Trans.* 20, 1905, 128ff.; Pearson, *Chances of death*, 1897, i, 13ff., 48; Westergaard, *op. cit.*, 21-56.

We can not within the narrow limits of this paper escape a certain amount of dogmatism. We must read widely elsewhere on the logic of chance. This much may, however, be said. It is plain that the probability of the event can not inhere in the event. The coming throw is either heads or tails, not half heads and half tails. An event is not even conditioned upon its probability. If, from a box of one thousand tickets, numbered consecutively, one draws any ticket at all, one obtains that particular ticket in the face of odds of 999 to one against it. The rule of such a drawing is that the improbable always happens. If the probability does not lie in the event, it must lie outside it; and we may here assert that it lies in the series to which the event belongs. When we ascribe a probability to a particular event we are simply seeing that event in a series in which the event is repeated as it varies in some particular phase; or, to put it more picturesquely, we see a series of repeated events telescoped within the single instance. This is our habitual mode of dealing with ignorance of this kind. Unable to prophesy the particular event, but able to prophesy its frequency, we content ourselves with the general in view of our impotence to deal with the particular.[13]

(And we may note in passing that ignorance has not bred knowledge in this process. Where reason was insufficient, prediction failed. We never can tell what will turn up next. We may be able to tell what will happen one way often and another way seldom, but that tells us not at all what the next one is).

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[13] The paragraph is compatible with Venn's chapter on *Measurement of belief*, *op. cit.*, 119ff., *q. v.*, although divergent in emphasis. See also the discussion by F. M. Urban, Ueber den Begriff der matematischen Wahrscheinlichkeit, *Vierteljahrschrift f. wiss. Philos. u. Soziol.* 35, 1911, 1-49, 145-185. Only the margins of the logical problem are skirted here. Much might be said of subjective and objective probability. For the scientist-psychologist it is perhaps useful to distinguish four kinds of probability: (1) observed frequencies; (2) expected frequencies; (3) participation in a series of expected frequencies; and (4) psychological expectation. (1) is scientific fact; (2), derived from (1), is a prediction; (3) is a probability, a 'telescoped' (2); (4), the actually observable mental expectation, is a function of (3) and other conditions for determining tendencies, which the psychologist might do well to investigate. See Venn, 152-162. The metaphysical problem of the relation of physical constitution to frequency is beyond the point; actually the relation is observed. In more exact usage it would be necessary to assume that the phrase, 'indefinite repetition,' be taken to mean that the given frequency is approached as a limit when the repetitions are multiplied. The approach would not be continuous; Venn, 118.

We may seem far from the subject of mental measurement, but not without occasion, as we shall see. Let us turn now to the part played by the normal curve of error.

2. The Role of the Law of Error

The normal law of error [14] has been both an inspiration and a limitation in statistical measurement. Its formulation by Laplace resulted during the last century in a wide extension and application of the law to numerous forms of scientific, social and biological measurement; but Laplace is not responsible for all that occurred. There is a bit of magic in the formula. The law came to play the part of a first principle of nature, of an ideal, given *a priori,* to which nature seeks to conform. The mathematicians wrought slowly, but they wrought a god. Against such blind faith later statisticians have protested. They call the normal law a "fetish" and its *a priori* use a "superstition." Nevertheless the "superstition" still lingers and is mixed up with mental measurement. For this reason we are going to enquire, concerning the law of error, what real value it has for us to-day as a scientific tool.

In the days when the application of the theory of probabilities was largely confined to games of chance,[15] the logical difficulties discussed above had little opportunity to make trouble. The fact is that the cards in a pack, the faces of a die, and the sides of a coin do in the long run and under the conditions of gaming turn up about equally often. At this level common sense takes care of the theorist. It makes no practical difference whether the reason he assigns for the equality of occurrence is his lack of sufficient reason for any other result or the empirical fact that these things do work in such a way. Nor does it matter if unwittingly he applies to the single event the frequency that belongs to the series and calls it the probability of the event, for he knows by common sense that probability is not prophecy and that he can not be assured of a realization of his expectations except in the long run. The fact is that the principle of insufficient reason and an indefinite conception of the nature of probability do when applied to games and modified by such unconscious reservations, furnish safe ground for practice, even though the logical implications are dubious.

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[14] The so-called "Gaussian" curve. The mathematical propadeutics for this function were prepared as long ago as the beginning of the 18th century (De Moivre, 1718). See Todhunter, *op. cit.* 71f., 136, 191ff., 552f. Laplace's *Theorie analytique des probabilités* brought together and supplemented in 1812 his work of thirty-five years, and gives the development of the law of error on pages 275ff. The proper date for the law is presumably 1786; see Todhunter, 485. Gauss gives the formula in Article 178 of the *Theoria motus corporum coelestium,* 1809, for which see his *Abhandlungen zur Methode der kleinsten Quadrate,* 1887, 102f. On his ascription of credit to Laplace, see *ibid.,* 207.

[15] The period covered by Todhunter, *op. cit.* Todhunter leaves off unfortunately where our most important period begins.

Laplace and Gauss extended the theory of chance to the probability of errors.[16] Errors are a new material and it is not plain at once that the old assumptions hold. So far as the development of the law of error goes, these mathematicians were content to make their assumptions and reason abstractly to the general conclusion. Laplace worked in the tradition of the old school. He developed the law of error as the limiting case of the binomial expansion when the number of terms is infinite. This process is equivalent to assuming that, in a given case, an error is the resultant of a large number of sources of error, every one of which may affect the result in one way or the opposite (or affect it or not affect it), *i. e.* every source of error may, in the particular case, come up " heads " or " tails," as it were.[17] Either we have, in practice, to assume the principle of insufficient reason, which, applied to errors, does not have the sanction of common-sense experience; or we have to achieve an analysis of the case into sources of error and determine for every one that its law is the law of the coin, a course which is ordinarily impossible. Gauss hypothesizes explicitly that positive and negative errors are equally likely, and derives the law on this basis. Logically his assumption is the same as Laplace's, but it is usually stated in a form that suggests a test. If positive and negative errors are equally likely, then the arithmetical mean is the most probable value. To test the validity of the assumption in practice, we have then only to select a series (of observations, say, which are subject to error) and see whether the arithmetical mean occurs most frequently.

Laplace's derivation suggests the nature of error, Gauss' its mode of manifestation. Both these famous mathematicians were arguing from premise to conclusion when they derived the law of error, and science remains in their debt. We have no complaint to make until we find the law applied without an effort to establish the premises in the particular case. *The*

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[16] *Cf.* W. S. Jevons, *Principles of science*, 1883, 375-385.

[17] Jevons, *op. cit.*, 380f. L. D. Weld, *Theory of errors and least squares*, 1916, 41-56, has an elementary discussion of the binomial expansion and the law of error. For the logic of this derivation applied to psychophysics, see E. G. Boring, *Am. J. Psychol.* 28, 1917, 465ff.

law will not work for errors, unless errors fulfill its conditions.

Gauss was clear about his assumptions, but he did not hesitate to apply the law to astronomical errors of observation.[18] Some of his followers, it seems, who had not witnessed the birth of the law, were less concerned in its application with its parentage. In theory of measurement the law was often accepted on its face value, in spite of the fact that a constantly increasing list of exceptions to it was being made out. In such cases the argument for the law seems to have been the practical one that it gave but little trouble in actual use. Thus Urban recently wrote:[19]

> We are confronted by the shocking situation that a proposition is triumphantly borne out by an immense indirect experience and that it can be proved neither by mathematical deduction nor by direct experience.

Urban, however, did attempt to prove it by showing, under the conditions of the psychophysical experiment, that positive and negative errors of observation, measured from the point of subjective equality, are equal.[20] Faith in the principle was established, however, long before psychophysics furnished this justification.

It is upon Quetelet, it seems, that we must fix the responsibility for the uncritical extension of the normal law to various human measures other than errors.[21] In making a man, he holds, Nature aims at an ideal, and the differences

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[18] On the assumptions: Gauss, *op. cit.*, 4; on the applications: 92ff., 129, 139ff., *etc.*

[19] Urban, *Psychol. Rev.* 17, 1910, 242.

[20] *Op. cit.*, 240-244.

[21] An interest in the application of theory of probabilities to a variety of human problems was of long standing. Todhunter mentions the following initial attempts at application: expectation of life (Graunt, 1662), human testimony (Craig, 1699), human innocence (N. Bernoulli, 1709), birth-rates (Arbuthot, 1710-12), sex-ratios (De Moivre, 1718), astronomical observation (Simpson, 1757), marriages, small-pox, and inoculation (D. Bernoulli, 1760), weather forecasts (Lambert, 1773), annuities (Lagrange, 1798), modes of election (Laplace, 1812), and scientific observation in general (Laplace and Gauss). The important names are N. Bernoulli, De Moivre, D. Bernoulli, Lagrange, Trembley, and Laplace. See Todhunter, *op. cit.*, 37, 41, 54f., 193, 196, 211, 228f., 265, 320, 335, 349, 423, 426, 446, 500f., 546, 589-613, 615ff.

Laplace in the *Essai philosophique sur les probabilités*, which is a separately published introduction to the *Théorie* (*op. cit.*), devotes space to the application of probabilities to natural philosophy, to the moral sciences, to testimony, to elections in assemblies, to the judgments of tribunals, to tables of mortality, and to the duration of life, marriages, *etc.* Gauss is interested in applications to astronomy and geodesy. But all these things were more or less subject to a law of errors. Quetelet's conception of any natural variation as an error is between men represent her degrees of error.[22] Quetelet refers for evidence to the measurement of circumferences of the chests of 5,738 Scotch soldiers and of the heights of 100,000 French conscripts. His position would be all very well were Quetelet content with supporting his hypothesis by this evidence. He seeks, however, to make it work both ways. He suggests that the deviation of the curve for the French conscripts from the normal law is due to fraudulent rejection for military service.

> This . . . law of continuity enables us to recognize a more remarkable fact: we might suspect it, but here we find it proved,—it is that the number of men rejected for deficiency in height is much exaggerated. Not only can we prove this, but we can determine the extent of the fraud. The official documents would make it appear that, of 100,000 men, 28,620 are of less height than 5 feet 2 inches: calculation gives only 26,345. Is it not a fair presumption, that the 2,275 men who constitute the difference of these numbers have been fraudulently rejected?[23]

Of course it is not a fair presumption, since the law depends on the official documents for confirmation. Nevertheless, while admitting the dependence of the law on experience, Quetelet proceeds in numerous cases to analyze experience by means of it. Such a double-edged sword is a peculiarly effective weapon, and it is no wonder that subsequent investigators were tempted to use it in spite of the necessary rules of scientific warfare.

Galton in the *Hereditary Genius* applied the normal law to

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an undoubted extension of the principle, although it is difficult to draw a line.

A. Quetelet was a Belgian statistician, who published an *Essai de physique sociale* in 1835 and a collection of *Lettres sur la théorie des probabilités* in 1846. He writes in the tradition of Laplace with little reference to Gauss. That Gauss' work on least squares was of interest to the French school is evidenced by its translation into French (from the Latin) in 1855, thirty-two years before a similar publication in the German language.

There is no question as to Quetelet's responsibility for the extension of the principle: *cf.*, *e. g.*, W. Lexis, *Zur Theorie der Massenerscheinungen in der menschlichen Gesellschaft,* 1877, 11; Kaufmann, *op. cit.*, 12, 163.

[22] *Letters on the theory of probabilities*, trans. 1849, 91-96. "The difference which Nature makes in the heights of men is not greater than that which inexperience would produce in the measurements taken on one individual man in an attitude more or less curved." "Everything occurs as if there existed a type of man from which all other men differed more or less." "Each people presents its mean, and the different variations from this mean, in numbers which may be calculated *a priori*." (pp. 95f.)

Cf. the discussion and criticism of "l'homme moyenne" in Kaufmann, *op. cit.*, 110ff.

[23] *Letters,* pp. 97f.

mental differences and, using it *a priori,* worked from frequencies of natural ability to a scale of equal intervals of ability. He frankly accepted "the very curious law of 'deviation from an average'" from Quetelet, showed that certain mental capacities obeyed the law approximately, and then argued from the law to "grades of natural ability, separated by equal intervals."[24]

The English school of biometricians, which derives directly from Galton, has furthered the *a priori* use of the normal law that obtains to-day in much biometric and mental measurement.[25] There is an early study by Pearson that illustrates the point.[26] Weldon had measured the lengths and breadths of two large samplings of crabs: one set from Naples and one from Plymouth. The Plymouth batch gave symmetrical distributions, but the frequency curve for the breadths of the Naples batch was skewed. Pearson, by an elaborate mathematical treatment, analysed the skew curve into the sum of two normal curves, the averages of which did not coincide, and Weldon accepted this result as evidence of "dimorphism." Pearson then applied his method to a symmetrical curve for the Plymouth crabs and found that there was no real solution. He assumed, therefore, that these data were "homogeneous." Further he laid down the general rule that some such analysis must be attempted upon every apparently normal curve before a conclusion can be reached as to the homogeneity of the data. If the actual results can be better represented by the sum of two normal curves than by any single normal curve, then there is heterogeneity. Even curves that approximate the normal very closely must be tested, for they may yield to analysis.

Thus Pearson showed that the distribution of shots at a target, given by Merriman,[27] is best represented by the sum of two normal curves. The gun must be thought of as aiming, not at the sixth band of the target as Merriman supposed, but at the fifth and seventh bands (explainable perhaps by a change of sighting during mid-firing). Here then, we find no attempt at all to seek empirical support for the normal law. The crabs were obtained in a single selection and the shots were made in a single experiment, and each set appears, even after it is charted in a graph, as if all the measures belonged together. But we apply the criterion of the normal law and we find the crabs dimorphic and the gun double-sighted.[28]

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[24] F. Galton, *Hereditary Genius,* 1869, 26-36. G. Th. Fechner, of course, had already used the law in the method of right and wrong cases with sensory material. He seems not to have hesitated in his acceptance of the law from Gauss: *Elemente der Psychophysik,* i. 1859, 104. He applied it, not to sensations, but to the *scheinbare Reizgrösse,* the effective inner stimulus: *Revision der Hauptpuncte der Psychophysik,* 1882, 40f., 45. Current notions of "deviation from an average" come undoubtedly, however, from Galton and Quetelet.

[25] See, for example, M. R. Trabue, *Completion-test language scales,* 1916, 29-60, where the normal law is assumed in order to obtain a scale of equal units. Trabue is quite explicit about the assumption (see note, p. 31), but he evidently believes it is correct or he would not devote so much space to developing it.

[26] K. Pearson, Contributions to the mathematical theory of evolution, *Philos. Trans.* 185A, i, 1894, 71-110. The basal data are given by W. F. R. Weldon, *Proc. Roy. Soc.* 54, 1893, 318-329.

Now the very remarkable thing about this easy acceptance of the law of error as the rule of nature is that it has been done all along in spite of very much better available information. Let us get some idea of what has been said.

We saw that Laplace's assumptions were implicit in his method and Gauss' explicit in his text. We had no quarrel with them, but we blamed Quetelet. Did Quetelet's thesis pass, then, quite unchallenged? No, for in the notes to the *Letters* he printed three letters from Bravais,[29] who presented figures on the skew distribution of barometric pressures, discussed various reasons for departures from the normal law, and said in brief:

> I think that generally every partial and distinct cause of error gives place to a curve of possibility of errors (or, if preferred, of differences about the mean), which may have any form whatever,—a curve which we may either be able or unable to discover, and which in the first case may be determined by consideration *a priori* on the peculiar nature of this cause, or may be determined *a posteriori* by observations on the isolated condition of other concomitant causes of error.[30]

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[27] M. Merriman, *Textbook of the method of least squares,* 1884, 14. These data have frequently been quoted as illustrating the application of the normal law to errors.

[28] Of course, if we wish to define "heterogeneity" solely by reference to the applicability of the normal law, nothing can be said, though the usefulness of the procedure might be questioned. The almost unavoidable implication of such a course, however, is exactly that which Weldon got from it, *viz.* that the two normal components point to a biologically significant dimorphism. That idea, moreover, originated with Quetelet: *Letters,* 96f. On the other hand, it is only fair to Pearson to point out that the theory of skew variation in homogeneous material must already have been taking shape; see his distinction in the year following between asymmetry due to heterogeneity and asymmetry due to the mode of variation of a homogeneous material: *Philos. Trans.* 1862, i, 1895, 344f.

[29] Quetelet, *Letters,* 286-295. Venn, *Nature,* 36, 1887, 411f., shows the same asymmetry of barometric heights that Bravais pointed out.

[30] P. 290.

In introducing the letters from Bravais, Quetelet remarked:

> In this memoir I only had in view the examination of the case in which accidental causes have had no tendency to act in one direction more than another.[31]

We have already shown that Quetelet actually established the law by the data and then corrected the data by the law.

In 1869 Galton accepted the law of deviation from an average as applicable to mental ability, but in 1879 he was ready to insist on exceptions:[32]

> My purpose is to show that the assumption which lies at the basis of the well known law of "Frequency of Error" . . . is incorrect in many groups of vital and social phenomena, although that law has been applied to them by statisticians with partial success and corresponding convenience. . . .
>
> The assumption to which I refer is, that errors in excess or deficiency of the truth are equally probable; or conversely, that if two fallible measurements have been made of the same object, their arithmetical mean is more likely to be the true measurement than any other quantity that can be named. . . .
>
> Suppose we endeavor to match a tint: Fechner's law, in its approximative and simplest form of sensation=log stimulus, tells us that a series of tints, in which the quantities of white scattered on a black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the eye to be separated by equal intervals of tint. Therefore, in matching a grey that contains 8 portions of white, we are just as likely to err by selecting one that has 16 portions as one that has 4 portions. In the first case there would be an error in excess, of 8; in the second there would be an error in deficiency, of 4. Therefore, an error of the same magnitude in excess or in deficiency is not equally probable.

Thus Galton argues that the geometric mean is the most probable value with sensory material or data that follow a similar law. The distribution curve in such cases would be markedly skew.

We remarked that Pearson in 1894 gave evidence of being influenced by the sanctity of the normal law. His faith was not great for he noted its limitations in 1895.[33] In 1900 the faith was gone.[34]

> It appears to me that, if the earlier writers on probability had not proceeded so entirely from the mathematical standpoint, but had endeavored first to classify experience in deviations from the average, and then to obtain some measure of the actual goodness of fit provided by the normal curve, that curve would never have obtained its present position in the theory of errors. Even today there are those who regard it as a sort of fetish; and while admitting it to be at fault

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[31] P. 286.

[32] *Proc. Roy. Soc.* 29, 1879, 365-367. The paper is followed by a discussion by D. McAlister, *ibid.*, 367-376 which gives the mathematics and presents some graphs of skew distributions that would result from the logarithmic scale of measurement.

[33] *Philos. Trans., loc. cit.*

[34] *Philos. Mag.,* 5th ser., 50, 1900, 173.

> as a means of generally describing the distribution of variation of a quantity x from its mean, assert that there must be some unknown quantity z of which x is an unknown function, and that z really obeys the normal law! This might be reasonable if there were but a few exceptions to this normal law of error; but the difficulty is to find even the few variables which obey it, and these few are not usually cited as illustrations by the writers on the subject.[35]

Pearson in developing a system of skew curves whereby he seeks to adjust theory to nature has performed great scientific service in combatting the superstition of the normal law.[36] The paper of 1894 suffered on account of its occasion, *viz.*, the problem of dimorphism in Weldon's crabs. But not all investigators have avoided initial error; and presently we must return again to Pearson.

These quotations from Galton and Pearson suggest a reason why the normal curve should be of exceptional occurrence. If "log stimulus" gives a normal curve, says Galton, then "stimulus" will not. If x does not give a normal curve, some persons, says Pearson, assert that z, of which x is a function, does follow the law. Suppose then that nature does conform to the normal law; we shall not in practice be fortunate enough to obtain normal curves of distribution unless we have chanced to use nature's own unit of measurement, or some unit directly proportional to it. Bertrand pointed this out long ago.[37]

> La règle des moyennes, il importe d'insister sur ce point, n'est ni démontrée ni exacte. S'il était admis que la moyenne entre plusieurs mesures fût toujours la valeur la plus probable, il en résulterait des contradictions. Quand on mesure une grandeur, on mesure, par cela même, toutes les fonctions de cete grandeur, son carré par exemple, ou le logarithme du nombre qui la représente. Pourquoi la valeur la plus probable du carré ne serait-elle pas la moyenne des valeurs obtenues pour le carré, et la valeur probable du logarithme, la moyenne des logarithmes? . . .
>
> Il ne faut pas, pour écarter l'objection, faire une distinction entre les grandeurs directement mesurées et celles qui résultent d'un calcul. Un mécanicien pourrait, bien aisément, annexer à une balance une aiguille marquant le carré ou le logarithme du poids. Ce carré ou ce logarithme deviendrait alors la grandeur mesurée. Le postulatum admis dans un cas devient donc impossible dans les autres.

A better example would be the distribution in size of the cubical crystals of common salt. In a sample of such crystals we might measure size by the height of the cube or by its weight. Since the latter measure should be approximately proportional to the cube of the former, it is plain that the normal law could not hold in both cases.[38] Now, if nature seeks to conform to the normal law, does she prefer to conform in lengths or in volumes or, perhaps, in some other more "natural" unit? At least, if man would seek the law of nature, he must have cogent reason for his choice of measures.[39] He can not (as the behavorist would agree!) tell what nature is doing unless he knows what nature is trying to do.

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[35] The remarks are occasioned by Merriman and the target-shooting. Merriman has subsumed these data under the "fetish"; but we have seen that it is not perfectly clear why Pearson in 1894 subsumed the same data by a two-fold application of the same "fetish."

[36] The early papers on "Skew variation in homogeneous material" are *Philos, Trans.* 186A, i, 1895, 343-414, and 197A, 1901, 443-459. See also "On the general theory of skew correlation and non-linear regression," *Drapers' Co. Research Memoir,* 1905.

[37] J. Bertrand, *Calcul des probabilités,* 1889, 180f.

We have been speaking of France and England, but the reaction against the "Gaussian dogma" was more explicit in Germany. In 1877 Lexis published a *Theorie der Massenerscheinungen*[40] in which frequency distributions were dealt with as normal, subnormal, or supernormal.[41] The emphasis was thus shifted from the normal distribution to normal deviations from the normal distribution.[42] The "Lexian ratio" measures this deviation.[43] This point of view has met with acceptance among German and Scandinavian actuarial statisticians, and forms, for example, the basis of Czuber's *Wahrscheinlichkeitsrechnung.*[44] We have already seen that von Kries in 1886 did not include among valid logical principles the law of insufficient reason. He was inclined to believe, however, that in many cases the "Gaussian law" was realized,[45] and he cites in support Bessel's observations, which Urban refers to as an example of asymmetry.[46]

Westergaard in 1890 sums up the empirical case.[47] He brings together the experiments on the drawing of balls and the like, and shows that with increasing number of cases the relative frequencies lie within a decreasing *Spielraum* and thus in a sense approach the expected frequencies. He then goes on to show that the frequencies of social statistics behave in a manner similar to the frequencies of the experiments on "chance." He is not so much interested in the applicability of the normal law as he is in the facts. The appeal to facts is, however, always a protest against theory which is given *a priori*.

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[38] It would be interesting to test this relation in the case of heights and weights of men. Height-and-weight tables are available in anthropometrical and somatological texts.

[39] We have a cogent reason, presumably, when, with the Fechner pendulum, we take as the unit, not the angle read on the scale, but the square of the sine of half that angle.

[40] Lexis, *op. cit.;* see especially 34ff.

[41] Fisher's translation of *unternormal* and *übernormal* as "subnormal" and "hypernormal" is due presumably to his otherwise excellent "dano-English," for which he so modestly apologizes: *op. cit.,* 125 and xv.

[42] If a pun is permissible, for Lexis' thesis is that the "normal curve" is not "normal" in the sense "usual."

[43] *V.* Fisher, *op. cit.,* 124ff.

[44] E. Czuber, *Wahrscheinlichkeitsrechnung,* 2 Aufl., 1910, 34-78.

[45] *Op. cit.,* 226ff.

[46] *Op. cit.,* 241. Much of the dispute as to whether the normal law is realized in fact, depends on just how much departure of fact from theory we allow without giving up the theory. Von Kries could call normal in 1886 what Urban would see as asymmetrical in 1910: standards have gotten more rigorous.

[47] *Op. cit.* 10-83.

After all the facts are the scientific business. We have traced what seemed to be a growing realization of the logical inadequacies of a theory, but have we perhaps not been witnessing a deepening appreciation of the facts? When anthropometric measurements were first being made, there stood out the fundamental fact of the massing of cases about the average, the rapid falling off of more extreme frequencies, and the extreme rarity of widely divergent cases. To this extent the normal law was the fact. But as interest centered upon the details, the inadequacies of so simple a generalization became apparent. There was effort enough at reconciliation, but, in general, science kept to the facts, and a more flexible system of representations came into use.

There is a pretty instance of the inadequacy of the normal law to scientific description in a research of Pearl's.[48] Pearl studied statistically the number of leaves in a whorl in Ceratophyllum. He found that the distributions for number of leaves were skewed one way at the proximal end of the main stem or of a branch, the other way at the distal end, and that the form of the distribution passed through symmetry somewhere between the two ends. Thus he writes:

> The phenomenon of skew variation stands forth in this case, free of doubtful interpretation through selection or any similar factor, clearly and definitely as a phenomenon of growth. In the face of facts of this kind it is difficult to understand how anyone can be so firmly convinced of the *Allgemeingültigkeit* of the normal or Gaussian law, as some biologists still are . . . Skewness in variation is a very real biological phenomenon, which may be changed and modified, not only in degree, but in direction, by various biological factors like growth, as, for example, in the present case.

In other words, if nature is aiming to make something follow the normal law, then, as the stem of Ceratophyllum grows, she changes continuously that something which is to fit the law. The real problem of scientific description is an account of nature's changing. To assume the normal law in such a case would be to shut one's eyes to the fact.

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[48] R. Pearl, *Variation and differentiation in Ceratophyllum,* 1907; especially 90f.

Here we may leave the question of the *a priori* nature of the normal law. There is, after all, no magic in it. It gives us back always what we put into it. If we know from experience what nature is up to, as we do with the coin, then we can proceed upon cogent reasons to apply the law and we get results. If we do not know, we must appeal to nature and see. We have then no reason to expect that we are going to find the law before the appeal is made, whereas we have considerable reason to expect not to find it, since the form of distribution depends on the unit of measurement and we have arbitrarily chosen one of a possible infinite number of units. When we do go to nature we find all degrees of resemblance to the law and divergence from it, and we may even find that the degree of divergence from the normal becomes the significant fact of our observation.[49]

3. THE RELATION BETWEEN THE UNIT OF MEASUREMENT AND THE FORM OF DISTRIBUTION

We have seen that if we alter the unit of measurement we alter the form of the frequency-distribution, and that if the change is in accordance with any function other than direct proportionality we get alterations of the skewness of the curve. Bertrand made the point with especial clearness, and it is strange that a relation so obvious should have received so little attention.

Williams recently ran upon the difficulty when experimentally he obtained psychometric functions for memory.[50] He expressed the frequencies for recall of a material as a function of the number of repetitions of the material, but he felt, for certain cogent reasons that the student of memory will appreciate, that the repetition was not a satisfactory unit in terms of which to work. The psychophysical effect of an early repetition is presumably greater than that of a late one and Williams would have liked a more truly mnemonic unit. If he could have taken the normal curve *a priori* as the necessary function, he could have adjusted his results to it and have discovered a mnemonic unit that was some particular function of the number of repetitions. Such a relationship would have provided a knowledge of the effectiveness of successive repetitions and would have produced a method for use with other mnemonic measures. All measures eventually could have been interrelated, and a great advance made in the psychophysics of memory. Unfortunately, however, there was no justification for assuming the normal law, and, with both law and psychophysical unit unknown, Williams, as he realized, was placed in the dilemma of being able to reason from neither to the other. The way out, of course, was to give up the impossible, and to stick to the observed facts, leaving the frequencies to stand as a function of the number of repetitions.

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[49] We have noted that the universality of the normal law was contested by the following persons: Bravais, 1845; Quetelet (admitted Bravais' argument), 1846; Lexis, 1877; Galton, 1879; McAlister, 1879; Bertrand, 1889; Pearson, 1895 and emphatically in 1900; Pearl, 1904; Urban, 1910; Edgeworth, limitedly in the *Britannica*. The list is by no means complete. We may add: R. L. Ellis, *Philos Mag.*, 3 ser., 1850, 321-328; Jevons, *op. cit.*, 1883, 374ff.; Venn, *Nature*, 36, 1887, 411ff. and *Logic of chance*, 1888, 23-52, 501ff.; Fisher, *op. cit.*, 1917, 149. Urban, *op. cit.*, 241, mentions Helmert, Laurent, Bessel, Guarducci, and Edgeworth as demonstrating asymmetry of distribution. Fisher, *loc. cit.*, refers to Oppermann, Cournot, and Dormay (especially Dormay) as showing the inadequacy of the Gaussian dogma. Urban's serious consideration of Pierce's data as showing symmetry is hard to understand for Pierce's symmetry was presumably assumed in the first place. His beautiful curves must have been fitted, for all he says of them, by eye, and when one attempts to treat his data statistically they do not check. *E. g.*, the numbers of cases for days 20 to 24 are respectively 495, 494, 502, 499, 498, and 497. Each should be 500. C. S. Pierce, *Report of U. S. Coast Survey*, 1870, 212ff.

[50] H. D. Williams, *Amer. J. Psychol.* 29, 1918, 219-226. See also Boring, *Psychol. Bull.* 15, 1918, 32f.

Trabue, in attempting to establish a language-scale " with equal distances between steps," ran into the same difficulty.[51] His material,— sentences to be completed by the Ebbinghaus method,—had no quantitative aspect except the number of arbitrarily chosen completion-tasks that could be successfully accomplished. He chose, therefore, to assume the normal law and to calibrate his material by reference to it, taking the probable error as the unit.[52] His hint of a cogent reason back of the normal law stands quite unsupported.[53] Now the language-scales presumably do " work;" but if they do, it must be for the reason that this form of measurement, pretending to nothing more accurate than Quetelet achieved, counts his degree of rigor successful.

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[51] See foot-note above, p. 12.

[52] Pp. 30ff. There is a further, somewhat obscured assumption, *viz.*, that the P. E. for one age equals that for another: see pp. 54ff. That the dispersion would not alter with age would hardly be expected; *cf.* J. B. Miner, *Deficiency and delinquency*, 1918, 252ff. At any rate the assumption is gratuitous, unless, as is always true, we argue that the unit is the premise in the case, and that premises are indisputable. The law of insufficient reason is usually applied to personal predilection.

[53] A foot-note on p. 31 says of the normal law: " If this assumption is made the results that follow are in . . . close accord with known facts; " but there is nothing more.

These two papers are but examples of how the problem of the unit arises in very different fields of mental measurement and how their solution is balked by an inability to assume in advance some distributive function like the normal law.[54] In such cases the limitation is pretty clear, for we have to make an assumption and it takes but little argument to show that an assumption in itself can not be fact.

There is, however, a more subtle way in which the will to believe in the normal law gets in its work. Suppose we give up assumption and go to experience, in order to study a special group of related phenomena; and suppose we find that very many of these phenomena, as we measure them, are distributed every one in accordance with the normal law? May we not conclude by analogy that the same law applies to other phenomena? If a penny comes heads and tails equally often, will not a silver dollar? If stature follows the normal law, what about length of forearm?

The answer is that it all depends on how much one knows of the nature of the case. One may reason from the penny to the dollar, if one knows — inductively, from experience — that homogeneity and shape in the penny are the attributes that condition the law, whereas size, chemical constitution, color, *etc.*, are irrelevant. (Even so one may make a mistake; a magnetized steel disk in a magnetic field might give unexpected frequencies.) To reason from stature to length of forearm, however, requires that we know that the size of the forearm is conditioned in the same way as is the total length of the body. It is not enough to know that both forearm and stature depend on the same factors; we must know that they depend upon them in the same way, that is to say, we must know that we are dealing with the same scale of growth. If the forearm were to vary as the square of x and stature as the cube of x, then prediction that the distribution of the one would be like the distribution of the other would be doomed to disappointment. With the coins, we know that the scales are comparable; each is expressed in percentage of heads. But in biological and psychological phenomena we can scarcely hope for any such certain analysis of the essential conditions.

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[54] There is no need to assemble a list of studies where the error in question is committed. Often it is present only by implication. Pearson, for example, in his study of "The relationship of intelligence to size and shape of head, and to other physical and mental characters," *Biometrika* 5, 1906, 105-146, assumes the normal curve in order to provide a scale on which his results may be plotted. He is careful to point out that the assumption is merely for convenience and that it does not affect the chief business of the paper, *viz.*, the computation of correlation ratios (pp. 106f., 118). Very well. But then Pearson proceeds to set up a scale of units called "mentaces," with every unit 1/100 parts of a particular group that about equals the standard deviation. A complete scale of mentaces is suggested, ranging from —300 to +300. Offhand it looks like measurement, although the careful reader discovers that it is merely an illusion resulting from overindulgence in the normal law.

Miner's discussion, *op. cit.*, 254-276, is here to the point. Miner sees the problem of the unit (255f.) and combats the Gaussian superstition (267ff., 272-276). His strictures on Pearson (272ff.) are tolerant. He does not seem, however, fully to appreciate (in spite of pp. 255 and 273f.) that the problem of the unit and of the form of distribution are one and the same.

Apparently then sometimes we may generalize and sometimes not. Can we make out a list of the classes of phenomena to which it is safe to say that the law applies? At least we may try.[55] And we shall have to consider five classes.

1. In the first place there are the *Glückspiele,*— coin tossing, card drawing, dice throwing, roulette, and the persistent urn or bag with the red and white balls inside, that introduces all students to the theory of probabilities. In such cases the presumption for the validity of the law is stronger than anywhere else. It seems that the red and white balls ordinarily do come out of the bag about equally often if they exist in equal quantities inside. Westergaard in 10,000 drawings got 5,011 white balls. Yet Pearson found that Weldon's 26,306 throws of dice exhibited "a bias toward the higher points." He calculated that the chances of 5 or 6 coming up on these dice exactly one-third of the time were less than two in 100,000, but that the chances of 5 or 6 coming up .3377 of the time

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[55] What is needed is a comprehensive statistical study. If some one would get together only a hundred distributions based on large numbers (physical, biological, and mental distributions as found in nature, and distributions from such things as games of chance, tide tables, incommensurable numbers, *etc.*, as found in human civilization), and if he would treat them all accurately *by the same method,* applying some criterion of goodness of fit to the normal law (*cf.* Pearson, *Phil. Mag., op. cit.*), we should begin to have some notion of the way in which all these things do behave. But the inexperienced statistician should not begin lightly on such a task. He must be prepared to use much judgment before results taken under very different conditions are comparable. The very decision as to the size of the class-interval in every case is tremendously baffling; and there are questions of weighting that are equally bothersome. When this task is done he ought to fractionate and do it all over, in order to see how the principle of the *Spielraum* as dependent on the law of large numbers works out. And after that, he has left the study of the effect of changes in the scale of units. How does the goodness of fit alter when the logarithm or the square root of the measure is substituted for the measure, and how much does it alter and under what conditions does it alter most? It is a large order and one that is well worth filling.

were as great as two in 16. "Oh well," we say, "you could not expect any real dice to be exactly homogeneous. There is not much difference between .3377 and ⅓." No, and in any real experiment ideal conditions are never exactly realized. All experimental observation is only as accurate as the control of the conditions of observation.[56] The tendency with Pearson's method is to conclude from particular cases against a generality by a method more precise than the precision of the observed cases allows.[57] In general the *Glückspiele* seem to follow the law as well as the faulty conditions of their construction and operation permit.[58]

2. It is generally maintained that the occurrence of digits in the expansion of an incommensurable number like π. ϵ. $\sqrt{2}$, log 7, *etc.* follows the normal law, and that all digits in the long run occur equally often. Here the law of insufficient reason is peculiarly tempting. If there is no relation at all between the intrinsic properties of a circle and the number of fingers on the two human hands which determined the decimal system, then why should not all digits turn up equally often in the expansion of π? There is no reason why they *should* not. The *fact* is that in the first 707 digits of the decimal of π the sevens occur only three-fourths as often as they should, although the other digits do better by the law.[59] This case, however, is but a single one,[60] and, as Venn points out, the chances of one digit's deviating from equality as much as do

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[56] Of course we *want* the die to come up equally often on every face. When it does not, we would rather blame the die than the law. But, though such a gaming 'instinct' may be built into human nature, it is nevertheless an insufficient reason for science. To blame the die because it does not fit the law is unfair; but to say that the die because of practical conditions does not disprove the law or even that it does approximately prove it, is allowable.

[57] But this is a paper in itself.

[58] For summaries of results, see Westergaard, *op. cit.*, 21-38; Pearson, *Chances of Death*, 1897, i, 11-25, 44-62. The failure of Monte Carlo to obey the law of chance seems not to apply to all roulette; *v.* pp. 57f. Westergaard's experiment is in *op. cit.*, 36f.; Pearson's computations are in *Phil. Mag.*, 5 ser., 50, 1900, 167ff. See also Fisher, *op. cit.*, 127-145.

[59] The actual frequencies, which Venn who discusses them (see note below) does not give, are: 0, 73; 1, 78; 2, 72; 3, 74; 4, 74; 5, 63; 6, 68; 7, 53; 8, 73; and 9, 79. The expansion of   is by W. Shanks, *Proc. Roy. Soc.* 21, 1873, 319.

[60] There is no reason why a division of a decade by a trinity should be prepotent of other trinities, that 10 divided by 3 should be 3.33333 . . . (in the decimal system; the duodecimal system gives a completed two-digit quotient); nor is there any reason why the product of 12345679 by 27 should be 333333333, *except* the absolutely adequate reason that such is the inherent nature of the case. But in the realm of probabilities these exceptions help to prove the rule.

the sevens is one in four.[61] Edgeworth found that there was one chance in ten that the first 1,800 digits of the expansion of the fraction 1/1861 were following the law of chance.[62] Both he and Venn are convinced, however, that the rule of the coin is applicable to such series of digits, and indeed such is the presumption though the generality is here less well established than for games of chance.[63]

3. In 1904, long after the publication of his first papers on skew variation in homogeneous material, Pearson made the following statement:

> [Suppose] we assume a certain distribution of frequency for the character in human populations. This distribution of frequency is given by the Gauss-Laplacian normal curve of deviations from the mean. . . . Now the problem before us is the following one:—Is this assumption legitimate? It certainly is not true for organs and characters in *all* types of life. But it really does describe in a most remarkable manner the distribution of most characters in mankind . . . I should be the last to assert that no human characters can be found that do not diverge sensibly from this Gaussian distribution. But I believe they are few, and that for practical purposes we may with nearly absolute safety assume it as a first approximation to the actual state of affairs.[64]

Everything hinges here on the word "approximation." The case is very different from that of the coin or of the incommensurable number. We do not know anything about the units with which we are working except that they are the units with which we are working. When we change from the penny to the dollar we keep the same scale, one of proportions of heads in a two-fold universe; when we change from π to $\sqrt{2}$ we also keep the same scale, one of proportions of digits in a ten-fold universe, but when we change from stature to the forearm we have no guarantee that the inch means the same thing biologically. And we must use a biological unit, if we are to bind our class together by a biological concept, if we are to predicate the normal law of all *human* characters. That the form of distribution is a function of the unit is unescapable. What we need to know, and what we do not know, is whether the differences that would occur with biologically reasonable variation of the biological unit from the physical unit are small enough to be included within the "approximation." A certain amount of alteration of the scale of measurement still leaves a sensibly normal curve. Have we any cogent grounds for assuming "with nearly absolute safety" that "the actual state of affairs" is not enough differently conditioned from those that we know about as to render it widely divergent from the normal law? This is a reasonable question, and within approximate limits it may be answerable. Certainly we may not generalize with Pearson on any other basis, and certainly Pearson neglects to provide this basis.[65]

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[61] Venn, *op. cit.*, 111-118, 247f.

[62] Edgeworth, *Cam. Philos. Trans.* 20, 1905, 128-130.

[63] See also S. Newcomb, "Note on the frequency of use of the different digits in natural numbers," *Amer. J. Math.* 4, 1881, 39f.

[64] *Biometrika* 3, 1904, 142f. Many curves are given which *look* like good fits. The measures of goodness of fit obtained by Pearson and A. Lee, *ibid.* 2, 1903, 357-462, show great variation. There are equal odds that the statures of "mothers" follow the law, and only a few chances in a thousand that the forearms of "fathers," "sons," and "daughters" do. Pearson seems here, as we should like him to do elsewhere, to take his own criterion with a grain of salt.

4. On such an insecure structure does Pearson attempt to build further. If the physical is normal, why not the mental?

> I put the problem to myself as follows:—Assume the fundamental laws of distribution which we know to hold for the physical characters in man, and see whither they lead us when applied to the psychical characteristics. They must: (a) Give us totally discordant results. If so, we shall conclude that these laws have no application to the mental and moral attributes. Or, (b) Give us accordant results. If so, we may go a stage further and ask how these results compare with those for the inheritance of physical characters.[66]

(The paper is on inheritance.) The assumption of the normal law gives results for the mental characteristics that look like the results for physical characters. Hence we may go ahead with our correlations in each case to indicate the degree of inheritance. The assumption accomplishes little beyond providing the scale for the printed graphs of intelligence; but that was in 1904.

In 1914 we find Pearson still arguing for a normal distribution of intelligence, and the occasions for his argument are certain empirical results which did not show a good agreement with the "Gaussian" law.[67] He is presenting the results of the application of the Jaederholm form of the Binet scale to school children of Stockholm. The samples are very small (261 normal and 301 defective children). There is one chance in 60 that the normal children give but a random deviation from a properly "Gaussian" distribution; there is one chance in 20 that the normal children plus an ideal fraction of the defective children are properly distributed in the "Gaussian" manner. Eight-year old children taken alone, however, do show an even chance for the applicability of the "Gaussian" law. This is slim evidence, but it appears that it is not yet time to declare the Gaussian image a false god:

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[65] He is, in fact, not clear on the part played by the unit of measurement. He is concerned much over the normal law and but little over the arbitrary scale of measurement that determines in advance the distribution.

[66] *Biometrika* 3, 1904, 147. We have seen that Pearson also assumes the "Gaussian" curve for mental ability in *ibid.* 5, 1906, 105ff.

[67] Pearson and G. A. Jaederholm, *Mendelism and the problem of mental defect, II: On the continuity of mental defect,* 1914; Pearson, *Mendelism and the problem of mental defect, III: On the graduated character of mental defect, etc.,* 1914. These papers are viii and ix in *Questions of the day and fray.* On adjustment to the normal curve see II, 28-36, 46f.

> There is absolutely no reason why the Gaussian curve should be dogmatically asserted to apply to frequency-distributions of intelligence . . . Still there are *a priori* suggestions that it should be tried. In the first place it does describe with a great degree of accuracy most physical measurements in man, and secondly, the Biometric school has found that it gives good results for many measures of intelligence. It is the view of the psychological joint-author of this paper that its comparative failure as applied to the present data lies rather in faults of the tests applied or in the method of applying them than in the non-Gaussian character of intelligence when adequately measured.[68]

This quotation is as near a confession of what the Biometric School is trying to do as we are likely to get. It is frankly seeking always to see Gauss in nature. Its excuse in the case of mental phenomena for a prejudice in favor of this one of an infinity of possible distributions is that the Gaussian curve "does describe most physical measurements in man," and that "it gives good results for many measures of intelligence." The physical agreements, however, are approximate, as we have seen, and the facts with respect to intelligence are questioned.[69] The real difficulty comes, however, in the grounds for analogy. The implication is that we may reason from the physical to the mental because, in this case, both are "human," and that we may reason from one test to another because both are tests "of intelligence." Now we have no distributions of 'humanness,' but only of measurements of human characters; and we have no distributions of intelligence, but only of particular measurements of intelligence.[70] Moreover, in every case the measure is arbitrarily chosen; and it is not even strictly a measure, in the sense of being the sum of equal units, unless we make it so on *a priori* grounds. In an intelligence-test, for example, we have no notion whether the difference between 99 and 100 points is the same difference in intelligence as the difference between 9 and 10 points, unless we are ready to define intelligence arbitrarily as the ability to achieve points in that particular test.[71] Now if we define intelligence with respect to one test, we can not assert *a priori* that any other test is a measure of intelligence, and should therefore show the same distribution. *A posteriori* we may show by correlation that a second test measures an ability similar to intelligence as defined by the first test, but in such a process we demonstrate empirically the similarity of distribution. There is no valid reason for expecting normality in one intelligence-test because it has been found in another; and the same reasoning applies to the argument from the physical to the mental.[72]

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[68] *Opp. citt., Mendelism, etc., II,* 46.

[69] Miner, *op. cit.*, 267-279, reviews the evidence for a normal distribution of tested intelligence, and concludes: "In spite of these arguments and the evidence of asymmetry of measurements at least of some periods of life it is to be noted that current opinion is probably contrary to this hyposthsis [hypothesis, the normal law], although, as I believe, because it has been mainly concerned with those who are not of extreme ability:" p. 275.

[70] *Cf.* here Miner's distinction between "physical units" and "more equivalent biological units," *op. cit.*, 255. We have noted that Miner does not take advantage of the full force of the distinction; the physical unit may be irrelevant to the problem in hand.

We are very far, then from a general conclusion that intelligence, a mental capacity comprehensible apart from the particular instrument by which it is tested, is normally distributed. Still farther are we from stating that mental abilities, whatever their nature, follow the normal law.

5. We have to investigate one more class of phenomena, the psychophysical judgments. In this case no claim for generality has been presented, but the trend of the facts bespeaks attention. Urban found that the phi-function of gamma is a good hypothesis for the psychometric function in lifted

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[71] No one who has ever "made up" an intelligence test ought to have any difficulty in appreciating the arbitrariness of the calibration. The tasks of the test are in the first place sheer casual invention. They are put into a scale and tried out. Alterations are made to "improve" the frequencies of success in different component parts, but no one can work accurately from the frequencies to the scale without presupposing the form of distribution. There is never any attention paid to making equal increments of intelligence measured by equal increments of the scale, for the excellent reason that no one has any such definite quantitative conception of intelligence. To call a year in mental age a unit is a fallacy of this sort. See Pearson, *Mendelism, etc.*, II, 36*ff.*; Miner, *op. cit.*, 260ff. There is no evidence (nor are the physical growth curves in any way relevant) that annual increments *of intelligence* are equal in any other sense except in that they are annual. Of course a year is a year, if "annual-ness" is what we are after.

[72] Pearson has appointed himself "watch-dog" of "logic in scientific procedure", *Mendelism, etc.*, III, 3f.; but he seems to need to make friends with one sophistical wolf ("an appearance of knowledge where we are as yet in a state of ignorance") in order to attack another.

weights and in certain acoumetric experiments.[73] Now the phi-function of gamma is simply the integral form of the normal curve. To say that the phi-function of gamma is the psychometric function is equivalent to saying that the dispositional variations of the psychophysical organism, when measured in a scale of units proportional to the scale in which the stimulus is measured, follow the normal law. If the lifted weights are measured in grams and the psychometric function approximates the phi-function of gamma, then the organism varies in the amount of its disposition for judging " heavier " or " lighter " according to normal law, provided always that the amount of this disposition is measured in grams. This relationship is scarcely obvious and the reader may need to read elsewhere,[74] before he can accept it. The essential thing to understand is that the applicability of the phi-function of gamma means that the organism is varying somehow in accordance with the normal law, but that the normality of its variation is as much a function of the unit in which the variation is measured as it is in any of the other cases which we have considered.

Now can we generalize? Can we ever say that all psychophysical judgments tend to obey the normal law? Urban makes no such attempt; rather is he against theorising and for description. " The nature of the dependence," he writes, " is not known and cannot possibly be deduced by any considerations *a priori*;"[75] and he refers us for the function to " the results of observation." This, to the scientist, is a refreshing return to facts from the realm of logic and mathematics; but is not final. Something can be said for generalization.

Unfortunately for ease of exposition, there are two kinds of psychophysics. In the one kind stimuli are presented and the observer reports upon the sense-impressions. In the case of the stimulus-limen, he simply reports sensation or its absence. He is making a brief introspection. In the case of the differential limen he makes a judgment upon the relative degrees of the two sensations, but the judgment is actually little more than a comparative description; the observer may be said to be introspecting here too.[76] In the other kind of psychophysics the observer's attention is directed upon the stimulus and not upon his own mental processes. In no sense does he introspect. Always he is making judgments of the stimulus. In introspective psychology this attitude is called the "stimulus error," but in psychophysics it may be in place. Urban's experiments are of this kind,[77] as has been much other psychophysical work. There is reason to believe, however, that psychology will profit more in the long run by work performed under the first attitude described.

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[73] On lifted weights: Urban, *op. cit.*, 257ff.; *Arch. f. d. ges. Psychol.* 16, 1909, 168-227, espec. 224f. On acoumetry, H. Keller's experiments, *Psychol. Stud.* 3, 1907, 49-89; and Urban's discussion of them, *Arch. f. d. ges. Psychol.* 18, 1910, 400-410. Keller used Wundt's fall phonometer, and the all-important unit of stimulus, which is only just mentioned, is the cm. The psychometric functions of the lifted weights are for grams.

[74] Boring, *Amer. J. Psychol.* 28, 1917, 465ff.

[75] *Psychol. Rev.* 17, 247.

In the case of the "introspective" psychophysics we are not to expect a generalization, because everything depends on the unit. Were the psychometric function to approximate the phi-function of gamma ten times, there would be no reason to expect it the eleventh. Suppose the eleventh case were the differential limen for auditory intensity with the Fechner sound pendulum as the instrument. Will the normal law hold? Perhaps when we use as the unit $\sin^2 (\Theta/2)$. If so, it will not hold if we use Θ as the unit. If it holds for neither, it may hold for some other function of Θ. The reports upon sensations depend upon Θ and the momentary disposition of the organism, but in no way upon the scale of units chosen. It is mere luck if the normal law applies. But whatever luck we get is good luck, for the psychometric function gives us the law of the dependence of dispositional variation upon the scale of measurement which we have chosen. A little knowledge of sensory mechanisms is worth the loss of a generality.

When the psychophysical judgments are directed upon the stimulus it may be that we can generalize, for now the scale and the judgment are related. With the sound pendulum we might be asked to judge the height of the fall, which is measured by $\sin^2(\Theta/2)$, or we might be asked to judge the angle. In the two cases we would be judging entirely different things, and it is quite possible that the normal law might apply in each. If we found that it applied in a large number of cases, then we might generalize by saying that errors in the judgment of a stimulus, that is to say, errors of observation, are distributed normally. This conclusion, indeed, is Urban's: "We thus obtain the remarkable result that the foundations of the theory of the errors of observation are found in the theory of psychophysical measurement."[78] In other words we would demonstrate psychophysically Gauss' original assumption that positive and negative errors are equally likely.

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[76] *Cf.* the nature of the constant psychophysical attitude set up in S. S. George's experiment, *Amer. J. Psychol.* 28, 1917, 1-37.

[77] In the discussion of Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.* 30, 1913, 113-152 (*cf.* also, *e. g.*, *Amer. J. Psychol.* 24, 1913, 274), Urban would appear to maintain that psychophysics deals with describable mental processes; but the implications in the accounts of his experiments, the lack of a reproduction of definite instructions given the observers, and the failure to discuss the introspective competence of the observers, all indicate that the observers were, or at least may have been, making judgments of stimulus rather than of mental process. See *The Application of Statistical Methods to the Problems of Psychophysics,* 1908, 5, 14; *Arch. f. d. ges. Psychol.* 15, 1909, 264f. In *Psychol. Rev.* 17, 229, he speaks of "the judgments of a subject who compares two stimuli." And indeed in the systematic schema of *Arch.* 30 (*op. cit.*) he takes account (1) of mental contents and processes, (2) of physiological conditions and processes, and (3) of physical conditions and processes, but leaves no place for meanings, awarenesses, the stimulus in relation to the subject. It is doubtful if Urban sought at all to distinguish between *Beschreibung* and *Kundgabe* in the psychical aspect of the psychophysical system.

If there is any general answer to the question: When can we generalize?— it is this. We may expect the normal law to hold within an entire class of phenomena, when it holds for a number of cases within the class, provided always that all variations of members of the class are capable of being expressed as variations of some common denominator. In other words, we must be able to see the same thing varying in different situations. With the penny and the dollar, the law holds for the turning up of heads. The things that make the penny different from the dollar, we know to be irrelevant. In lifted weights and acoumetry we may know, by a control of attitude, that we are studying, not kinaesthesis and auditory sensation, but errors of observation. If Urban did know this, then he was supplied with the only possible ground for expecting the one result on the basis of the other.

4. The Logical Possibilities
(Conclusion)

At last we are ready to take account of stock! There are four logical possibilities, but the circumstances of the case are such that it appears unlikely that the actual mental measurement can make much use of more than one. Let us see.

1. In the first place, there is the possibility of determining *a priori* the form of distribution. This course, applied to the normal law, is popular, but it is usually without sanction. In quantitative psychology we should resist it. We may not assume the normal law in an interpretative study except upon

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[78] *Psychol. Rev.* 17, 243; but Urban is being betrayed into generalizing. His statement is based only on the lifted weights and perhaps the acoumetric experiments. The validity of generalization under this attitude is hardly to be demonstrated so easily.

valid grounds. We have seen that the law of insufficient reason furnishes no such grounds. Knowledge simply does not come out of ignorance. The only scientific grounds that are presumptive of the normal law are an intimate knowledge of the constitution of the particular case and of the function that frequencies of occurrence are of that constitution. Such a function comes in science from experience, and, whenever it is thoroughly understood, from experiment. On such cogent grounds we may conclude *a priori* to the normal law or some other form of distribution, and we are following such a logical process in the few cases where we find it legitimate to generalize about the form of distribution for a given class of phenomena.

Two corollaries follow.

(a) If we knew that the normal law had to hold in a particular case, then we could experiment with an arbitrarily chosen unit and determine, by working from the law, the function that the "true" psychological unit, which is following the law, is of the arbitrary unit of the experiment. Any such hope, however, is bound to be illusory, for a knowledge of the constitution of the case involves *a priori* the knowledge of this unit. We may say that, if the dollar is like the penny, we shall get 50% heads; but we can not reverse the argument and say that, because we get 50% heads, the dollar is like the penny.[79]

(b) There is at present very little prospect in the field of mental measurement that our knowledge of the psychological constitution of a mental function or process will be sufficient to enable us to begin work upon a problem with the assurance that the resulting distributions must be normal. There is a bare hint of an exception, that awaits verification, in the psychometric functions of errors of observation. But the evidence indicates that instances, where there are cogent reasons for assuming the normal law in advance of empirical determination, will be extremely rare for a long time to come.

2. We may begin in mental measurement with the psychological unit, for if we can not determine the unit from the distribution, we may, nevertheless, determine the distribution from the unit. Such, in fact, is the necessary scientific order.

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[79] *I. e.*, in physical constitution. We are not denying inverse probability, but insisting merely that a statement of physical constitution is more complex than a statement of frequency and that the same frequency may be variously conditioned. If we know only that we get 50% heads, we can not reason that the underlying physical constitution is that of the dollar or the penny. For all we can tell we may be dealing with an asymmetrical loaded die, with one head and five tails, that belongs with the penny only in the matter of frequency of heads and not in general physical constitution.

The great difficulty is, as we have just pointed out, to find anything that we may properly call a psychological unit. The sense-distance is such a unit. It is a unit of measurement that is mental *per se*.[80] Most of the sensory work in psychology, however, has had to do with the determination of limens and has not made use of the mental unit at hand. The extensive psychophysics of memory has not achieved a psychological unit, nor have the other departments of the psychology of process. In the psychology of capacity or function the case is worse. For example, with intelligence, which is the mental capacity most often measured, we have seen that, not only is there no attempt to make equal increments on an intelligence scale correspond to equal increments of intelligence, but that the concept of intelligence is so vague that any such accurate quantitative relationship is in practice almost meaningless. We are not, however, at our rope's end.

3. If a psychological unit is not to be achieved we may use a "physical" unit, that is to say, some arbitrarily chosen aspect of an arbitrary scale of measurement. Such units are the year (mental age), the second (mental tests where time is the measure), the item or task (where the number of points is the number of unit tasks completed, *e. g.*, the U. S. Army intelligence tests), the gram (lifted weights), the syllable (memory experiments), and so on. Every such unit is arbitrary. There is no evidence that equal increments of its scale correspond to equal increments of the psychological entity measured. When we define intelligence, in order to gain definiteness of conception, as ability in some particular test, we are simply substituting a "physical" unit for the ideal, but unachievable, psychological unit. Now the application of a unit that is not psychological to a quantity that is psychological does not yield a measure of the quantity. It will place the quantity in a given position upon the arbitrary scale, and will determine the rank-orders of a number of quantities so placed, but the assumption that rank-orders tell the amount of a given quantity, or, if the zero be unknown, of a given increment, is, of course, unwarranted.

There is one case, of frequent occurrence in the behaviorism of mental tests, where the "physical" and psychological units seem to become identical. Suppose we are studying the learning of typewriting and express our results in the number of words written per minute. Do not words-per-minute truly measure ability in typewriting? Here we are involved in a question of point of view. Words-per-minute measure the product of typewriting. They may perhaps be said to measure ability in typewriting from the point of view of the employer of a stenographic force; they are the ability of his office force over against a given job. A behavioristic psychology that identified behavior with mere product, without any reference at all to the conditions of the product, might take the point of view of the employer; but such a psychology would achieve only a physics, or more likely a common sense, of typewriting. Any definition of behavior as *response to a situation*[81] brings in the behaving organism and changes the ground. We can not say that words-per-minute truly measure the behavior of the psychophysical organism or its response to the situation. Does an increment of ten words per minute mean the same change in ability or in behavior or in response when added to an ability of 50 words per minute as when added to an ability of 100 words per minute? To the employer, yes; but not to the psychologist, or, presumably, to the organism. It is indeed an heroic measure, when we can not make the unit psychological, to attempt to a-psychologize psychology.

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[80] *Cf.* E. B. Titchener, *Experimental psychology, Quantitative student's manual*, 1905, xix-xxxvii. The thing is so simple when it is seen! In physical science the nature of the unit is given in the presuppositions of the science, and in psychology it is given mentally in the sense-distance. It is the indefiniteness of the mental function that makes trouble and gives rise to the attempt to create a unit in retrospect by an appeal to the hypothesis of the normal law. Since this paper deals with the appeal to the normal law, we may, however, pay homage to the sense-distance and proceed.

4. We are left then with the rank-orders of our psychological quantities, given by reference to a fixed but arbitrary extra-psychological scale; and it is with these rank-orders that we must deal. We are not yet ready for much psychological measurement in the strict sense. It is true that psychophysics may yield a unit and also true that ordinarily it does not. The sense-distance has had only a limited use, and we do not yet know that the stimulus-distance (for it is upon stimulus-distances that the psychometric functions of errors of observation are founded) constitutes a true unit in a behavioristic psychophysics. The case, however, is not so bad as it seems at first. There is nothing new in the contention that mental measurement is impossible,[82] whereas now we do gain the assurance that rank-orders at least are validly demonstrable. And there

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[81] *Cf.* the point of view of behavior presented by E. B. Holt, *The Freudian wish*, 1916, espec. 153ff. Behavior is *what* the individual is *doing*, not that he is moving. We can perhaps measure movement, but we have yet to conceive the unit for "doings."

[82] Indeed, in confining ourselves to a consideration of the part played by the normal law, we are leaving a famous controversy untouched. *Cf.* O. Klemm, *A history of psychology*, trans. 1914, 150-155, 232-267.

is a great deal that can be done with rank-orders. We can deal with frequencies, medians, and quartiles. For example, it is considerable to know of two groups that the lower quartile of the ranks in one overlaps and is practically coincident with the upper quartile of the other. What we must remember, however, is that we are dealing with the statistics of medians, quartiles, contingencies, and correlation ratios; not with the statistics of averages, standard deviations, coefficients of correlation, and linear regressions. All those statistical constants, that imply a scale of equivalent units, violate in use the conditions of the case and lead to a precision of result that is an artifact. The serial constants, that do not presuppose a unit, yield less intricate resultants, but they present a rougher picture that represents truly the rough material which they describe.[83]

The initial error in the application of the theory of probabilities was the assumption of the law of insufficient reason. It was wrongly supposed that knowledge could somehow be wrought out of ignorance. This very error, however, has never been routed. It has gone on, multiplying mischief. The substitute for insufficient reason is cogent reason. The more we know of the intimate nature of the entity with which we are dealing the more accurate and complete can our descriptions become. But, if in psychology we must deal—and it seems we must—with abilities, capacities, dispositions and tendencies, the nature of which we can not accurately define, then it is senseless to seek in the logical process of mathematical elaboration a psychologically significant precision that was not present in the psychological setting of the problem. Just as ignorance will not breed knowledge, so inaccuracy of definition will never yield precision of result.

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[83] *Cf.* C. J. West, *Introduction to mathematical statistics,* 1918, 94.

FOVEAL ADAPTATION TO COLOR[1]

By Hubert Sheppard

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Introduction

The primary aim of this study is to describe the course of foveal adaptation to color. We have also obtained records of the times of adaptation, and have attempted to show their variation with variations of intensity and chroma; but our main purpose throughout the experiment has been to secure introspective reports of the attributive changes which occur while the fovea or macula lutea is under stimulation by a color, from the moment of exhibition until the color has disappeared or until there is no further alteration of hue or tint or chroma. The peripheral retina is discussed only in so far as its discussion bears upon adaptation at the fovea.

Wherever possible, all earlier experiments which seemed to relate, directly or indirectly, to our problem were repeated, and their results tested, before our own technique was finally decided upon. Very little work has been done, however, that bears directly upon foveal adaptation considered as a mode of experience. Adaptation in general depends (1) upon wave-length, wave-form, and wave-amplitude of light, and (2) upon the time during which the light-waves affect the eye. Hence the problem may be approached from the side of light-dark adaptation, of color adaptation, or of negative after-image. Previous investigators have studied the first and third of these topics rather than the experienced course of color adaptation.

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[1] From the Psychological Laboratory of Cornell University.

The term *adaptation*, as applied to visual experiences, was first used by Aubert, to denote "the accommodation of the eye to light-intensities."[2] Nowhere in his writings does Aubert connect this term with color-changes, for which he uses the term 'fatigue.' In his paper on fatigue and retinal after-image,[3] as elsewhere, he argues that there is a close analogy of function between the fovea and the peripheral retina. Colored objects, he says, appear colorless in both direct and indirect vision, if the stimulus is sufficiently long continued. He is, therefore, familiar with complete adaptation, but includes it under the term 'fatigue.'

Exner, working under the direction of Helmholtz, made the first systematic study of adaptation (fatigue) to color-stimulation.[4] In an experiment in which he describes the course of the after-image, he finds that three parts of the spectrum are much less altered in appearance by 'fatigue' than other parts, namely, *R* from the end of the spectrum to a point between the lines C and D, *G* between E and b, and *B* near G. He notes also that, if the after-image of a color is projected upon another color of the same hue, this second color appears much less saturated.[5]

Von Kries investigated the problem of adaptation from both the physical and the psychological points of view. He found that "with long fixation, a color loses in brightness as well as in saturation."[6] suffer no change in hue, while the other colors change in the direction of *Y* or *B* from *G*.

Hess made a study of the tone-changes of spectral colors when the retina was fatigued with homogeneous light.[7] He observed that all colors possess either a *Y* or a *B* valence. That is, all colors, from near a pure *R* to a pure *Y*, or near a pure *G* to a pure *Y*, will change under adaptation towards *Y*; and all colors from near a pure *R* to a pure *B*, or near a pure *G* to a pure *B*, will change towards *B*. Further, during the process of adaptation the stimulus-color always becomes less and less saturated, until it disappears; the complement of the stimulus-color always remains unchanged in hue; and the appearance of a spectral color after adaptation corresponds in general to a mixture of that color with its complement.

Burch was able to produce temporary color-blindness by means of general adaptation at any intensity.[8] In an article entitled "On the Production of Artificial Colour-blindness to Successive Contrast" he describes the process of adaptation as complete for *R*, *G*, *B* and *V*. In another experiment he was able to get complete adaptation in 3 min. when the intensity of the stimulating light was bright moonlight. His method for very high intensities was as follows: The eye of an observer was exposed to bright sunlight. which had been focussed by a burning-glass through a colored screen. After the eye had been subjected to this intense light until all sensation of the particular color was lost, the observer proved to be temporarily color-blind to the color used in stimulation. Further, four parts of the spectrum, *R* from A to B, *G* in the neighborhood of E, *B* about half-way between F and G, and *V* at and beyond H, showed marked fatigue-effects, differing in degree but not in kind. All direct sensation of the stimulus-color was temporarily lost: *R* for 10 min., *G* for a still longer period, while the recovery from *V*-blindness might require a couple of hours. Whenever the eye is completely fatigued to any color, that color is absent from the spectrum, whatever the fatiguing intensity may have been. The observer is, in fact, color-blind for the particular color. After fatiguing the eye with three of the four colors, Burch found that only one color remained.[9]

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[2] H. Aubert, *Physiol. d. Netzhaut*, 1865, 39 ff.

[3] Moleschott's *Untersuch.*, IV, 1858, 220 ff.

[4] J. Aitken, *Roy. Scot. Soc. of Arts, Proc.* 1871-2, and E. Hunt, *Colour Vision*, Glasgow, 1892, made similar studies of adaptation. A detailed account of their method and procedure is given later by Burch.

[5] S. Exner, 'Ueber einige neue subjective Gesichtserscheinungen,' *Arch. f. Physiol.*, I, 1868, 375-394.

[6] Nagel's *Handb. d. Physiol. d. Mensch.*, III, 1905, 213-220.

Further, *Y* of a wave-length of 560 μμ, *G* of 500 μμ, and *B* of 460 μμ

[7] C. Hess, *Arch. f. Ophth.*, 1890, XXXVI, Abtl. 1, 1 ff.

[8] G. J. Burch, *Phil. Trans.*, 191 B, 1899, 1-135. *Roy. Soc. Proc.*, 66, 208 ff, 216-219.

Troland has unpublished data from which he is able to determine a theoretical course for the process of adaptation in all the adapting sense departments. Retinal adaptation follows the same general course as that of the other adapting organs.[10] The rate of the process is greater in the beginning, with a decrease rapid at first, then more gradual, until it finally reaches a level at which it is constant.[11] Troland finds also "that, at least for stimuli of fairly high intensity, adaptation does not proceed to a point at which the resultant sensation reduces to a neutral gray."[12]

In sum, then, Exner, von Kries, and Hess come to almost exact agreement with regard to the gross results of adaptation to color. (1) The 'primary' colors, *Y* 550-574$\mu\mu$, *G* 495-520 $\mu\mu$, *B* 435-471 $\mu\mu$, do not change in hue under the process of adaptation. Hess finds a psychological *R* outside of the solar spectrum which also does not change. All other colors change either towards *Y* or towards *B*. (2) A color, after adaptation, corresponds in general to a mixture of this color with its complement.

As regards adaptation to color at high intensity, there is difference of opinion. Burch gets complete general adaptation with any intensity which produces temporary color-blindness to the stimulus-color. Troland, on the other hand, finds that color-adaptation at high intensity does not reduce the color to a neutral gray.

Experimental: Plan of the Experiment

The present experimental work upon foveal adaptation to color was begun in October, 1918, and ended in September, 1919. The observers were Dr. C. W. Bock (B), Mr. H. Sheppard (Sh) and Mr. M. J. Zigler (Z), instructors in psychology, and Mr. H. Liddell (L), Miss A. K Sullivan (S), and Miss M. Finck (F), graduate students in the department.

L, S and Z were given several weeks' training before any data were taken for our records; F was relatively unpractised.

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[9] A. W. Porter and F W. Edridge-Green, *Proc. Roy. Soc.*, 85 B, 1912, 435 ff, find somewhat different results for *B* and *G;* but Burch, *Proc. Roy. Soc.*, 86 B, 1912-13, 117-18, argues that their work was not carefully done, and that their results are not strictly reliable.

[10] L. T. Troland, *Am. J. Psych.*, 25, 521 ff.

[11] *Ibid.*, 508.

[12] *Ibid.*, 520.

Our object, as has been stated above, was to describe the course of foveal adaptation to color under as many different conditions as possible. We do not attempt a theory, nor do we draw any theoretical conclusion whatever.[13] We aim only at a qualitative study of the experienced course of adaptation, with secondary reference to the times of adaptation for the different colors used.

The experiment as a whole comprises three parts. In Part I. we examine general adaptation to a surface color in direct sunlight. Here the experienced course of adaptation is described in its various stages. In this preliminary experiment we also take account of foveal adaptation in a general and tentative way. In Part II. we work with surface-colors, and the process of adaptation takes place under certain controlled conditions. These conditions greatly reduced eye movement, and kept the illumination constant. Here we describe the experienced course of foveal adaptation to 9 different colors, *R, O, Y, YG, G, BG, B, V,* and *C*. In addition we consider the effect of chroma upon adaptation. In Part III. pure spectral colors are used with various intensities of illumination. Here we describe the experienced course of foveal adaptation, and note in what respect this process differs from the course of adaptation when a surface-color is used. Difference of chroma is also considered. Two colors, differing in hue, are matched for chroma, and the process and time of adaptation are recorded in each case. We also give the results of some experiments made with high intensities of stimulus.

Part I.: Preliminary Experiment

GENERAL ADAPTATION IN DIRECT SUNLIGHT

We are here concerned to determine the course of general adaptation when a surface-color, illuminated by direct sunlight, stimulates the entire retina. In former experiments peripheral adaptation to color has been found to be more rapid than foveal.[14] Our main problem throughout the preliminary experiment was the study of the course of adaptation, by introspective observation, as the process travelled over the retina from periphery to fovea. If there is a rapid fading-out of a color under peripheral stimulation, as is indicated in the literature, it ought to be possible to observe this change in its progress towards the central retina.

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[13] Papers on 'An Experimental Treatment and Discussion of the Theory of Adaptation and Color Vision' as well as 'The Early Development of Color Vision' are now under consideration and will follow shortly.

[14] J. W. Baird, *Color Sensitivity of the Peripheral Retina,* 1905, 53-74.

Procedure.—A drawing-board of 28 x 36 in. was fastened in a window on the west side of the optics room. The board served as a table upon which to place the stimulus-color. The following papers were selected for stimuli in the preliminary tests:

Color	Brightness Value		Match to a Gray	Brightness Value of Match	
	Black	White		Black	White
R	288°	72°	211°	88°	272°
BG	206	154	149		
Y	144	216	170	154	106
B	263	97	190		
GY	162	198	125	112	248
V	285	75	235		
C	259	101	208		
G	250	110	132	97	263
B					
O	216	144	92		
G			120	112	248
B			148		

A sheet of colored paper, 19 x 24 in., was spread upon the table. In the center of the sheet was a very small light dot for a fixation-point. *O* sat facing the stimulus with one elbow resting upon the table and the chin in the palm of the hand. This position brought the eye within about 25 cm. of the fixation point, and was found to be convenient for fixation in monocular vision. All the experimental work for this part of the experiment was done during the months of July and August, from 2 to 4:30 o'clock in the afternoon. We were careful to select days when the sky was free from clouds or haze.[15]

The instruction to *O* was to report all change in color sensitivity, from the time the eye first fixated the stimulus until the color either faded out to a gray, showing no trace of color, or reached a stage where no further change in hue, tint, or chroma was perceptible. *O* was asked to give a running report of all sensory change, and in as much detail as possible.

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[15] It is very difficult to get complete adaptation to *R, BG, B, C* and *V* in a reasonable length of time unless the sun is shining brightly. The sky must be free from clouds or haze, or the times of adaptation will vary considerably.

Results.—The results are given in summary for the different observers and stimuli.

Observer B. Stimulus *Y*.—A gray film soon appeared in front of the stimulus-color. This film came in from the periphery. There was a slow change in chroma until the color faded out to a white or very light gray. A small area surrounding the fixation-point persisted longer than the other parts of the field. The change was slow at first, then more rapid towards the last.

Stimulus *BG*.—The outer edge began to pale rapidly at first. This paling extended inward towards the center. The center remained richer in chroma than the outer edge for some time. Finally the surface evened up into a gray field.

Stimulus *B*.—There was no change in hue. The haze appeared over the surface from the outside, although very slowly as compared with some of the other colors. The color persisted for a considerable time around the fixation point. In the process of fading out there were many bluish spots scattered about among white filmy spots. These spots would even up into a white or light gray surface.

Stimulus *R*.—While the color was fading out from the periphery, certain portions did not appear uniformly colored; little filmy red dots were scattered about over the surface. Finally the red spots paled out to gray, and then evened up over the whole surface. The color around the fixation-point remained for some time as a dirty brown color. The final gray surface was very dark.

Stimulus *V*.—There was a rough cloudy surface over the color for a considerable time. The color began to pale out from the outer edge, and the change traveled towards the center. The roughness always smoothed out into a gray film of a thick texture. It is very difficult to tell just when adaptation is complete.

Stimulus *O*.—It faded into a beautiful light yellow, beginning on the outer edge and gradually decreasing towards the fixation-point. From the *Y* it went into a gray which was somewhat darker than the *O*. The steps were perceptible though gradual. All changes were from the periphery towards the center.

Stimulus *C*.—Almost immediately the surface began to grow white in the outer portion of the field. This white gradually approached the center. The final stage was a lustrous silvery gray.

Stimulus *G*.—There was a slow change in hue from *G* through *Y*. The *Y* finally changed into a very light gray, very much lighter than the stimulus-color.

Stimulus *GY*.—The green faded out of the periphery very quickly. This process always took place before the *Y* began to go. The *Y* then changed into a very light gray. The surface of the stimulus-field at times had a rough, uneven appearance. This gradually changed into an even surface without any color.

Observer Sh. Stimulus *Y*.—Almost immediately the color began to grow lighter and the chroma poorer. There was no change in hue. As soon as the color began to fade a filmy surface appeared over the surface of the color. The fading out process always came from without towards the fixation-point. The periphery became white some time before the fixation-point. The final stage was an almost white surface.

Stimulus *BG*.—There was a gradual, though rapid decrease in saturation from the beginning. Also there appeared a very rough, uneven surface about the fixation-point, which was much more saturated than the rest of the field. This area gradually evened up into a gray. The hue changed slightly to *B*.

Stimulus *B*.— The blue faded out very slowly from the periphery to the center. At all times the hue appeared to shine through a white filmy surface which formed over the color. This effect persisted to the end.[16] The complete stage of adaptation was a very pretty whitish gray, like an after-image of a velvet-black surface. There was no hue-change whatever that could be observed. The entire process was a slow fading away of the color into a filmy surface. Two cases of staring blindness occurred during this series of observations.

Stimulus *R*.— The red is unlike any other color in adapting out to a gray. It passes through different stages of hue-change from the first. Almost immediately it passes over into a red-orange, then to an orange of poor chroma, and finally into a dirty, dingy brown, which is very persistent. This brown finally goes into a poor gray, then to a good gray surface.

Stimulus *V*.— Violet is the most stubborn of all the pigment-colors to a complete adaptation in sunlight. There is a feeling of uncertainty as to the judgment, because the violet is almost of the same brightness as the gray surface which persists when adaptation is complete. (The two are so near alike that it is often difficult for *O* to decide when the final stage is reached.) There is a rough cloudy surface at first, which gradually smooths down with time. Two changes in hue always take place. At first there is a tendency for the red to persist; this presently adapts out, and leaves a bluish-violet surface of very poor chroma.

Stimulus *O*.— Orange adapts out very much like red from the second stage of the red, except that the orange is richer in chroma than the red at this stage. When the color reaches the brown stage, it is almost exactly like the same stage in the red, except that the orange is brighter than the red. One effect of staring-blindness was observed, which lasted for three or four seconds.

Stimulus *C*.— Almost immediately the surface began to take on a bright lustrous appearance. This effect gradually thickened, until the whole surface was of a beautiful silver lustre. The hue changed somewhat to blue, thus causing the surface to become darker.

Stimulus *G*.— For the first few seconds the color did not appear to change. Suddenly there is a change, the yellow appears and the green seems to weaken. From this point there is a rapid fading away. The yellow appears to go out first; the green, now much less saturated, fades out into a beautiful gray surface which is very smooth in texture. Two cases of staring blindness were observed.

Stimulus *GY*.— The color is stubborn at first; the green suddenly goes out; and then there is a gradual, though rapid, fading away of the yellow to a beautiful gray surface. After the green passes away, the process of adaptation is very much like that of the yellow. The final stage, the gray surface, is similar to the same stage in the yellow adaptation.

Observer Z. Stimulus *Y*.— The color began to fade in the periphery almost immediately; this caused the center to appear much lighter, and richer in chroma. The small center persisted for some time, although slowly fading all the time. Finally the whole surface of the stimulus-color became a uniform gray

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[16] To get the effect of the soft white surface, when adaptation is complete, a Hering blue must be used. A paper that has a glossy surface is very difficult to observe for this length of time.

Stimulus *BG*.— There was no apparent change in hue. A thin filmy mist came in from the edge of the paper, which caused all color to disappear from the periphery. This colorless effect gradually went towards the center, until the whole color disappeared.

Stimulus *B*.— No hue-change. The light or white foggy mist approached the center from the periphery until the whole surface was a very poor *B*, almost gray. This state persisted for some seconds. Finally all color faded away, leaving only a gray field.

Stimulus *R*.— There was always a decided hue-change. It went first to a very poor yellowish *O*, then to a dirty yellow gray. This persisted for some seconds. The final stage is a peculiar gray. It appears to be a little darker than the stimulus-color.

Stimulus *V*.— There was a slight hue-change to *B*. The decrease in chroma was marked at first. However, the final state is difficult to judge, since the final gray and the *V*, very poor in chroma, are much alike.

Stimulus *O*.— There was a change in hue to *Y*, poor in chroma. This change began in the periphery, and traveled towards the center. Finally the center went out, exactly like the *Y*.

Stimulus *C*.— This is a very stubborn color. It appears to be uneven in surface during the adaptation-process. It changes from periphery to center, but not so markedly as the other colors. The rough uneven surface seems to smooth down into the dark gray film as it thickens over the surface. There is a hue-change, but it is difficult to observe in its course. It would appear to change to *B*, although this can not be observed clearly.

Stimulus *G*.— Very slow change to *Y*. This change only comes about when the color has lost nearly all chroma. The film thickens from the periphery towards the center. This gradually conceals all color. The resulting gray surface appears to be a little lighter than the stimulus-color.

Stimulus *GY*.— The periphery began to fade very quickly. For several seconds this part of the field looked like wool, while in the center of the field there was a *YG* of very good chroma. Finally the center began to fade out, rather suddenly, to a good gray surface. The hue changed to *Y*, although the final stage came when the color was very low in chroma.

Observer S. Stimulus *Y*.— The film came over the surface from the periphery in 20 seconds. There was no apparent change in hue. The final stage was a colorless paper.

Stimulus *BG*.—No change in hue. The surface began to fade out from the periphery, all the time going towards the center.

Stimulus *B*.— Same process as the *BG*. The final gray was somewhat lighter than the stimulus-color.

Stimulus *R*.— There was decided hue-change as the color began to grow less saturated by the gray film which came in from the periphery to the center. The color went through a poor *O* to a very weak muddy *Y*. This last stage persisted for some seconds.

Stimulus *V*.— This is a very stubborn color. It is difficult to decide when all the color has faded out. The film began to appear on the periphery very quickly, moving to the center. The final stage appears to be of about the same brightness as the stimulus-color. There is a hue-change to *B*, although it is very difficult to judge.

Stimulus *O*.—The film began in the periphery. For a little, this seemed to hang on the outer part of the color. Finally it moved over the center. At this time the hue changed to a very poor *Y*. Then the process was the same as *Y* in the last stage.

Stimulus *C*.— The color made some change in hue, but it was so slow and gradual that it was difficult to judge. There was a peculiar uneven surface during the course of adaptation. The cloud appeared from the periphery, moving to the center. This gradually weakened the chroma.

Stimulus *G*.— A hue-change to yellow in the final stage. The chroma gradually grew less, the center remaining longer.

Stmulus *GY*.— Same as *G*, except that the change to *Y* was more marked.

The first part of the accompanying table shows the adaptation-times under the conditions described. The second part shows times obtained, still in direct sunlight, when a disc, composed as indicated in the table, was viewed against a large grey background. Observation in these circumstances was difficult, owing to the lack of a fixation-point.

ADAPTATION TIMES, IN SECONDS.

AVERAGE OF FIVE OBSERVATIONS

Obs.	Sh		B		Z		S		
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av. of Av.
R	198.2	6.5	192.6	10.5	190.8	9.4	194.8	5.0	194.1
O	89.6	3.9	89.4	6.9	91.8	5.4	93.8	1.4	91.1
Y	45.6	3.9	39.2	3.8	43.6	6.3	55.0	1.2	45.9
YG	49.2	7.1	53.2	5.8	50.4	6.5	52.4	7.4	51.3
G	52.0	6.8	54.6	9.5	49.6	2.9	56.6	3.5	50.6
BG	126.6	5.6	109.4	11.9	115.2	9.3	128.0	4.0	119.8
B	247.6	4.7	220.2	12.9	201.8	8.6	211.4	4.7	220.0
V	200.0	8.4	201.4	4.7	201.8	1.7	234.0	10.8	209.3
C	102.0	8.4	100.0	10.0	108.0	1.6	111.0	4.8	105.2

180° OF COLOR + 180° OF BLACK AND WHITE (BRIGHTNESS-VALUE)

R	93.8	7.0			104.0	8.6	96.8	12.6	98.2
O	42.0	5.2			45.5	3.2	45.2	4.2	44.2
Y	21.0	2.0			27.0	5.4	22.2	1.8	23.4
YG	25.4	4.7			29.0	2.1	26.0	4.8	26.8
G	26.0	7.8			23.5	6.6	21.6	4.3	23.7
BG	57.0	4.8			50.3	3.4	56.0	6.4	54.4
B	118.0	13.6			122.0	2.6	123.8	7.0	121.3
V	108.0	6.4			98.6	4.8	112.8	7.1	106.5
C	53.4	7.1			44.3	2.4	52.0	6.0	49.9

FOVEAL ADAPTATION AT MEDIUM INTENSITY

PRELIMINARY SERIES

We next sought to determine the experienced course of adaptation to color, at medium intensity, when only the fovea was stimulated by the color.

For stimuli, a circular disc, 100 cm. in diameter, was cut from each one of the 9 colors shown in the preliminary experiment. Every disc was mounted upon a gray background of 24 x 30 in. The background was free from any trace of color, and had a brightness-value of 140° white and 220° black. A frame on a table held the stimulus-card upright. The stimulus was placed at a distance of 225 cm. from the eye of the observer; the image of the stimulus-color fell within the macula.

The experimental work was done in a large light optics-room. Light window-shades were drawn over the windows, so that the room was mildly illuminated with a soft white light. The instructions to *O* were the same as in the preliminary experiment.

The quantitative results (times in sec.) are set forth in the following table. The averages are of 5 observations.

Obs.	Sh.		S.	
Color	Av.	M.V.	Av.	M.V.
R	137	7.5	178	8.1
O	140	6.5	140	8.4
Y	100	4.5	105	4.2
YG	88	4.5	95	7.0
G	80	5.5	110	6.2
BG	100	3.2	98	3.1
B	118	4.5	109	3.2
V	201	10.5	199	7.0
C	175	7.2	178	8.0

The reports of the observers may be summarized as follows:

R. The color began to lose in chroma very quickly. The outer edge of the circle paled out first, the paling gradually approaching the center. There was a little hue-change; the *R* faded slightly towards a very poor *O*. The resultant gray was a little lighter than the stimulus-color.

O. A thin gray film gradually thickened over the surface of the color. This caused the chroma to become poorer with the progress of adaptation. The hue changed slightly towards a *Y*, very poor in chroma.

Y. The hue faded away under the gray film until no color was perceptible. There was no hue-change.

YG. There was a hue-change to *Y*, very poor in chroma. The final stage was somewhat darker than the color.

G. The hue changed to *Y*, very poor in chroma. The gray film very quickly obscured the surface of the color, only a greenish gray remained for some time at the last stage of adaptation. Slightly darker than the stimulus-color.

BG. There was no perceptible hue-change, only a fading out to a very pretty gray. The film began to come in from the edge of the disc, gradually approaching the center of the color. The gray field was a little darker than the stimulus-color.

V. The hue changed to *R* and then to *B*. The resultant gray was lighter than the stimulus-color.

B. There was no perceptible hue-change in this color. In every respect it faded out like the *BG*.

C. Gradually faded out to a *B*, very poor in chroma. This state persisted for some time. Then the field was almost of the same brightness as the stimulus-color.

It soon became evident that this experimental arrangement was unsatisfactory. The observers complained of eye-movement in almost every observation; and the difference in illumination from day to day influenced the adaptation-time considerably. Further, there is a marked difference between the adaptation-times of the two observers for certain colors, especially *R* and *G*. It was therefore necessary to secure better conditions of observation.

Part II.: Foveal Adaptation at Medium Intensity
Controlled Series

Procedure.—The experimental work was done in a dark room. The apparatus and stimuli were those described in our account of the preliminary series. The illumination was produced by three 75-watt, 110-volt, Edison-Mazda daylight bulbs; it was equal to 212.4 c. p. at a distance of 2.5 m. from the stimulus-color and a distance of 1 m. above and a little in front of *O*. The current was supplied by a D. C. motor in the university service.

In order to avoid eye-movement, as far as possible, the following precautions were taken. (1) A time-recording apparatus was constructed, to register accurately the time of every change in visual sensation. This made it needless for *O* to speak during an observation. The recording apparatus consisted, first, of a kymograph, set to revolve once in 3 min. A double electrical contact-marker worked over the smoked drum. To one marker was attached in circuit a large seconds-pendulum, while the other marker was in circuit with a telegraph-key operated by *O* in reporting the sensory changes. (2) A comfortable head-rest was fastened to the table in front of the stimulus-frame. With this arrangement, *O* could hold the head in a constant position during an observation.

O adjusted the head-rest, covered one eye with a black velvet eye-shield, and made ready to fixate the stimulus when the light was turned on. The instructions were as follows: "When the light is turned on, fixate the center of the colored disc, and hold it if possible without moving the eye. If the eye does shift from its fixation, bring it back to position as quickly as possible. When the light is turned on and you have fixated, press the telegraph-key five times in rapid succession. Every time the eye moves in any way during fixation, press the key once. For every color-change of any kind, press the key twice. When the color fades out, or when it reaches a point where it shows no further change, press the key again five times. You will be asked to give an introspective report of these changes at the end of every observation."

Results.—The results are given in summary for the different observers and stimuli.

Observer S. Stimulus *Y.*—An after-image came very quickly. The mist appeared from the outside, causing the color to become less saturated and much lighter. As the mist grew thicker, there was a tendency for it to fuse in the border before the center became solid; thus the center was the last part to disappear. The cloud was much lighter than the background.

Stimulus *BG.*—The after-image was noticed very quickly. Soon the cloud gathered over the surface of the color like a fog or mist. This caused the color to be much less saturated. For a little while I could see the color through the fog as if it were behind the mist. The cloud which covered the color was darker than the stimulus-color.

Stimulus *B.*—A soft white film began to form over the surface; at about the same time I observed the after-image; in fact, the after-image and the fog could be clearly recognized as one. The after-image was merely superimposed over the color, and appeared to stand out from the color. Where this haze fell on the outside edge of the stimulus-color and on the bordering background, it took on the nature of the after-image; where it was on the stimulus-color it was a beautiful white fog or mist. As this mist or fog gradually grew thicker, the color slowly lost in richness of chroma without any hue-change. The resulting gray was of the same shade as the background.

Stimulus *R.* The gray film was peculiar in this color. It came in from the outside. The color appeared very dark through it. The mist was not smooth, but rough, very much like hazy clouds on a summer afternoon. It never had a smooth texture, like the blue, yellow or orange. At times I noticed shooting phenomena from the outside, like smaller clouds suddenly moving across the surface of the disc.

Stimulus *V.*—The cloud suddenly came in from the outside, at about the same time as the after-image was noticed. It was very hazy at first, but slowly thickened to a very dense mist. The color did not seem to change in hue in any way, but the haze seemed to thicken and conceal it from view. The cloudy gray was somewhat darker than the background.

Stimulus *O.*—The after-image was seen very quickly, bordering the color almost all the way around; with this came a very peculiar cloud, which seemed to be a part of the after-image, extending over the color, hiding the color. For a while the view resembled the sun behind a luminous fog.

Stimulus *C*.— The color became almost a bright red behind the thin misty cloud, when the cloud would move a little to one side. The after-image and the cloud could be recognized as the same surface, floating over the surface as the eye moved. The cloud was smooth and not rough like the red. There were shooting phenomena as with the red.

Stimulus *G*.— The cloud appeared about the time the after-image was observed. When the haze became very thick, there was noticed a little yellow mixed with the misty surface. This was probably a hue-change of the stimulus-color. The cloud in the beginning seemed to move in from the outside, growing thicker towards the center.

Stimulus *GY*.— The cloud appeared as if in front of the stimulus-color in the form of a light hazy mist. It became thicker and thicker until the color was obscured. The color could be seen for a considerable time through the fog; as the mist grew thicker, the color appeared to lose in richness of chroma, until only a gray of about the same brightness as the background remained.

Observer Sh. Stimulus *Y*.— An after-image came very quickly, which extended all the way around the stimulus-disc. The after-image was at first very poor in chroma. Almost at the same time a whitish film began to form over the surface of the color. As it thickened, it appeared to be of the same brightness as the background. This gray surface began as a very thin sheet moving over the surface. At times it appeared as threads or filaments, stretching across the surface, as if they were in motion; these thickened into a solid uniform surface.

Stimulus *BG*.— There was a short interval of time before the after-image was seen on the border around the stimulus-color. The misty haze about the same time was observed to move inward over the surface of the color. The film soon thickened in texture so as to obscure the color.

Stimulus *B*.— The after-image began to show very quickly around the edge of the stimulus. The haze began to form almost as quickly, and thickened into a very white misty film. This stood out away from the color, and at no time did it appear to be a part of the stimulus-color. The color gradually lost in richness of chroma as the film thickened. When the cloud had taken on a filmy or sheet-like appearance, it was very easy to see that it was an extension of the after-image over the stimulus-color, as a misty white luminous fog. It looked very much like a thin sheet of tissue-paper spread over the color; the film at all times was in front of the color and not a part of the color. The whole adaptation-process for the blue was a fading out or disappearing of the color behind the fog.

Stimulus *R*.— The after-image appeared very quickly as a bluish-green border on one side of the disc. At first it was pale, or washed out. but it gradually increased in chroma until it was very rich. At the same time a haze began to form over the stimulus-color, which gradually grew thicker. The haze appeared to come from the outside of the disc and to settle in front of the color. This haze for the red was a rough, uneven mist or fog; not smooth in texture like that of the yellow or orange. The hue showed a remarkable change during the course of adaptation. It changed almost as soon as the haze began to form over the color into a red-orange, then to an orange of very poor chroma, and then to a very dirty, dingy brown. This brown was persistent for some time; finally it changed into a gray surface which was somewhat darker than the background.

Stimulus *V*.—A very pale, faded after-image soon appeared from behind the stimulus-color, scarcely visible on the gray surface surrounding the stimulus. The cloud or fog seemed to float in from the side of the color towards the center; not so pronounced as with the red. The film at first was thin and more solid than with the red, but not so smooth in texture as with the blue or orange. (In three observations there was a slight hue-change towards blue, the red going out first. In two there was only a gradual fading out of the color into the fog).

Stimulus *O*.— The pale blue of the after-image, which was seen almost from the first, soon increased in chroma until it was a rich, well saturated blue. At the same time a beautiful hazy mist was gradually thickening over the color. This film appeared very much like the sun behind a thin fog. An interesting phenomenon occurred with a shift of the eye. There could always be observed two distinct discs in the visual field; the one was the stimulus-color, and the other was the film or fog in front of the color. These two discs, when not superimposed, the cloud upon the stimulus-color, always gave three parts of a circle. The part of the luminous fog which was upon the background showed a very rich after-image. The segment of the stimulus-color which was uncovered by the fog, caused by a shift of the eye, was a rich bright color, of the same hue as the stimulus-color; its brightness always depended upon the brightness-value of the background with reference to the brightness of the stimulus-color. The third portion was that part of the stimulus-color upon which a part of the cloud was superimposed. This was a beautiful gray, in which no color could be perceived in the final stage of adaptation.

Stimulus *C*.— The after-image was slow in appearing. The cloud effect was generally noticed as soon as the after-image. A change in brightness is always noticed as the filmy surface begins to form over the color. At this point the observer can always tell when adaptation is really progressing rapidly, because the color begins to pale out very rapidly. The luminous fog was always smooth, gradually thickening from a filmy surface to a veil very much like a thin sheet of paper spread over the surface of the color. This finally became so thick in texture that the color was obscured.

Stimulus *G*.— The cloudy mist began to form over the surface of the color by floating in from the outside towards the center of the disc. This movement and the after-image came at the same time. This haze grew thicker, which caused the color to become less and less saturated until only a gray surface was left. Generally there was a slight change in hue towards yellow.[17] The yellow would appear as if it were in the foggy veil which floated before the stimulus-color.

Stimulus *GY*.— The after-image began as a very pale, washed-out carmine. It extended all the way around the stimulus disc. With an increase in the richness of the chroma of the after-image also came a thickening of the hazy film which formed with the after-image. The cloud was apparently of the same brightness as the background. In nearly every case the green was observed to adapt out first, leaving a dirty, muddy surface which remained as a stubborn color for a considerable time.

Observer L. Stimulus *Y*.— Soon a film formed over the surface of the color, which did not appear uniform at first, but thicker in spots. The color began to lose in saturation from the first, changing its hue to a tan or very light unsaturated yellow. The haze finally grew so thick that the color was obscured from view.

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[17] This was no doubt due to the fact that the Milton-Bradley *G* contains a slight amount of *Y*.

Stimulus *BG*.— The after-image and the cloud-effect appeared very quickly and at almost the same instant. The fog was very light and fluffy. It stood out in front of the color. The hue of the color gradually faded away behind this white mist as the fog became thicker. The final stage of adaptation gave a gray surface equal in brightness to the background.

Stimulus *B*.— The after-image was quickly observed; at about the same time the misty cloud spread quickly over the surface of the color. The fog was almost pure white and appeared to fit the stimulus-color. It seemed to stand out away from the color. This mist gradually thickened into a thick cloud which hid the color of the disc.

Stimulus *R*.— The cloud floated in over the surface of the color from the outer edge of the disc. It was not smooth in texture, but somewhat rough and uneven. The red disc changed at first into a kind of orange, then to a yellowish color of very poor chroma, and finally faded out to a dark gray.

Stimulus *V*.—.The after-image appeared very bright from the beginning. The misty haze is somewhat darker than the background. It gradually thickened over the color. It is difficult to tell the final gray, when adaptation is complete, from the violet when it is very poorly saturated.

Stimulus *O*.— The haze over the color and the after-image around the stimulus appeared at the same time. The cloud thickened and obscured the color from view. For some time the color could be seen as if shining through the fog. The final gray was of the same brightness as the background.

Stimulus *C*.— The after-image was at first dim and slow in appearing. The cloud was very thin at first and somewhat darker than the background. The color gradually paled away in the fog.

Stimulus *G*.— A very weak after-image appeared at first, which gradually increased in chroma. A misty cloud came in from the outside of the color towards the center. This haze increased in thickness until it obscured the stimulus-color. It is of the same brightness as the background.

Stimulus *GY*.— Cloud-effect appeared very quickly. This caused the color to become very much less saturated, as if a thin sheet of white paper had been suddenly spread over the surface. The mist thickened so as completely to hide the color. The final gray is the same as the background.

ADAPTATION TIMES IN SECONDS

AVERAGE OF FIVE OBSERVATIONS

Obs.	S		Sh		L		
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av. of Av.
R	84.8	7.0	91.4	7.9	98.0	6.4	91.4
O	88.2	7.4	92.2	5.4	92.4	11.1	90.1
Y	97.6	5.9	90.8	3.8	98.8	7.4	95.7
YG	62.0	6.4	63.2	3.4	58.4	7.5	61.2
G	55.2	1.3	58.4	3.5	55.0	3.2	56.2
BG	42.0	5.2	39.8	4.2	39.2	3.0	40.3
B	125.0	7.2	107.2	12.6	128.4	4.9	120.2
V	82.4	2.4	84.0	3.2	78.0	5.6	81.3
C	73.2	6.2	73.4	7.3	75.4	6.9	74.0

The curve for the times of adaptation in sunlight differs considerably from the curve for the same colors at medium intensity. *R* is relatively shorter at medium intensity, while *B* is slightly longer than *B* in sunlight. On the other hand, the *Y* and *G* regions of the two curves take almost opposite positions. There is not only a rapid rise in the *Y* region for medium intensity, but there is a shift of the shortest time to the *BG* region. This difference might evidently be due to a difference in the chroma of the *Y* and *G* regions, caused by the two sources of illumination.[18] We therefore determined the chromatic limen of observer Sh for every color under both sources of illumination.

Procedure.—A color-mixer was placed in the window, on the table used in the preliminary experiment, so that the discs should revolve in a horizontal position. The disc was made up of a black-velvet paper, a good white, and a colored paper. The relative proportion of the brightness-value of the color was kept constant by varying the black and white discs as the color component was increased or decreased. The method of limits was employed (five series). *O* did not see the disc until it was in motion, and then only long enough to make a judgment. The experiment was repeated in the dark-room with the illumination used for medium intensity, and also in diffused daylight.

The *results* are shown in the following table:

CHROMATIC LIMENS

COLOR	SUNLIGHT		DARK ROOM ILLUMINATION		DIFFUSED DAYLIGHT	
	Av.	M.V.	Av.	M.V.	Av.	M.V.
R	18.0	3.6	22.0	6.4	19.3	1.6
O	34.0	3.8	20.0	6.2	8.0	1.3
Y	55.0	6.1	18.0	3.8	19.7	2.5
YG	46.0	4.3	28.0	5.2	23.0	3.6
G	37.0	4.3	32.0	4.3	22.2	2.3
BG	30.0	2.2	40.0	5.4	19.0	1.6
B	12.0	3.2	12.0	1.1	11.0	1.6
V	22.0	4.6	30.0	3.6	33.0	6.3
C	26.0	6.3	25.0	4.1	13.3	3.1

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[18] The gray background upon which the stimulus-color was mounted, and which tested free from color under the daylight electric-bulb illumination, gave the following composition when placed in direct sunlight illumination: Black 215°, White 125°, B 12°, and G 8°.

There can be no doubt as to the effect of chroma on the time of adaptation in direct sunlight, and in illumination used in the dark-room experiment. The proportion, through the whole series of nine colors, runs in the right direction. Wherever the adaptation-time is long, the chromatic limen is small, and conversely. For diffused daylight, there is a slight variation; *R* and *V* are out of proportion. This exception may be due to the fact that *R* and *V* are the dark colors; since the illumination was rather weak, the limen would tend to be high. For all the lighter colors there is a close approximation to the law stated above, that where the time is high the limen is low. We shall later find that the same thing holds for spectral colors; chroma is the principal factor which influences the time of adaptation, with a possibility of intensity as a minor factor.

Part III.: Pure Spectral Colors

We proceed to describe the course of foveal adaptation to pure spectral colors, and to compare the behavior of these colors with that of surface-colors. We were careful to present the spectral color to *O* under controlled conditions. The wavelength of the spectral band producing the stimulus was determined before every observation; the intensity of the light producing the spectral color was always known; and the amount of light entering the eye was kept constant by the use of an artificial pupil.

Procedure.—The apparatus consisted of a remodeled spectrophotometer.[19] In addition to the original equipment of the apparatus, we inserted a field in front of the lens, so that any desired width of spectral band could be obtained in the form of a circular disc of color. An artificial pupil, 2.5 mm. in diameter, was placed 14 cm. in front of the stimulus field, and 2 mm. from the eye. A small projection-lantern, burning a 200 w. 115 v. Mazda nitrogen-filled daylight bulb, illuminated the spectroscope to produce the spectral colors. Before the lantern was placed a plate of thin and very finely ground glass to cut out the image of the filament in the light-bulb. The amount of light that entered the collimator of the spectroscope was 61.4 c. p.

The observations were made in the dark-room. All the light for illuminating the spectroscope was thoroughly concealed, so that only the color through the artificial pupil could be observed. A head-rest was set in front of the spectroscope.[20] All changes in color-sensation were recorded in seconds by means of the apparatus described in Part II. *O* was given 15 min. of dark-adaptation before beginning the experimental work, and a period of 5 min. was allowed for rest between observations. Since monocular vision was employed, and the eyes were alternated from observation to observation, there was a period of at least 10 min. before an eye was restimulated.

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[19] Manufactured by the Cambridge Scientific Instrument Co.

For stimuli, circular fields were cut from the spectral band having the following wave-lengths; *R* 740-770 *μμ*, *Y* 545-575 *μμ*, *BG* 495-525 *μμ*, *B* 440-470 *μμ*, *V* 405-435 *μμ*. The field was 6 mm. in diameter, and was just large enough to fall well within the macula lutea. The constancy of the light could be tested at any moment by means of a switch which turned the current through voltmeter and ammeter.

The *results* appear in the following table, as averages of five observations.

ADAPTATION TIMES IN SEC.

OBS.	SH.		S		Z		F		L		Av. of Av.
Color	Av.	Mv.	Av.	Mv.	Av.	Mv.	Av.	Mv.	Av.	Mv.	
R[21]	102	4	103	6	110	9	98	7	109	13	104.4
Y	166	10	164	10	180	21	Failed		166	7	169.0
BG	119	9	126	10	132	6	164	16	117	3	131.6
B	103	6	104	2	120	4	121	10	90	6	107.6
V	69	8	69	9	78	4	90	6	77	6	76.6

Observer F failed to get complete adaptation for *Y* at this intensity. In explanation we can only state that she had had less practice than the other *O*'s when she began the observations. All the others reported complete adaptation for all stimuli used.

The curve of the adaptation-times rises from *R*, reaches its maximum at *Y*, and gradually descends to *V*. This is almost an exact figure of the brightness-curve for spectral colors.[22]

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[20] It was found at the beginning that any movement, even that of winking the eye, influenced the time of adaptation. The head-rest enabled the observer to hold the head in one position, leaving only steady fixation to be attended to. After a little practice with the artificial pupil, a good observer has no trouble in holding an observation period, often without even winking the eye.

[21] A psychological *R* was made up from gelatins. It was composed of *R* and *B* gelatins so combined that no hue-change was observed during the process of adaptation. The time of adaptation was: Sh 100, Mv. 3.2; S 104.2, Mv. 2.1; Z 104.9, Mv. 6.4; L 98.9, Mv. 11.2. F did not observe in this part of the series.

[22] W. de W. Abney, *Researches in Color Vision*, 78-111.

Hence we may have recourse to two factors, to explain the difference in the curves for surface-colors and for pure spectral colors: brightness and chroma. To test the first factor, we equated all the colors for brightness-value and then repeated the series, with the following result:

Adaptation Times in Sec.

Color	Sh.		S		L	
	Av.	Mv.	Av.	Mv.	Av.	Mv.
R	96	5.4	82	4.5	105	3.1
Y	115	6.6	120	3.0	130	6.9
BG	97	5.5	100	6.7	96	4.3
B	59	3.1	68	6.5	65	1.1
V	68	3.1	52	5.0	68	2.3

It is clear that an equation for brightness does not altogether remove our difficulty. To test the second factor, namely, the possible effect of chroma, we arranged a series of experiments as follows. Three color-fields were used: 4.5 mm., 3 mm., and 2 mm. in diameter. Since *V* has the lowest adaptation-time, a stimulus of this wave-length and brightness was set up on a second spectroscope. White light was added to another color in the spectrophotometer until the observer judged the two colors equal in chroma. When this point was reached, the observer was given 15 min. of dark-adaptation, and then observations were made with the three fields; the same colors were employed as in the first part of this experiment. The following are the averages for 10 observations by every observer on every field:

3mm. Field

Obs.	Sh.		S		Z		F		L	
Color	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
R	47.2	5.4	63.4	3.3	48.0	6.0	62.4	4.8	67.0	9.2
Y	47.2	3.4	66.4	2.3	48.2	5.1	62.8	9.6	63.8	5.4
BG	47.2	2.6	65.2	7.4	46.0	4.4	60.5	3.1	74.2	4.8
B	41.8	1.8	57.8	5.4	45.6	3.4	61.9	3.1	70.0	2.8
V	44.4	2.4	63.0	4.4	46.2	4.2	62.6	8.2	67.8	3.4

4.5mm. Field

R	64.4	6.7	57.8	3.5	59.2	4.6	64.4	5.3	55.0	9.6
Y	46.0	4.4	75.8	5.0	46.0	4.0	50.2	1.9	74.2	6.5
BG	47.4	6.8	73.4	4.5	43.6	3.7	89.0	1.2	80.8	3.8
B	54.6	6.8	63.4	8.8	46.6	7.1	71.6	7.9	67.0	6.4
V	46.0	3.2	63.8	2.6	44.8	1.8	106.0	8.8	64.4	8.6

2mm. Field

R	50.6	5.7	62.0	4.8	46.2	3.8	55.1	6.3	64.2	10.6
Y	62.6	1.9	59.4	8.3	53.8	3.8	61.2	6.8	64.8	7.4
BG	47.8	7.0	56.6	10.0	50.6	5.9	70.0	10.2	65.0	5.2
B	52.8	9.8	69.2	3.0	55.0	4.4	51.1	7.1	63.8	5.6
V	57.0	2.0	58.6	8.5	53.4	4.7	52.4	6.4	67.2	14.1

The introspective reports may be summarized as follows:

Observer Z. *Y.*— After about 30 sec. I observed a thin filmy cloud which began to form on the rim of the circle. The cloud was darker than the *Y*, and contained some color. This film became thicker and darker until there was no color left. The *Y* under the film always became brighter, paler, and gradually took on a washed-out appearance.

R.— The red began to get lighter. The hue began soon to change to a very good orange. This began about the time a thin film was forming over the color. The filmy surface gradually grew darker and lost color, until a good gray surface covered the color. At the same time the hue changed from *O* to *Y*, and then finally to a gray.

G.— The green began to fade out to a *Y* as soon as it began to lose chroma. The loss of chroma began along the border of the colored field. I could recognize the after-image, when the eye moved, as being a portion of the filmy surface over the color. The film was a soft white cloud. The *Y* then faded out to a very good gray.

B.— The whole surface began to brighten up together with a reduction in chroma. There was no change in hue. The chroma gradually faded out to a good light gray. This process was like a cloud gradually thickening so as to obscure a color beneath it.

V.— The *V* began to lose chroma very quickly; at the same time there was a change in hue; first the red faded very quickly to a certain degree, then the blue, and finally both faded out together. This loss of chroma was again in the form of a white film collecting over the surface of the color. Began on the border of the field, and then spread to the center, all the time growing thicker until the color was obscured.

Observer S. *Y.*— There was no change in hue. In the beginning of observation there appeared a light ring around the edge of the stimulus-color due to contrast. The color gradually paled out to a good light gray surface.

R.— The red went immediately to a very pretty orange, and from this to a yellow. A dark gray cloud appeared in the center of the disc. This gradually spread out over the surface of the color, and covered the whole disc.

G.— There was no hue-change; only paled out to a good gray surface. The final stage was almost a white. The cloud first appeared in the center of the disc and spread over the whole surface of the stimulus-color.

B.— The blue did not change in hue, but lost in chroma, until a very pretty gray surface remained. The color gradually grew lighter as it lost in chroma. There appeared a gray cloud in the center of the disc which spread over the entire surface.

V.— The hue of the violet changed, first to blue, losing the red; then the bluish-violet lost in chroma, until a very dark gray surface remained. The cloud appeared in the center of the disc and spread over the surface of the color.

Observer Sh. *Y.*— A whitish film began immediately to collect over the surface of the stimulus-color. It appeared to be a little brighter than the color. The film gradually grew thicker until it obscured all the color, leaving only a light gray surface. There was no perceptible hue change.

R.—The red went through a color-change, first to orange, then through *O* to yellow, and finally to a dark gray. A film was observed forming over the surface of the color at the same time it began to change in color. The film began to form in the center of the stimulus-disc, and gradually spread outward, covering the whole disc.

G.— There was a change in brightness at the beginning. A bright ring was noticed around the edge of the field. This extended inward towards the center very quickly. Then a light film began to form over the surface of the color. This gradually grew thicker until all color was obscured. There was no hue-change.

B—There was a decided change in brightness. A light film formed immediately over the color. This grew thicker until all color was obscured. The resulting gray is almost a white.

V.— There was a change in hue towards the blue. The red faded out first; at the same time there was a loss in chroma, by a thin cloud forming over the surface. A very dark gray finally formed over the color.

Observer L. *Y.*—The color became a little lighter, and lost in chroma until it became a light gray. There was no hue-change.

R.— The *R* changed to *R-O, O, Y,* and finally to a dark gray.

G.— There was no hue-change. A cloudy mist began to form in the center of the circular field, and spread over the entire surface, which decreased the color in chroma until it was completely faded out.

B.— There was a gradual loss in chroma; no hue-change. A cloud-mist formed immediately over the surface of the color, which gradually thickened until no color was noticed. The final stage was almost white.

V.— There was a change in hue to a violet-blue color. The red almost faded out first; then the color was poor in chroma. The final stage was a very dark gray.

Observer F. *Y.*— The color grew paler by losing in chroma. There was no hue-change.

R.— There was decided hue-change in this color. It went to *O*, then to a very poor *YO*, which reminded me of a dirty *Y*. Finally lost in chroma, until a gray resulted which was darker than the color.

G.— The *G* lost in chroma by paling away. No hue-change.

B.—Same as the green.

V.—The *V* changed to *R* and then to *B,* all the time losing in chroma.

Since a chroma-match between two colors that differ in hue is only a relative matter, there will always be some variation. So far, therefore, as time of adaptation is concerned, we have every reason to be satisfied with our results. The averages for the 3 mm. field are, it is true, more uniform than those for the other two; the 4.5 mm. and the 2 mm. fields show slight fluctuations. But no variation is as great as with the brightness-equation.

From the introspective reports we find that there is no change in hue except for *R* and *V*. All the other colors which we used faded away steadily to a gray field. The adaptation-process makes its appearance in the form of a film or mist over the color. This causes the color to become less and less saturated, exactly as was the case with the surface colors. When there is a shift of the eye from fixation, *O* always reports three continuous portions of the field: a crescent of after-image; on the opposite side to this, a crescent of stimulus-color, very rich in chroma; and between the two crescents a white or gray surface over the stimulus-color.

ADAPTATION AT HIGH INTENSITY

It was found, in the first series with spectral colors, that foveal adaptation would not proceed to a neutral gray when the light that entered the collimator of the spectroscope was raised to more than 60 c. p. At the same time three important facts were observed in regard to foveal adaptation at high intensity. (1) All our records show that there is a tendency for eye-movement to increase very rapidly at the end of a fixation-period, in some cases as much as 4 or 5 times the amount at the beginning of a period. (2) Our *O*'s report that adaptation is always very rapid at the beginning of a period, no matter how high the intensity of illumination may be, provided it does not produce a blinding glare.[23] (3) There is always a good deal of pain in fixating a bright colored light, even with foveal fixation, when the intensity passes much beyond 100 c. p. This pain may be noticed even as long as two or three days after the observation. It appears, then, that when a certain intensity is reached the eye moves so frequently, during the latter part of the period, that the process of adaptation can not be completed. The pain is probably caused by the excessive contraction of the pupil in response to the reflex set up by the very intense beam of light striking the fovea through the artificial pupil. In the following experiment we tried to overcome these difficulties, and to study the process of adaptation at high intensities.

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[23] See P. G. Nutting, *Trans. Illum. Eng. Soc.*, xi, 1916, 943.

Procedure.—The exposure-apparatus was a projection-lantern which burned a carbon 13 mm. in diameter, carrying a 30 amp. (125 v.) D. C. current. The lantern was mounted with a projection-lens of 2 in. diameter. In front of the projection-lens was placed a short-focus lens to bring all the rays of light to a pencil-focus. The actual amount of light focussed by the lens was about 6,200 c. p. The head of the observer was held in a steady position by means of a head-rest. The focus of light was adjusted to fall within the eye. Colored gelatin-filters (Eastman Kodak Co.) of known wavelength and transmission-power were used for stimuli; a filter of any desired color could be immediately fitted into one side of a pair of goggles, made to hold the filter securely. In the other side of the goggles was fitted a dark piece of cardboard, to exclude all light from the unstimulated eye. A foveal comparison-light was set up in such manner that any spectral color, from the intensity afforded by the lantern to the lowest possible intensity, could be obtained immediately by shifting the lever of a rheostat. We measured the actual intensity of the light that entered the eye through the filters by the following method. A 500 watt Mazda lamp was screened to match daylight (acetylene flame and No. 79 filter) and was set up at one end of the photometer bench This procedure was adopted because the photometry of a carbon arc for this purpose is not as precise as the photometry of an incandescent lamp. A flicker photometer and a 20 c. p. comparison lamp were set up at the other end. When necessary, neutral-tint filters were used to reduce intensities.

For determining the transmission, a setting was made with the filter screening the 500 watt lamp, and then a setting was made without the filter; the ratio of these two readings on the candle-power scale gave the transmission. The following is an average of several trials:

COLOR	FILTER NO.	TRANSMISSION	x C.P. OF LANTERN=ABOUT	
R	70	7.6%	471.2	c.p.
Y	73	35.9%	2225.8	"
BG	75	18.6%	1153.2	"
B	48	19.5%	1209.0	"
V	76	1.6%	99.2	"

All work was done in the dark-room. *O* adjusted the filter to the eye, placed his head in the head-rest, and awaited the stimulus-light. The instructions were: "Fixate the bright circle of light until all color disappears. When you no longer see any color, remove the goggles, fixate the spectroscope, and describe what you observe." The following are the average adaptation-times in sec. for four observers:

R	Sh	92	The hue quickly changed to *O*, then to *Y*, very poor in chroma. The *Y* persisted for some time. Finally all hue disappeared. The foveal comparison-color was pure gray, much lighter than the stimulus-color. All intensities were gray.
	Z	109	
	S	120	
	F	100	
	Av.	105.3	

Y	Sh	165	There was no change in hue. The color gradually faded away. The comparison light at all intensities was almost white; no trace of color.
	Z	177	
	S	150	
	F	175	
	Av.	166.8	

BG	Sh	120	No change in hue. Very light gray. No color at any intensity.
	Z	112	
	S	98	
	F	130	
	Av.	120	

B	Sh	240	No change in hue. Very light gray for all intensities.
	Z	182	
	S	155	
	F	148	
	Av.	181.3	

V	Sh	200	There was a change in hue; the *R* apparently weakened first; then there was a noticeable decrease in chroma for the *B*. The *R* disappeared, leaving only a very weak unsaturated *B*, which slowly faded into a dark gray. The foveal comparison light showed no trace of color for any intensity.
	Z	183	
	S	185	
	F	220	
	Av.	197.0	

We were able to get complete foveal adaptation for all *O*'s with these intensities, as for all lower intensities. After a long fixation, beyond the point where adaptation was complete, we were able even to raise the comparison-light somewhat higher than the standard, and still had complete adaptation in the fovea. However, long fixation with a bright light is very painful; the pain even persists for two or three days as an ache or soreness within the eye.

It might be objected that the blinding glare of the positive after-image here influenced the judgment. We were, however, careful to test this factor before finally deciding upon the technique for the series. If the arc was fixated without the filter, there always resulted a blinding glare. When the foveal comparison-light was fixated in this condition, there was only a slight change in brightness of the color, which immediately passed away. This was not true when a stimulus-color was fixated. The foveal comparison-light was always colorless, and so remained. It must, however, be small enough to fall within the foveal cup, and must not extend to the periphery of the macula.[24] Otherwise a more pronounced effect will be noted from glare.

Adaptation at high intensity is difficult to describe; but so far as we were able to observe the same general process takes place that has been observed throughout the earlier experiments of this study.

Conclusions

In general adaptation to direct sunlight, as well as under other conditions of adaptation, both to surface and to spectral colors, the fovea has a longer adaptation-time for color than any other part of the retina. This law aparently holds for high intensities as well as for medium and low intensities.

Chroma is the principal determinant of the rate of adaptation. Intensity is a second factor. The effect of both chroma and intensity has been observed with surface colors as well as with spectral colors.

The rate of adaptation is always faster at the beginning of a fixation-period, gradually decreasing, and becoming very slow at the end. The rate at the beginning also depends upon the chroma; the richer the chroma, the faster is the initial rate of adaptation. It gradually becomes slower later, and the whole process lasts longer than for a color poorer in chroma.

There is no difference in the experienced course of adaptation between a surface color and a pure spectral color. A color, under the process of adaptation, very quickly begins to decrease in chroma. This decrease is observed in the form of a light film which spreads over the color. The film continually thickens, until the color is obscured under the fog. The appearance of a color after adaptation corresponds in general to a mixture of that color with its complement. This relation was observed in nearly every observation. If the eye moved slightly from fixation, there were always reported three parts in the field: a crescent of the after-image, an opposed crescent of the stimulus-color, and an intermediate neutral gray surface.

Under suitable conditions, adaptation in direct vision can be carried to completeness at any physiological intensity.

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[24] J. von Kries, *Z. f. Psych. u. Phys. d. Sinnesorg.*, xv, 1897, 327.

THE FOSTER-CHILD FANTASY

By Edmund S. Conklin of the University of Oregon

This study was designed to test the validity of the generalization made by the psycho-analysts that most or all children experience the foster-child fantasy, and to determine some further facts concerning the phenomenon. As a stage in the development of the "family romance" it is said that all or nearly all children conceive the notion that they are the children of much greater parentage. If this is so, especially as some think that it exerts much influence upon the conduct of the child, it is of great interest to students of childhood and adolescence.

The generalization seems to be well on the way toward acceptation. Abraham apparently considers it universal and sufficiently well demonstrated (1. pages 40-41). Adler refers to it as a not infrequent occurrence (2. page 206). Hall in his interpretation of Jesus' concept of divine parentage treats the foster-child fantasy as a very common experience of childhood (3. pages 251-2). Jelliffe is more daring and states that the family romance is a "universal phenomenon" (4. page 54), although he adds that any stage, and this could of course apply to the foster-child stage, may be "much abbreviated." White treats it as established in his introduction to psychoanalytic psychology (6.). Many Freudian writers, however, refer to or use the concept of the family romance with an air of certainty but without specific mention of the foster-child fantasy. Possibly they assume its existence as a part of the romance. Rank's study of the myth of the birth of the hero is most frequently given as reference (5.). Both Jelliffe and White quote Rank's presentation at length. And Rank presents the matter with apparent confidence in the conclusiveness of its demonstration. He also says that psycho-analyses are the only means of determining such traits (5. page 63).

Acceptance of the establishment of the foster-child fantasy as a common trait of childhood has hitherto depended upon the validity of conclusions from psychoanalyses, of psychoneurotic adults mostly, some of whom died long prior to the time of the analysis. That Rank's presentation so often quoted occurs in the course of a study of mythology should not be permitted to lead to the supposition that it is an induction from his myth data. He himself says that it is a summary of the traits of childhood discovered by Freud " from his experience with the psychology of the neuroses " (5. page 63). Whatever may be thought of the validity of generalization from such data, it must be evident that here is an opportunity for checking the results of psycho-analytic work by use of another psychological tool, the questionnaire. In fact it is for just such work as this that the questionnaire is best suited, for a census of certain human experiences. If it is true that most children wonder whether " they can really be the offspring of their prosaic parents " and develop notions about their real parentage, then it should not have entirely disappeared from voluntary recall by mid- or even later adolescent years. Hence questions presented to adolescents under circumstances calculated to arouse a sober attitude of willingness to cooperate should produce other and valuable evidence concerning the frequency and nature of the foster-child fantasy and serve at the same time as a check upon the generalization made from psycho-analyses. Such is the nature of the study here presented.

With such a purpose, the questionnaire could be very simple and direct. After many preliminary experiments it was finally used in the following form:

It has been observed that some people during childhood or youth acquire the notion that their parents are merely foster parents. The purpose of this questionnaire is to determine how common such an experience is and some facts about it.

Your sincere effort to answer the following questions accurately will be appreciated.

Negative answers if true are just as valuable as positive.

Write answers in the space following each question.

1. Age at present.

2. Sex.

3. Did you ever have the experience of day dreaming or believing that you were an adopted or foster-child, that your supposed parents were not your real parents? (Answer yes or no).

4. Was it merely a day dream or did you believe it to be true?

5. Who or what did you think that you were?

6. If you can remember what caused such thoughts, please state it as clearly as you can.

7. About how old were you at the time?

8. Did these ideas lead you to any special acts or changes of conduct? If so, please state what.

9. If you believed yourself an adopted or foster-child, about how long did the belief last?

10. Has the belief disappeared? Tell why it disappeared if you can.

11. State below any remarks or information which may be of interest concerning the matter.

This was answered by students in scientific departments of the University of Oregon and by students in the two upper classes of eight different high schools in the same state. The college students wrote under the supervision of the writer or one of his colleagues in psychology. In the high schools they were written under the supervision of the superintendent or principal, people experienced in the handling of students and here selected for their interest in research. Several of them had formerly been students in the writer's department. In order to insure as nearly perfect uniformity in the presentation of the questionnaire as possible, printed directions were supplied to all who assisted in its presentation. Care was taken to make certain that the students understood that a serious purpose existed for the questions, which otherwise might have provoked frivolity, and by this means serious answers were obtained. Out of more than nine hundred returns only five bore any indication of insincerity. However otherwise this study may be attacked the author is confident that insincerity in its data is not one of its weaknesses.

Nine hundred and twenty one returns came to the writer. Of this number five were rejected because of apparent insincerity, ten because of failure to answer some important question such as age or sex, and two more were dropped because they were actual instances of foster children. Thus the total number of returns upon which this study is based became 904. Of these 640 were from the two upper classes in the high schools and the remaining 264 were college students, mostly freshmen and sophomores. Their distribution by age and sex follows:—

AGE DISTRIBUTION

Age	14	15	16	17	18	19	20	21	22	23	24	25	over 25	Total
Male	2	11	38	86	68	56	44	21	13	5	5	2	7	358 or 39.6%.
Female	5	10	70	168	134	69	43	17	11	6	4	3	6	546 or 60.4%
Total	7	21	108	254	202	125	87	38	24	11	9	5	13	904

Socially this group might best be characterized as homogeneously American. Students of foreign born parents were few and students of foreign birth rare. They came as a rule from homes of moderate to considerable culture, identified chiefly with commercial, agricultural or professional pursuits. Extreme poverty or utter illiteracy would rarely be found.

FREQUENCY OF THE FANTASY

Question No. 3 was designed to discover the frequency of recallability of the fantasy and was worded to include with affirmative answers all degrees of belief or unbelief. It was answered by all of the 904 students participating in the study. The distribution of the answers was as follows, affirmative indicating recall of some experience with the fantasy and negative no immediate recall:—

	Male	Female	Total
Affirmative	87 or 24.3%	171 or 31.4%	258 or 28.5%
Negative	271 or 75.7%	375 or 68.6%	646 or 71.5%
Total	358	546	904

AGE DISTRIBUTION OF ANSWERS TO QUESTION NO. 3

Age	14	15	16	17	18	19	20	21	22	23	24	25	over 25	Total
Affirmative														
Male	1	4	8	27	13	8	13	4	3	1	3	—	2	87
Female	1	2	22	50	41	20	14	8	2	5	2	2	2	171
Total affirmative	2	6	30	77	54	28	27	12	5	6	5	2	4	258
Negative														
Male	1	7	30	59	55	48	31	17	10	4	2	2	5	271
Female	4	8	48	118	93	49	29	9	9	1	2	1	4	375
Total negative	5	15	78	177	148	97	60	26	19	5	4	3	9	646
Grand total	7	21	108	254	202	125	87	38	24	11	9	5	13	904

It will be observed that the median for the affirmative group is the same as for the negative group. The likeness of these distributions indicates that there is no significant change in the frequency of recallability with age, that for the mid-adolescent years at least there is no rapid suppression. The older ones recall the experience as frequently as the younger. If this phenomenon continues on into the higher ages it would be out of harmony with the psychoanalysts' assertions concerning the disappearance of childhood fantasies from voluntary recall, but the data here for the upper ages is not sufficient for generalization. It merely suggests an alluring opportunity for further research. That 28%, however, of this group of more than nine hundred young people can immediately recall having experienced the fantasy does lend considerable support to the assertions of the psychoanalysts concerning its frequency.

Attitude Toward the Fantasy

Preliminary experimentation and inquiry revealed that some people could recall definitely having experience with the fantasy but merely as a day dream while others had actually believed themselves foster-children. For this reason question No. 4 was designed to bring out the frequency of the two groups, but it did rather more than was expected. It led to answers which revealed many gradations of belief. Some would not even admit that it was a day dream and used expressions of which a few follow:—" I merely wondered if I might belong some place else," " Just a casual thought," " Did not believe it to be true but thought of possibilities if it was," " Merely wondered," " Merely a random thought not developed to any degree or thought of later," " Just pretending," " Just a thought that I knew at the time wasn't so," " I liked to pretend my parents were foster parents." For the lack of a better term such answers were classified below under the heading, Casual Thought. Of those who answered that they believed their fantasy to be true some added qualifying phrases which also indicate gradations on the scale of belief but at the other end of the scale from the samples just given. Here follow some of the phrases in the belief group;— " I believed that it might be true," " I perhaps believed it true," " Day dream at first later believed it to be so." " Half believed it at the time," " I believed it or rather hoped it to be true," " Perhaps I did not fully believe I had foster parents but I doubted that my parents were truly mine." But most of the answers in this group leave no room for doubt for they are such phrases as " I believed it " or " I thought it to be true." The day dream group also indicates occasionally such gradations of belief as indicated by the following quotations;— " A day dream but at times was uncertain," " Merely a day dream but thought on it a good deal," " More of a day dream rather wishing I might be some one else's child," " . . . . wondering how it would feel to be a foster child," " Day dream but I nearly believed it to be true," " It was dreaming I suppose and yet I often thought that perhaps I had had a different father," " Day dream afraid it might be." Below are the frequencies of the answers in tabular form.

Frequency of Belief in the Fantasy
(Answers to Question No. 4)

	Male	Female	Total
Belief	17 (19.5%)	54 (31.6%)	71 (25.4%)
Day dream	58	101	159
Casual thought	12	15	27
No answer	—	1	1
Totals	87	171	258

From this it will be seen that all but one of those who answered question No. 3 affirmatively gave some indication of their attitude toward the fantasy. Twenty five per cent of all who could recall the experience recall it as being believed at the time. More than 19% of the males believed it and more than 31% of the females. Of all the 904 answering the questionnaire 7.8% recalled experiences of the fantasy and believed it, 4.5% of all the males had it and believed it, and so did 10% of all females.

Forms of the Fantasy

(Answers to Question No. 5)

Answers to this question indicated several degrees of development of the fantasy. Many returns merely said "adopted child" or "Never thought that far" or other words with like meaning. Such answers are grouped in the table below under the first heading as they seem to represent an incipient or undeveloped stage of the fantasy. Then there are those, of considerable number, who thought themselves foundlings of insignificant inheritance or destitute orphans who had been taken out of pity from an asylum. A few of these answered with the single word, orphan; but most gave some qualifying phrase or word which indicated socially inferior origin. These are all grouped below under the second head. A small number very clearly stated that they thought themselves children of parentage of a similar social status (third heading in the table). Another considerable group had developed ideas of greatness. Kings, princes, great wealth, great beauty, great intellectual powers,— all these figured in the concepts of their supposed true parentage. A few thought of themselves as strange, mysterious or supernatural beings, using these very adjectives on their returns, and they are so described in the table. It may be worth while to add that these few did not all come from any one community but from different parts of the state, thereby eliminating the possibility of collusion. Then there were those who recalled the fluctuating nature of the fantasy and report themselves as thinking of now one and now another type of person. Under Miscellaneous and Indefinite are presented those who gave such answers as "Kidnapped," "Traded by mistake," etc. making them otherwise unclassifiable. The frequency of occurrence of each group is indicated below as well as the percentage which the total of that group is of the entire number answering this question.

	Male	Female	Total
Incipient stage of fantasy(No clear concept)	47	75	122 or 54%
Orphan or foundling	7	26	33 or 15%
Parentage of similar social status	2	5	7 or 3%
Child of great parentage (Great wealth 20, royalty or nobility 11, miscellaneous 9)	8	32	40 or 18%
Strange, mysterious or supernatural being	4	1	5 or 2%
Many different characters	4	6	10 or 4%
Miscellaneous and indefinite	2	5	7 or 3%
Totals	74	150	224

Of the two hundred and fifty eight people whose answers to Question No. 3 indicated experience of the fantasy, two hundred and twenty-four or 87% answered this question. It was answered by 85% of the males and 88% of the females who answered No. 3 affirmatively.

CAUSES OFFERED FOR THE FANTASY

The following table summarizes the answers to question No. 6 and in most instances the items are in the words of the questionnaire returns. The percentage figures indicate the per cent which the accompanying figure is of the total number of that class answering this question. (Totals here cannot be checked with totals in other tables because here some give more than one answer).

	Male	Female	Total
1. Suggestion: Books, stories, etc., and the hearing and seeing of actual instances of foster parentage.	30 (43%)	66 (46%)	96 or 45%
2. Dissatisfaction: Limitations of home, longing for wealth, discontent.	1 (1%)	9 (6%)	10 or 5%
3. Mistreatment (Actual or supposed): Harshness, partiality, teasing, fault finding, inconsistent treatment, desire for affection, lack of respect for parents.	19 (27%)	34 (24%)	53 or 25%
4. Peculiar Family Circumstances: Prolonged absence from parents, parental disagreement, great difference in children's ages, family quarrels, previous marriage of one parent.	2 (3%)	5 (3%)	7 or 3%
5. Accidental Causes: Parent misunderstanding child, misunderstanding of adult remarks.	1 (1%)	3 (2%)	4 or 2%

	Male	Female	Total
6. Activity of Mind (Instinct to think): Precocious philosophizing, mere play of imagination, much alone, nothing to do.	11 (16%)	16 (11%)	27 or 13%
7. Growth Stimulus: Early adolescent development, desire for the different.	1 (1%)	2 (1%)	3 or 1%
8. Absence of family resemblance, mental and physical.	3 (4%)	13 (9%)	16 or 7%
9. Miscellaneous: Praise with ulterior motive, self pity, lure of elsewhere or of the wild, mood, circumstances, desire for older brother, vivid dream.	7 (10%)	2 (1%)	9 or 4%
10. Unable to recall.	5 (7%)	13 (9%)	18 or 8%

Of those giving affirmative answers to question No. 3, 214 or 83% answered this question. Of these, 70 were males or 80% of the males answering question No. 3; and 144 were females or 84%. In only two instances is there a noticeable difference here between the sexes. Absence of family resemblances seems to be more significant for the girls and there is a similar preponderance of boys in the group called mental activity or the manifestation of the instinct to think. In passing it is interesting also to observe that all of the ten cases under dissatisfaction had ideas of greatness in their fantasies. This will be discussed further in the next section.

Relation of Fantasy Forms to Causes Given

Observing almost as many reporting notions of inferior parentage as there were of those reporting ideas of greatness, with other indications of developmental stages of the fantasy already mentioned, it seemed possible that a segregation of the causes which each of these groups offered might also be productive. The results of this segregation appear in the following table:—

	Orphan or Foundling Group	Ideas of Greatness
Suggestion	19 or 58%	12 or 30%
Dissatisfaction	0	10 or 25%
Mistreatment	5 or 15%	6 or 15%
Miscellaneous and Indefinite	9 or 27%	12 or 30%

The most conspicuous thing appearing in this comparison is the complete absence of dissatisfaction causes in the "orphan group" and its emphatic presence in the "greatness group." Mistreatment is about equally influential in the two forms of the fantasy, and suggestive causes are given much more frequently for explanation of the fantasy by those in the "orphan group" than by those in the "greatness group." For help in grasping the meaning of these likenesses and differences reference should be made to the presentation of data concerning the forms of the fantasy. There will be found indications of stages of development, as was also suggested in the answers to the question concerning belief in the fantasy. Following this suggestion of a developing fantasy the table just presented would seem to indicate that mistreatment and suggestion first give rise to the idea of foster-parent-hood which early takes the form of orphanage or inferiority of some sort, perhaps assisted by feelings of helplessness or inferiority in the face of supposed ill usage or partiality for other children. Then with the continuation of the fantasy, growth and the vigor of approaching adolescence come ideas of greatness, doubtless again assisted by the suggestive causes mentioned, and reflection on these ideas of greatness might be the reason for the appearance of the alleged dissatisfaction causes in the "greatness group," the dissatisfaction thus being more consequence than cause. This seems much more likely than that the ideas of greatness should come first and with continuation of the fantasy the dissatisfaction disappear. Comparison with the complete table presenting all causes alleged will show all dissatisfaction cases are found in this table correlated with ideas of greatness, whereas only a small number of the suggestion cause cases and mistreatment cause cases are found here. All dissatisfaction cases in this study had ideas of greatness. Such a statement would be far from the truth for any of the other cause groups. It will be further observed that while the miscellaneous and indefinite group is a considerable percentage of the whole only a most improbable distribution of them, if more definite answers were obtainable, could alter the relationships between the groups.

AGE TO WHICH FANTASY IS ATTRIBUTED

(Answers to Question No. 7)

The following table presents the ages which 240 of the 258 people who had recallable experience with the fantasy mention as being the approximate time of its occurrence:—

Ages	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Indefinite or No Answer	Total
Males	1	2	5	5	7	5	20	5	10	3	7	3	2	2	10	87
Females	5	8	10	11	28	18	26	7	21	11	6	7	3	2	8	171
Total	6	10	15	16	35	23	46	12	31	14	13	10	5	4	18	258

It will be observed that definite answers were obtained from 93% of the group. Medians for both the male and the female distributions fall in the ten year group. This is undoubtedly too low because of the method used in tabulation. Some gave a definite figure, some said about a certain age and of course in these instances the figure given was used. But there were also many who answered in terms of an approximate range of years, as for example " About ten to twelve." Where this was done the tabulation was by the lowest figure given. Thus the actual measure of central tendency is higher than ten years, probably between ten and twelve years. Of course these are only rough approximations, but it must be remembered that most of these are approximations by young people not very far removed from the period in question, by young people still in their adolescent years (the median for the entire group studied being 18 years).

EFFECTS OF THE FANTASY UPON CONDUCT

(Answers to Question No. 8)

In this table are presented in summarized manner the answers to that question which sought to discover if any considerable number could recall influences of the fantasy upon their behavior, and in the event of there being such what the nature of the conduct effects might be. Of course the table is based upon the answers of those who had in response to question No. 3 indicated recallable experience with the fantasy.

	Male	Female	Total
No answer given	15 or 17%	27 or 16%	42 or 16%
No conduct effect recalled	55 or 63%	84 or 49%	139 or 54%
Some effect reported (See next table)	17 or 20%	60 or 35%	77 or 30%
Totals	87	171	258

It is worthy of note that this question was answered positively or negatively by 83% of the males and 84% of the females, or 84% of the group reporting experience with the fantasy.

The 77 people or 30% above who reported that the fantasy did have some effect upon their conduct, which they could recall, describe the effect in various ways but with sufficient clarity to make possible an effective grouping. It appears in the table following:—

Nature of the Conduct Effects	Number	Per Cent
Deliberative thought, seeking proof, meditation, etc.	26	34%
Depressing emotional reactions:—		
Melancholy, embarrassment, fright, etc.	6	8%
Domesticating conduct:—		
Expression of greater affection for parents (1), conscious imitation of parents (1), improved conduct (1), more considerate conduct (2).	5	6%
Alienating conduct (from parents):—		
Teasing, tormenting, deceitful, disobedient, revengeful, etc. toward parents (7).		
Stimulated independence of conduct, running away considered and car ried out, or more self confident (9).		
Acting part of supposed real self (4)		
Reserved toward parents (4).		
Telling of the discovery (3).		
Seeking discovery by real parents (1).		
Deliberately acting in strange manner (1).		
Suspicious, sulky, saucy, snobbish, etc. (8).	37	50%
Miscellaneous (Poetic efforts, and disinclination to study).	2	3%
Positive but indefinite	1	—

The sub-topics in the table just presented are in most instances words and phrases taken directly from the questionnaire returns. In the recollection of these students the predominating effect of the foster-child fantasy upon conduct, or at least its conduct concomitant, is conspicuously that of alienation from the parents. This is of course in clearest harmony with the family romance generalizations of the psycho-analysts. The seeking of proof and meditative thought might be considered as a stage preliminary to the alienation in the fully developed phenomenon. So also the depressing emotional experiences reported. But the tendencies classed above as domesticating indicate a tendency in the other direction. Of course the writer is quite well aware that these may not be effects of the fantasy at all but merely growth concomitants, perhaps causing the fantasy. The writer is here merely seeking and presenting the opinions of his subjects.

As before, the returns in the "orphan group" and the "greatness group" were then isolated and the answers each gave to the question concerning conduct effects was tabulated according to the rubrics just used in order that any significant group peculiarities might again be revealed. The result was as follows:—

Effect Reported	Orphan Group	Greatness Group
Deliberative thought, etc.	6 or 18%	2 or 5%
Depressing emotion	2 or 6%	....
Domesticating conduct	3 or 9%	....
Alienating conduct	7 or 21%	13 or 32%
No conduct effect reported	13 or 40%	17 or 43%
No answer given	2 or 6%	8 or 20%

The great difference between the percentages of the two groups giving no answer to the question makes any generalizations from the other differences hazardous. The other differences suggest, however, a trend away from deliberative thought, depressing emotion and domesticating conduct (early stages perhaps of the fantasy) toward alienating conduct.

DURATION OF THE FANTASY BELIEVED
(Answers to Question No. 9)

People answering the questionnaire who had actually believed themselves to be foster children were asked to indicate as nearly as possible how long the belief continued. The following table summarizes their estimates:—

	Male	Female	Total	Per cent
No answer or too indefinite for use	4	8	12	17%
Very brief time (few minutes to few days)	6	11	17	24%
"Three months," "four months", or "few months".	2	5	7	10%
About one year	2	4	6	8%
About two years	1	13	14	20%
About three years	1	6	7	10%
About four years	..	3	3	..
About five years	..	1	1	..
"Several years"	..	3	3	..
About fifteen years (prolonged separation from parents).	1	..	1	..
Totals	17	54	71	

Some indicated in their answers that the belief was intermittent by the phrase " off and on " for the time specified. The long duration of the believed fantasy is a little surprising. Thirty five or 49% think the belief endured for a year or in some cases much more. It is also of interest that the question was answered in a specific manner by 83% of those reporting belief in the fantasy. Generalizations from these figures would, however, be unreliable because of inconsistencies in reporting. The experience was apparently spasmodic or recurrent with many; and in answer to this question some gave an estimate of the period within which these recurrences came, while others gave merely the approximate duration of a single occurrence.

DISAPPEARANCE OF THE BELIEF

The tenth question revealed that as a firm belief the fantasy had disappeared in all cases; although two or three report a lingering influence well described in the phrase of the one who said, "At times it seems to haunt me."

The second part of the question asked why the belief had disappeared, of course seeking the opinion of those answering. The answers to this appear in the following table:—

REASONS GIVEN FOR DISAPPEARANCE OF BELIEVED FANTASY

	Male	Female	Total	Per cent
Growth and development of intelligence, recognition of absurdity of belief, discovery that others had experienced the same thoughts.	9	24	33	46%
Parental intimacy (parental confidence, assurance and explanations and parental affection).	4	17	21	30%
Discovery of proof (Observation of family resemblances, discovery of records, etc.)	..	12	12	17%
Miscellaneous (One vague answer, one where aspects of belief failed to materialize).	..	3	3	..

The numbers in this table refer to the number of times each reason was mentioned. As some were mentioned more than once the numbers will not of course check with the other tables. The percentages are based on the number who believed their fantasy, that is 71. This question was answered by 83% of them in a definite manner. The others did not answer or said they were unable to do so.

COMPARISON WITH RESULTS OF PSYCHOANALYSES

That inquiry of more than nine hundred young people should reveal twenty-eight per cent who could immediately recall experience with the foster-child fantasy supports the Freudians at the outset. To this number ought of course to be added all those who could immediately recall the experience, those who had it and with whom it is now beyond even mediate recall (instances of which with adequate proof have come to the writer's attention aside from those here studied), and those in whom it may have suffered a psychoneurotic suppression. Of course such an addition cannot be made in terms of figures; but the mention of such cases indicates the minimal nature of the twenty eight per cent discovered by this study. Thus considered the number is sufficiently large to warrant the assertion that it is a common experience of childhood. So far the psychoanalytic generalization has been justified. And a more definite statement can now be substituted for Rank's that "many persons . . . very frequently remember occasions of this (foster-child) kind."

From psychoanalytic studies the foster-child fantasy was located in the pre-adolescent years (Freud's period of sexual latency or Jung's pre-sexual period). The two hundred and fifty-eight young people found in this study to have had recallable experience with the fantasy located it in the same period of development. The median of the answers given fell between the ages of ten and twelve years. The years eight to twelve inclusive covered 61% of the answers.

Taking Rank's oft quoted statement of the course of the family romance as the authoritative presentation for the psychoanalysts one finds in addition to the basic parental detachment motive of the romance the mention of two immediate causes of the foster-child fantasy, feelings of parental neglect or lack of affection and romantic literature. These two immediate causes have been amply verified by the results of this study. They were the two causes most frequently mentioned by these young people. But there has also been brought to light certain other supplementary causes, not mentioned by Rank, which in the minds of these subjects are of some importance. They are prolonged absence from the parents, marital infelicity observed by the children, precocious philosophizing, lack of companionship and the absence of mental or physical resemblance to the parents. The last is of peculiar interest not only because it is so common a topic of discussion before children but also because the subsequent development of family resemblances is several times mentioned in these returns as a cause for the decline of belief in the fantasy. One of those who had never experienced the fantasy remarked that "the reason I have never acquired such a notion is the fact that all my life every one has spoken of the great resemblance I have for my father."

Rank presents the fantasy as undergoing development from an early stage in which the child thinks of himself as an adopted or step-child to the higher stage characterized by expansive ideas, variously elaborated according to the child's adroitness and the material at his disposal. This questionnaire study not only supports but also considerably elaborates the psychoanalytic conclusion. All through the returns there are indications of the developmental stages of the fantasy. Concerning the forms of the fantasy there were those, a large group, who had but the vaguest if any idea beyond the thought of foster-childhood, some saying that they had never thought further than that. Then there were those who reported thinking themselves as different characters at different times. The apparent relationship of the different form groups (incipiency, orphan, same social status, great parentage, supernatural being) suggests the same thing. The relationship of forms to causes carefully tabulated indicated the developmental interpretation of the fantasy as the only feasible explanation of the otherwise strange distribution of causes mentioned. The relation of the forms to the reported conduct effects pointed, if less clearly, in the same direction. Examination of the conduct effects indicated stages from meditation on a more or less fascinating idea to decidedly alienating conduct. Degrees of belief also appeared very clearly in the answers given. Even without knowledge of the psychoanalysts conclusions it would have been difficult if not impossible to have interpreted this data otherwise. And the conclusion is in harmony also with the known traits of childhood imaginative fancy as well as the growth of delusions in paranoia.

The forms of the fantasy named herein revealed not only the two mentioned by Rank but also another equally prominent, that of inferior parentage, which seems to lie between the two because it has a definite form and the incipient stage lacks definition. The few cases of fantasies of parentage of similar social status hint at another intermediary stage, decidedly harmonic with the concept of an outward tending libido, which might follow the inferiority stage. And the few cases of those who conceived themselves as of supernatural or very mysterious nature might point to a still higher development of the fantasy, rising to heights above mere concepts of worldly greatness as in paranoid delusions. If true this might be of much interest to those attempting to apply psychoanalytic concepts to the interpretation of the founders of religion. But the writer is far from confident that the last two are genuine stages in the development of the fantasy. The number of cases is small and they might prove to be merely circumstantially conditioned variants from the normal course. For the inferiority stage, however, in which the child thinks of himself as a foundling or poor asylum orphan, following the earlier or incipient stage in which there is no concept beyond the thought of foster-childhood, there is sufficient evidence in the data presented. Of course the demonstration of a stage in the course of the fantasy characterized by inferiority concepts immediately reminds one of Adler, and it might be that ideas of greatness develop as compensations for these inferiority concepts which doubtless are accompanied by feelings of inferiority.

In considering the forms of the fantasy it is interesting to notice Rank's statement concerning the sources of the ideas of greatness. He says: " The child utilizes an accidental coincidence of actual happenings (meetings with the lord of the manor, or the proprietor of the estate, in the country; with the reigning prince, in the city. In the United States with some great statesman, millionaire) " (5, page 65). To the writer there is in that quotation the implication that in this country the fantasy is less likely to take the form of royalty or nobility. Perhaps this is so, but as a matter of fact eleven of the forty cases of expansive ideas, here studied were found to have the form of princes, kings or nobles. The appeal of non-democratic institutions is apparently not dead in the minds of American children.

As has been so often presented in the psychoanalytic literature, the principal motive of the family romance is the progressive detachment from the parents. Rank points out that revenge and retaliation motives also appear in the course of the romance, but considers them as stimuli for further imagings, further developments of the fantasy. The progressive detachment from the parents was clearly indicated in the writer's returns from those who recalled changes in conduct at the time of the fantasy. Many mentioned much thought about the matter and the deliberate seeking of proof for their notions, some wrote of depressing emotions as the consequence of their fantasy, a still smaller number state that they were moved to better behavior and greater affection for their parents (a domesticating tendency), but by far the larger number reported instigation to conduct which would inevitably alienate them from parental influence. The general direction of the conduct effect (or concomitant conduct) was clearly away from parental authority in the recollection of these adolescents. The few who mention a domesticating tendency may indicate a slight hesitation to take the plunge into independence, a slight oscillation at the outset before fixation of growth direction is established. The many reports also of revengeful and retaliatory conduct, the conscious tormenting of the parents, harmonizes perfectly with Rank's statements.

In fine this study seems to have resulted in an amplification as well as a justification of the results of psychoanalytic investigation. Psychoanalytic conclusions concerning the foster-child fantasy have stood the test of checking by a different method of research. At the same time it throws doubt upon the statement of Rank that the psychoanalysis of psychoneurotics is the only tool by which the imaginings of childhood may be studied (5. page 63). It is possible that the questionnaire might by itself never have discovered the foster-child fantasy and the family romance, but this study has demonstrated its usefulness as a tool for the checking of the results of psychoanalytic study and also for their amplification. Rank has compared psychoanalysis with empirical observation, but the questionaire as used here is not empirical observation. It approaches in its request for free personal recollection much nearer to the anamneses of psychoanalysis and has the advantage of being able to bring data readily from numbers large enough to permit of statistical treatment.

Summary and Conclusions

The foster-child fantasy has been demonstrated to be a common experience of childhood. Twenty-eight per cent of the 904 returns here tabulated could immediately recall experience with it.

Twenty five per cent of those who reported experience with the fantasy (7.8% of all) believed in it as a correspondence with facts. The others expressed many varieties of attitude from partial belief to none at all.

Three stages of development of the fantasy were indicated. In 54% the fantasy had never taken any definite form, 15% thought of themselves as poor orphans or foundlings, 18% had developed ideas of greatness. Two other stages were suggested in the returns but by numbers too small for generalization.

Suggestion (45%) and supposed mistreatment (25%) are most frequently given by those questioned as causes for the fantasy. But other causes, although less frequent, were indicated sufficiently often to deserve consideration; precocious thinking (13%), lack of physical or mental family resemblance (7%), and dissatisfaction with home conditions (5%).

The fantasy is located by more than half of those reporting in the period of 8 to 12 years of age.

The most common effect of the fantasy recalled, as described by these subjects, is conduct alienating them from parental influence and authority.

The duration of the fantasy for those who had actually believed themselves foster-children was reported to be more than a year by 49% (maximum 15 years). It was reported as of very brief duration by 24%, and as lasting a few months by 10%.

Growth and the development of intelligence is most often mentioned as the cause of the removal of the fantasy among those who had believed it, but there are two thirds as many mentions of parental intimacy and a smaller number reporting discovery of convincing proof, such as physical or mental similarities and actual records.

Comparison with the psychoanalytic presentation, especially that by Rank, results in both support and amplification of the generalizations from psychoanalysis. The frequency of the fantasy, the age location of it, causes given, two stages of its development, and the progressive detachment from the parents are alike in the results from both methods of study. The questionnaire method has, however, added the discovery of other causes, another stage in its development, some modification of statements concerning sources of the fantasy forms, and has revealed itself to be a serviceable supplement to psychoanalysis.

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The writer desires here to publicly express his appreciation of the earnest and expert assistance supplied him by the following officers of Oregon's public schools in collecting the data upon which the foregoing study is based:—C. A. Howard of Eugene, A. C. Strange of Baker, George Hug of McMinnville, A. G. Smith of Roseburg, C. R. Bowman of Klamath Falls, Naomi Williamson of La Grande, R. W. Glass of Cottage Grove, and P. E. Baker of Medford.

A METHOD OF STANDARDIZING THE COLOR VALUE OF THE DAYLIGHT ILLUMINATION OF AN OPTICS ROOM

By C. E. **Ferree**, G. **Rand**, and I. A. **Haupt**, Bryn Mawr College.

In a previous article (Psychol. Rev. 1912, XIX, pp. 364-373) we have given a method of standardizing the intensity of light in an optics room illuminated by daylight and have described provisions for keeping constant the intensity selected as standard. If still more exact conditions are wanted for conducting work on color sensitivity another variable, namely, the changing color of daylight, should perhaps be taken into account. It is the purpose of this paper to describe a method of standardizing the color value of the daylight illumination of a room and of correcting the changing values to the standard value in such optics rooms as we have in our laboratory.

We mean by standardizing here only a means of matching the daylight selected as desirable and of reproducing at any given time as a standard the color value which matches the daylight selected. Our plan does not include primarily a means of determining what is the most desirable color value of daylight to use, although that might well be made a valuable feature of the plan. To be of the greatest service in psychological optics the plan should be both reasonably simple of accomplishment and have a fair degree of precision. In devising a method we have endeavored to keep both of these requirements in mind. Obviously the first step to accomplish is to have a source of light the color value of which can be kept constant within acceptable limits. A well seasoned tungsten light operated by a constant current gives a light of sufficient constancy of composition to serve our purpose.[1] It has the advantage too that its changes of color with change of voltage are roughly similar in direction to those which occur in daylight. A second step is to have a means of changing the color value of the light to match the daylight selected as desirable and a comparison surface on which to make the match. The first of these purposes can be accomplished by means of thin gelatine filters of known spectrum transmission, properly selected and combined to give the color values needed; and the second by means of any photometer head which presents a sufficiently good field for the comparison. We have already described a simple and inexpensive photometer for daylight work which with the proper selection of filters can also be made to serve the present purpose very well. For a description of this photometer the reader is referred to this Journal, 1916, XXVII, pp. 335-340. When the proper filters are inserted on the side next to the standard lamp this photometer can be used to make both the photometric and colorimetric comparison with no change either in the apparatus or the adjustment of the lamp. With the instrument set for the color and intensity of light selected as standard it can be determined at a glance whether the illuminaton of the room fulfills the desired requirements with regard to both of these features.

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[1] In reply to an inquiry about the constancy of color value of the light emitted by the tungsten filament operated at a constant voltage, also its constancy as compared with the light emitted by the carbon filament, we have the following from the Bureau of Standards. "With regard to the constancy of the color from lamps operated at a constant voltage, it can be said in general that almost any seasoned incandescent lamp will burn a considerable time without any appreciable change in the color of the light, unless the filament is operated at a temperature above the normal. In comparing carbon with tungsten filaments the relative constancy will depend on the conditions. If the two are set at voltages which will give the same color, the tungsten will change much more slowly than the carbon In fact a tungsten filament operated at the normal color of carbon lamps will usually

We have stated that the method employed should have a satisfactory degree of precision and sensitivity. In order to get some estimate of the sensitivity of the method or what amounts of color change can be detected at different intensities of illumination, the following experiments were conducted. (1) The light from two well seasoned tungsten lamps of equal watt value (40-watt, type B Mazda) was brought to a color and brightness match at the photometer head. The change in voltage required to produce a just noticeable change in color tone was then determined. The work was conducted in a dark room and for convenience in getting the proper range of intensities the photometer head was removed from its place in the box photometer and mounted on an ordinary photometer bar. In making the determinations one lamp, A, was operated at 107 volts and set at the desired distance from the photometer head. The other lamp, B, was operated at such a voltage and set at such a distance from the head as was needed to give an exact color and brightness match. When

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burn thousands of hours with no appreciable change in color. Whether different lamps of the same kind give light of the same color depends on how closely the lamps are kept uniform in manufacture. As furnished on the market at present different tungsten lamps of the same size will be found to be more nearly alike than carbon lamps."

the match was obtained the voltage of lamp A was lowered until a just noticeable difference in color tone could be detected at the screen. A measure was thus had of the amount of color change that could be detected by the instrument in terms of the amount that is produced by a given variation of the voltage of a tungsten lamp the specification and the conditions of operation of which are known. When the voltage of lamp A was lowered the intensity as well as the color value of the light at the photometer head was changed. This required a resetting of the lamp B to bring the two photometric fields to equal brightness. However, as a check on the judgment of just noticeable difference in color tone each observation was made under more than one brightness relation between the two photometric fields. The judgment was not difficult to make. Its precision in fact compares very favorably with that of the photometric judgment. The determination was made with the lamps at different distances from the photometer head,—14, 20, 30, 40 and 50 cm. to give the differences in intensity at which it was desired to make the determinations of colorimetric sensitivity. For the sake of a comparison of the colorimetric sensitivity of the photometer heads more commonly used, the determinations were repeated with Lummer-Brodhun heads of the contrast and disappearance types. The results of these determinations are given in Table I.

TABLE I*

A COMPARISON OF THE SENSITIVITY OF DIFFERENT PHOTOMETER HEADS TO CHANGE IN COLOR OF LIGHT OF TUNGSTEN LAMP (MADZA TYPE B) OPERATED AT 107 VOLTS

Distance of Lamp From Photometer Head (cm.)	Voltage Change of Lamp to Give Just Noticeable Change of Color Tone		
	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head
14	0.25	0.5	1.0
20	0.50	1.0	1.5
30	0.75	1.5	2.5
40	1.00	3.0	3.0
50	1.25	3.5	3.0

*The results of this and the following tables are for Observer R. The main points were verified by a check observer, H.

(2) In the second series of experiments a comparison was made of the sensitivity to change of color tone with the three photometer heads, the Bunsen and the two types of Lummer-Brodhun, when the photometric surfaces were illuminated by the tungsten lamps, natural color, and when they were illuminated by the light from these lamps filtered through gelatines so selected as to match the daylight in one of our optics rooms at 11 A. M. on a clear day. The intensity of light at the photometer head was made the same for both kinds of illumination. The comparison was made at three intensities of illumination, corresponding to those given by the tungsten lamps in the preceding set of experiments when placed at 30, 40 and 50 cm. from the photometer head. The comparison was not made for the other intensities because they could not be obtained on our photometer bar on account of the reduction in intensity produced by passing the light through the daylight filters. That is, the lamps could not be brought nearer to the photometer head than 14 cm. and to match the tungsten lamp at 30 cm. from the head, for example, the lamp in front of which the filter was placed had to be set at a distance of 14.3 cm. The results of this comparison are given in Table II.

TABLE II

A COMPARISON OF THE SENSITIVITY OF DIFFERENT PHOTOMETER HEADS TO CHANGE IN COLOR OF LIGHT OF TUNGSTEN LAMP (MADZA TYPE B) OPERATED AT 107 VOLTS AND OF THIS LIGHT FILTERED TO MATCH DAYLIGHT

Distance of Lamp From Photometer Head Giving Equal Illumination for Each Intensity for		Voltage Change of Lamp to Give Just Noticeable Change of Color Tone					
		Type B Mazda Lamp			Type B Mazda Lamp With Daylight Filter		
Type B Mazda Lamp (cm.)	Type B Mazda Lamp With Daylight Filter (cm.)	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head	Lummer Brodhun Head Contrast Type	Lummer Brodhun Head Disappearance Type	Bunsen Head
30	13.5	0.75	1.5	2.5	0.25	0.50	0.75
40	18.8	1.00	3.0	3.0	0.25	0.50	1.00
50	25.2	1.25	3.5	3.0	0.25	0.75	1.50

(3) A somewhat limited comparison was made of the sensitivity of the Bunsen head to change in color tone when the photometric surfaces were neutral and when they were colored. This was done because the color of some pigments is known to be very sensitive to changes in color value of the

light falling upon them. For the purpose of making this comparison a number of such standard pigments as are commonly found in psychological laboratories were substituted for the white screen of the Bunsen head. Two cases were made of this investigation. (a) Six colored screens were used, selected from the Hering series of papers: the dark red, the orange, the yellow, the yellowish green, the blue-green and the violet; and three from the Milton-Bradley series: the red-violet, the red-violet, tint No. 1 and the orange-red, tint No. 1. The light from the 40-watt lamp was in each case passed through the daylight filter referred to above. The brightness of the photometric surfaces was kept constant in all cases at a value equal to that of the white screen illuminated by the filtered light of the tungsten lamp at a distance of 74 cm. This was the highest brightness that could be obtained with this lamp for the colored screen having the lowest coefficient of reflection. This intensity of illumination was selected in order that the colorimetric comparison should be made in all cases for the same brightness of surfaces compared. The results of this comparison are given in Table III. (b) Since in the preceding

TABLE III

A Comparison of the Colorimetric Sensitivity of the Bunsen Photometer Head When Provided With White and Colored Fields—This Comparison was Made With the Intensities of Light Adjusted to Give in All Cases the Same Brightness of Photometer Fields, Namely, the Brightness of the Blue Pigment (Pigment With Lowest Coefficient of Reflection) Illuminated With the Highest Intensity That Could Be Obtained With the Filtered Light of the Mazda Type B Lamp Operated at 107 Volts

Photometer Field	Distance of lamp (cm.) from photometer head giving brightness of colored fields equal to that of white field illuminated by this lamp with daylight filter at 74 cm.	Voltage change of lamp to give just noticeable change of color tone.
Yellowish-green	43.0	1.0
Violet	15.1	2.0
Red-violet	22.6	3.0
Orange-red, tint No. 1	40.0	3.0
Red-violet, tint No. 1	33.5	4.0
Yellow	50.8	4.0
Orange	38.0	5.0
Dark red	31.5	6.0
Blue	13.8	6.0
Blue-green	27.3	8.0
White	74.0	4.0

experiments the pigments of the higher reflection coefficients had to be used at lower illuminations than the other pigments in order to fulfill the conditions that the colorimetric comparison should be made in all cases on surfaces of equal brightness, it was decided to make a comparison of the most favorable of these colors with the neutral screen at illuminations approximately equal to that used for the color of lowest reflection coefficient in the former experiments. This, it will be remembered, was the highest that could be obtained with the filtered light of the 40-watt lamp. For this purpose the yellowish green, the yellow and orange-red, tint No. 1, screens were used. The results of this comparison are given in Table IV. These results, it scarcely need be pointed out, have a much more direct bearing on the problem of selecting screens for our standardizing instrument than those of the former comparison. That is, in the selection of screens for such instrument we are concerned with relative sensitivities at equal illuminations, not equal brightnesses, of screen. Obviously in the final selection of screens for any particular instrument that screen should be chosen which shows the greatest sensitivity for the range of illuminations possible for the instrument or for the range that is likely to be used.

TABLE IV

A COMPARISON OF THE COLORIMETRIC SENSITIVITY OF THE BUNSEN PHOTOMETER HEAD WHEN PROVIDED WITH WHITE AND THE MORE SENSITIVE COLORED FIELDS USED IN TABLE III, AT INTENSITIES OF ILLUMINATION EQUAL APPROXIMATELY TO THE HIGHEST THAT COULD BE OBTAINED WITH THE FILTERED LIGHT OF THE TYPE B MADZA LAMP OPERATED AT 107 VOLTS

Photometer Field	Distance of lamp (cm.) from photometer head giving equal brightness of white and colored fields for Mazda type B lamp with daylight filter	Voltage change of lamp to give just noticeable change of color tone
Yellowish-green	16.5	0.25
White	26.5	1.50
Yellow	13.5	1.50
White	20.0	1.0
Orange-red, tint No. 1	15	1.50
White	22	1.25
Violet	15.1	2.0
White	74	4.0

An inspection of the results in Tables I-IV shows the following points.

(1) As might be expected smaller color changes could be detected at the higher than at the lower intensities of illumination. That is, at the higher intensities the color of the light was less saturated (the inhibitive action of the achromatic on the chromatic component of the retinal excitation) therefore a smaller change was needed to be just noticeable.

(2) The colorimetric sensitivity of the photometer heads employed is in the following order from greatest to least,—the Lummer-Brodhun, contrast type; the Lummer-Brodhun, disappearance type; the Bunsen. As bearing on the type of field that gives high colorimetric sensitivity such comparisons are of importance to colorimetry by the monochromatic method.

(3) Smaller changes of voltage were required to produce a noticeable change in the color of the light filtered to match daylight than in the color of the unfiltered light of the Mazda type B lamp.

(4) Of all the colored screens employed the yellowish-green, the violet, the yellow and the orange-red, tint No. 1, showed greater sensitivity to changes of color of light of the type produced in these experiments (changes similar to those which occur in daylight) than the white screen when the comparison was made at equal brightness of screen. When, however the comparison was made at illuminations approximately equal to the highest illumination that could be obtained with the filtered light of the 40-watt lamp only the green showed a greater colorimetric sensitivity than the neutral screen. The green screen seemed to be peculiarly sensitive to the changes which are produced by adding the longer wave-lengths to daylight. That is, the adding of the red wave-lengths appeared to decrease the saturation of the green and the adding of the yellow wave-lengths, to increase the yellow component already present. At the highest intensity of illumination used a change of only 0.25 volt was needed to cause a noticeable change in the color of the screen. Indeed at this intensity of illumination the colorimetric sensitivity of the Bunsen head was so much improved by the substitution of the green screen as to equal that of the more sensitive of the two Lummer-Brodhun heads with the white screen.

As we have already indicated the work of standardizing for color value and intensity may be done at the same time. It may be accomplished as follows: A daylight is selected of the color value and intensity desired. On the side of the photometer head next to the standard lamp gelatine filters are inserted so chosen that when the light from this lamp run at a given voltage and set at the position on the bar needed to give the intensity match, is filtered through them, the photometric surfaces illuminated by this light match in color value the surface illuminated by the daylight. To reproduce this standard at any future time all that is needed is to reset the lamp in the same position on the bar and to reproduce the voltage. With the photometer set up at the point of work in the room or as near to it as possible, the process of checking up the illumination both as to color value and intensity becomes very simple. A glance at the photometric field is sufficient to give the desired information. Since it is not so easy to arrange for the correction of daylight to the color value chosen as standard as it is to correct the intensity, feasibility may dictate that the work be done within a certain range of variation of color value. The apparatus recommended may be used to standardize this range as follows: If the daylight chosen as desirable be that of skylight near the middle of a clear day the changes that are apt to occur during the course of the day or from day to day may be approximated roughly by lowering the voltage of the standard lamp or by lowering the voltage supplemented by the addition of one or more of the thin gelatine filters properly selected. When this is done and the lamp is reset to compensate for the change of intensity produced, a range of variation of color value is fixed within which the daylight illumination must fall or be rejected for the particular work in hand. In such a case the photometer is set so that the standard surface in the photometer head is illuminated with the light chosen as the limit towards which the color value may vary and still be accepted for the work in hand. It is very easy then at any particular time to judge whether the dominant color of the daylight incident upon the receiving surface of the photometer falls within the limiting value.

Obviously a means of correcting the changing daylight to the color value chosen as desirable would be an advantageous supplement to the work of standardizing. The method we purpose for use for this is as follows: At a distance above the diffusion sash of ground glass installed beneath the skylight in our optics rooms, sufficient to give a good spread of light, will be installed opaque pendant reflectors of the distributing type. These reflectors will be supplied with filters which transmit an excess of the short wave-lengths. They will be installed on separate circuits, one or more to the circuit, so that a variable proportion of the artificial light may be used as is needed. Further to vary both the composition and the amount of artificial light used, wall rheostats will be included in as many of the circuits as is found to be advisable. Quite a wide range of composition and intensity of light can be obtained also by the use of lamps of different types and wattages. The artificial units may be installed well out of the road and the desired direction and throw of light be obtained by the shape of the reflector and the angle at which it is installed. Further the amounts of daylight used in getting the desired composition may be controlled by a system of curtains placed under the skylight above the diffusion sash and the artificial units. The elaborate system of curtains which is already installed beneath the diffusion sash in our rooms will serve as they do now for fine gradations of intensity of light and may prove useful to some extent perhaps for the control of the composition.

An alternative to a flexible system of correction of actual daylight for both composition and intensity is the utilization of some one of the artificial daylights which are now on the market. Of the unfiltered sources the carbon dioxide tube gives perhaps by common agreement the closest approximation to skylight. Its cost, however, is prohibitive for the greater number of laboratories. The same thing might well be said of the best of the filter units. The ones most familiar to us all and the most available from the standpoint of cost are the blue bulb lamps. With regard to these lamps, however, only a rough approximation to daylight is claimed. We have thought that it may be of interest to show here a spectrophotometric comparison of one of them, the type C-2 Mazda lamp, and of some of the closer approximations to daylight, with the black body at 5000 degrees absolute which is sometimes taken as the standard of average daylight. (See Fig. I.) For the comparative curves given in this figure we are indebted to the Electrical Testing Laboratories, 80th street and East End avenue, New York City. The comparison here, it will be remembered, is photometric, not colorimetric.

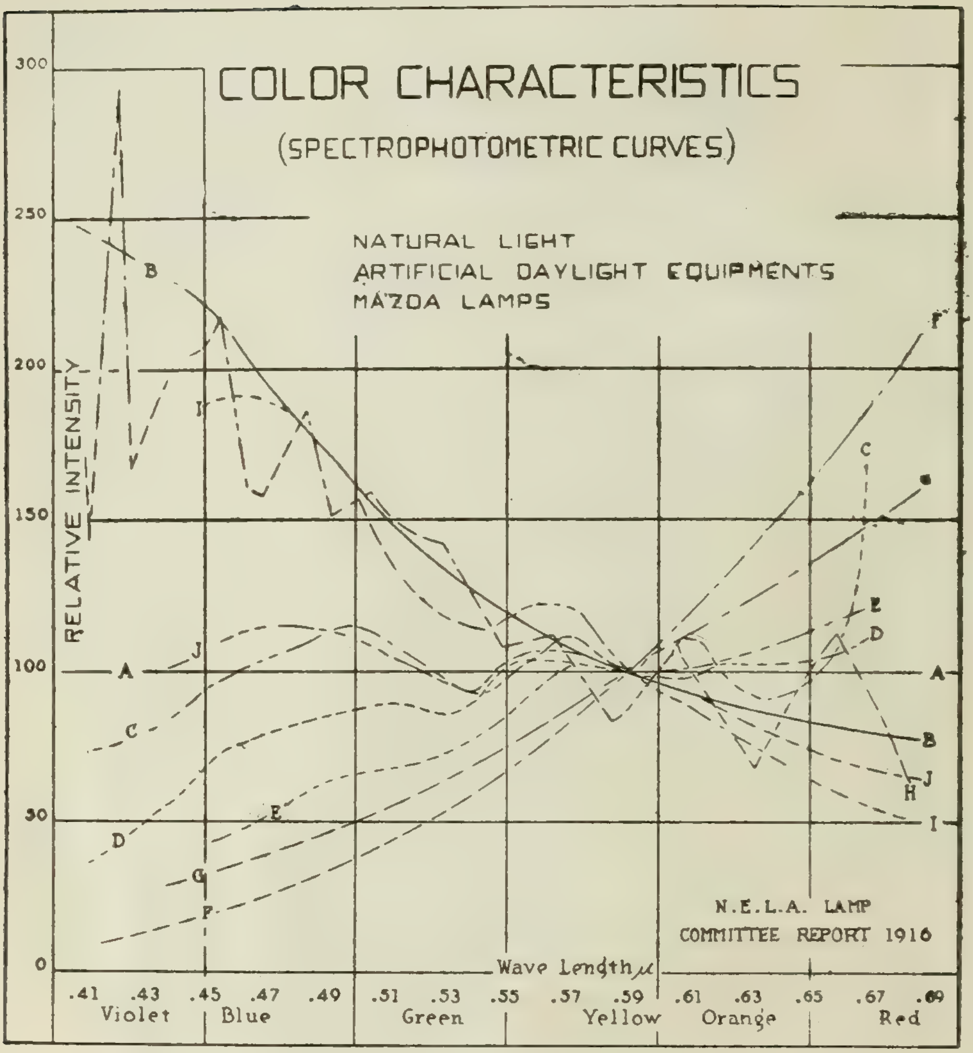

FIG. I

A—Black body at 5000 degrees absolute ("Average Daylight")
B—Blue sky (Ives) I. E. S. Transactions, 1910, p. 208.
C—Daylight glass with Mazda C lamp (Brady) I. E. S. Transactions, 1914, p. 952.
D—Bluish glass with Mazda C lamp (Sharp) I. E. S. Transactions, 1915, p. 220.
E—Mazda C_2 lamp.
F—Mazda B lamp (7.9 lumens per watt).
G—Mazda C lamp (20 lumens per watt).
H—Moore tube (Paper read before I. E. S. November 11, 1915).
I—Trutint glass, (Luckiesh) I. E. S. Transactions, 1914, p. 839.
J—Trutint glass, (Luckiesh) I. E. S. Transactions, 1914, p. 839.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER, H. P. WELD and K. M. DALLENBACH

XLVII. SIZE *vs.* INTENSITY AS A DETERMINANT OF ATTENTION

By ALMA M. BOWMAN

This paper is related to two of the experimental studies on the determination of attention which have been published from the Cornell Laboratory. Meads, working with form *vs.* intensity, obtained from five observers the positive result "that a light-form of relatively low intensity may have as great a power to attract attention as a formless light of relatively high intensity."[1] Curtis and Foster, working with size *vs.* intensity, were unable to reach any positive conclusion. Their three observers gave widely differing results: one showed a positive and one a slightly negative effect of size, while to the third size was apparently indifferent; and in the case in which the effect of size was negative, spatial position proved to be more important than a fourfold increase in area.[2]

It may very well be the case that form is, and size is not, a powerful determinant of attention. If this conclusion is sound, then the diversity of result obtained by Curtis and Foster means simply that in their experiment the effect of size was cut across by other influences which the plan of the experiment did not permit them to bring under control. There is, however, between the two studies a difference in method. Meads employed the method of limits; Curtis and Foster, taking their cue from the report of one of Meads' observers (Professor Weld) that expectation might be a disturbing influence, changed over to the method of constant stimulus-differences.[3] Since now the method of limits has given positive results, and the method of constant stimulus-differences has given results that are positive, negative and indifferent, it seems plainly necessary to attack the problem of size by the method of limits. This is what we have done in the present paper.

Apparatus.

The apparatus was set up, as usual, in a dark room. The observer, screened by heavy black curtains, sat comfortably at a table, his head held firmly in position by an adjustable biting board. Facing him at the distance of approximately 50 cm. was the ground glass plate upon which the two areas and the fixation-point appeared. The stimuli were as before: a standard Greek cross of 56 sq. cm. was compared with similar crosses of half and twice its area. The fixation-point lay between the crosses, at a distance of 10 cm. from each; it was illuminated throughout the experiment, though it was not visible during the exposure of the crosses.

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[1] L. G. Meads, "Form *vs.* Intensity as a Determinant of Attention," this JOURNAL, xxvi., 1915, 151.

[2] J. N. Curtis and W. S. Foster, "Size *vs.* Intensity as a Determinant of Attention," this JOURNAL, xxviii., 1917, 294f.

[3] Meads, 151; Curtis and Foster, 293.

Exposure was made by a Wundt gravity tachistoscope set for one-tenth of a second; the actual time, as measured by the Hipp chronoscope, was 100.3 ± 0.7 sigma. Heavy felt pads, laid at the base of the instrument to catch the shutter, made the exposure almost noiseless.[4]

The light was obtained from a 40 watt Mazda Daylight lamp placed in a projection lantern behind the tachistoscope. The rays passed into a diffusion box which was white inside, and whose far end contained two circular ground glass windows, 18 cm. apart and 8 cm. in diameter. The amount of light issuing from these windows was controlled by cardboard shutters, which by a graded series of circular openings carried the illumination by steps of 4% down to 12%, by steps of 2% down to 4%, and by steps of 0.5% down to 0.5%. The illumination of the comparison crosses might be varied within these limits; that of the standard cross remained constant at 20%.[5]

Experiment.

The following instructions, identical with those of the previous work, were read to the observers at the beginning of every experimental hour. "At the signal 'Now' put your attention definitely on the fixation-point. Two crosses of unequal size will be exposed. Judge which of them, if either, is the more clear; that is, which of them catches your attention the more." The method employed, for the reasons given above, was the method of limits. The observers judged 'Right,' 'Left,' or 'Equal.' 'Doubtful' judgments, in accordance with present practice, were thrown out, and the observations were repeated.[6]

The observers were F. L. Dimmick (*D*) and L. B. Hoisington (*H*), assistants in psychology; Myrl Cowdrick (*C*), graduate student in the department; and K. M. Dallenbach (*Da*). *D, H.* and *Da* were highly practised in the observation of clearness.[7]

Before the experiment began, a preliminary series was given to every observer in order to determine the critical range. Short warming-up series were also given at the beginning of every experimental hour. The experimental series were arranged for measurement of the space error and for compensation of the irregular influences of practice and fatigue.

Every observer made four series: two with the large cross, standard right and left; and two with the small cross under similar conditions. The series was composed of ten ascending and ten descending observations. The order in which the four series were presented to the observers was as follows:

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[4] Meads (150) used a spring tachistoscope with a time of 110 ± 8 sigma; Curtis and Foster (293) used a pendulum tachistoscope with a time of 110 ± 3 sigma.

[5] Meads and Curtis and Foster used an episcotister; but Curtis and Foster found the range of experimental variation insufficient (294). Our arrangement overcomes this difficulty.

[6] S. S. George, "Attitude in Relation to the Psychophysical Judgment," this JOURNAL, xxviii., 1917, 33ff.

[7] *D* is the observer who showed the positive effect of size in the work of Curtis and Foster (293, 295); *H* had observed in a study of the measurement of attention (this JOURNAL, xxvii., 1916, 459); and *Da* had observed in several such studies (this JOURNAL, xxiv., 1913, 465ff.; xxvii., 1916, 443ff.; xxix., 1918, 204ff.).

SERIES	OBSERVERS			
	D	*H*	*Da*	*C*
Large cross, standard right	1	2	3	4
Large cross, standard left	2	3	4	1
Small cross, standard right	3	4	1	2
Small cross, standard left	4	1	2	3

A series was completed within a single experimental hour.

Results.—The results are given in the table below:

O	Position	Small Cross				Large Cross			
		Av.	M. V.	Gen'l Av.	Deviation from stan'd	Av	M. V.	Gen'l Av.	Devia'n from Stand'd
D	right	53.0	13.8	43.2	+23.2	10.9	4.7	10.5	—9.5
	left	33.4	8.3			10.1	4.7		
H	right	79.2	4.8	59.1	+39.1	27.6	5.8	15.2	—4.1
	left	39.0	10.6			2.9	1.5		
Da	right	40.2	6.6	38.9	+18.9	18.0	2.5	14.0	—6.0
	left	37.6	5.2			10.1	4.3		
C	right	30.6	5.2	24.2	+4.2	23.0	5.6	19.0	—1.0
	left	17.7	3.1			15.1	3.7		

This table shows the average per cent. of illumination necessary for the comparison crosses if they are to be equal in clearness to the standard cross. The data of the 5th and 6th, 9th and 10th columns indicate that the small cross, to be as clear as the standard, must be more intense; and that the large cross, to be as clear as the standard, must be less intense. The *direction* of our results is thus constant for all four observers. There is, however, a marked difference between *C* and the other observers: her *plus* and *minus* deviations from the standard are very small. *C*, it may be noted, was a comparatively untrained observer. If we consider only the results of the three observers who were highly trained in the observation of clearness, we find uniformity of tendency, well-marked in all cases, though showing individual variation of the order 2 to 1. For *C*, the *m. v.* is also larger with both crosses than the difference between the standard and comparison stimuli. Even for the trained observers, the *m. v.* with the large cross is not always satisfactory.

But although the direction of the results is constant, the *magnitude of the space error* proves that the effect of size was cut across and obscured by another influence. The data which indicate the effect of position are given in the 3d and 7th columns of the table. For both of the comparison areas and for all observers the left-hand position is the more favored. The position-ratio is 2 to 1 for *H* and *C* with the small cross, and rises as high as 9 to 1 for *H* with the large cross. The left-hand position has, however, no advantage in the matter of relative variation.

Conclusions.—We conclude that the difference between the results obtained by Meads for form and by Curtis and Foster for size is a true difference, not attributable to the difference of the methods employed.[8]

If we may judge by the results of our trained observers, size as determinant of attention may bear to intensity a ratio which varies from 3:1 to 4:3. For a less trained observer size was indifferent.

In the two experiments upon size (that of Curtis and Foster and our own) spatial position proved to be a major disturbing influence. It seems that the left-hand position has an attentional advantage. We are now undertaking a further and more systematic study of this subject.

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[8] Cf. also H. Woodrow, "Outline as a Condition of Attention," *Journal Exp. Psychology*, i, 1916, 23 ff.

THE TONOSCOPE AS A MEANS FOR REGISTERING COMBINATION TONES[1]

By Evelyn Gough and Genevieve Robison

The possibility of producing and demonstrating combination tones in the mass of air was clearly established by Helmholtz.[2] He found that the essential condition for the generation of strong objective combination tones was that the same mass of air should be violently agitated by two generating tones simultaneously. The siren and the harmonium, each of which possesses a common wind chest in which the air is set in vibration by all notes played upon it, produced objective combination tones which were extremely powerful.

The presence of the combination tones in the air was demonstrated by the use of vibrating membranes and resonators adjusted to be in unison with these tones. Helmholtz found in using the harmonium that when the two generators were supplied by air from different bellows the objective part which the resonators reinforced was much weaker. When no common air chamber or mechanical connection existed for the two tones, as in the case of tones from two singers, two separate wind instruments, or two violins, the reinforcement of the combination tones by resonators was small and dubious. In such cases the combination tones, which might be clearly audible, he believed to originate from vibrations in the ear, either in the tympanic membrane or in the ossicles of the middle ear.

Apparatus and Method

The present paper reports experiments showing that the Seashore tonoscope has certain unique advantages as an apparatus for the objective demonstration of combination tones.[3] A description of the apparatus employed may throw light on the results. An early model of the tonoscope driven by a synchronous motor at a speed regulated by a 10 v. d. tuning fork was available. It was energized by a commercial alternating current. For controlling the strength of current closely the rheostat furnished with the tonoscope was supplemented by electric lamps. A manometric capsule, designed with special view to effectiveness with the high pitches of female voices, was substituted for the standard piece. It differs from the standard in certain respects. The capsule is much smaller, the membrane being 12.5 mm. in diameter. The gas supply is admitted at relatively high pressure through a small inlet (.25 mm. in diameter). A relatively large tube (2 mm. in diameter) leads from the gas chamber to the flame tip. The flame tip is one of the small-sized tips which are used on small acetylene hand lights. A gas cock makes possible fine adjustments of the flame.

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[1] From the Psychological Laboratory of Smith College. The writers wish to express their indebtedness to Professor D. C. Rogers for his generous assistance in regulating and modifying parts of the apparatus used and for his helpful suggestions in the preparation of this paper.

[2] Helmholtz, Sensations of Tone, pp. 152-159.

[3] Seashore, The Tonoscope, Psychol. Monog., No. 69, 1914, XVI, 1-12.

Ordinary dental dam is used for the membrane. For the purpose of this particular experiment a rubber voice tube 30 cm. long was employed. It was stopped at the end with a cork and sealed. In opposite sides of this tube were inserted two smaller tubes with tin funnels at their free ends for mouth-pieces.

Two tones sung simultaneously into the branches of the voice tube evidently move along a common air passage to the membrane and through it are communicated to a gas chamber and a flickering flame in turn. In passage they strongly agitate a common air chamber, a common gas chamber, the rubber walls of a common voice tube, and a loosely stretched membrane, any one of which would, perhaps, be capable of adding combination tones to two primary tones which are made to vibrate through it.

The purpose of this experiment has been to discover whether or not with this apparatus it is possible to get distinct readings on the tonoscope for vibration rates of tones formed by the combination of two tones sung simultaneously into the voice tube, which are wholly different from the readings produced by either tone sung alone.

The inquiry has so far been confined to difference tones of the first order and summation tones of the first order. Pairs of pitches were selected which could be sung easily and read plainly from the tonoscope, and whose difference or summation tones, if they appeared, would also possess vibration rates within the effective range of this instrument. Variously pitched tuning forks gave the objective pitches which the singers attempted to duplicate at each trial. Each of the two tones was sung and recorded separately, then they were sung together, and a complete reading was taken.

RESULTS

			Pitch from Tuning Fork	Tonoscope Readings
1	Generators	a^1	440 v.d.	146 x 3 = 438 v.d. 219 x 2 = 438 v.d.
		c^1	264 v.d.	132 x 2 = 264 v.d.
	Difference Tone	f		174 x 1 = 174 v.d.
2	Generators	g^1	396 v.d.	196 x 2 = 392 v.d. 131 x 3 = 393 v.d.
		b	247½ v.d.	125 x 2 = 250 v.d.
	Difference Tone	d		142 x 1 = 142 v.d.
3	Generators	g^1	396 v.d.	192 x 2 = 384 v.d. 128 x 3 = 384 v.d.
		a	220 v.d.	110 x 2 = 220 v.d.
	Difference Tone	f		164 x 1 = 164 v.d.
4	Generators	f^1	352 v.d.	175 x 2 = 350 v.d. 117 x 3 = 351 v.d.
		a	220 v.d.	110 x 2 = 220 v.d.
	Difference Tone	c		131 x 1 = 131 v.d.
5	Generators	b	247½ v.d.	124 x 2 = 248 v.d.
		g	198 v.d.	196 x 1 = 196 v.d.

RESULTS—Continued

		Pitch from Tuning Fork	Tonoscope Readings
Summation Tone	(a^1+)		(Theoretical position at 148 x 3 = 444 v.d.) None observed
6 Generators	a	220 v.d.	114 x 2 = 228 v.d.
	f	176 v.d.	171 x 1 = 171 v.d.
Summation Tone	(g^1)		(Theoretical position at 199 x 2 = 398 v.d. 133 x 3 = 399 v.d.) None observed

With the manometric capsule which was employed in these tests tones whose vibration rates lie within a range of the first, second, and third, multiples of the dot frequencies on the tonoscope are easily read. The patterns corresponding to the different dot frequencies, however, are distinctly different in appearance. Pitches which coincide in rate with a dot frequency within the range of the tonoscope give strong contrasts and bring out the dots in much elongated form and at the same distances apart that they hold on the stationary drum. Those which correspond to multiples of the dot frequencies cause the dots to appear with degree of contrast, separation and elongation which are in inverse proportion to the multiple numbers.

Readings for the difference tones were easily gotten in the tests, and where the generator tones were so chosen as to give difference tones within the range for direct reading, the dots representing these tones stood out with striking clearness. In fact, where the generator tones corresponded to multiples of dot frequencies while the difference tones coincided with them in rate, as in the four illustrations given in the table, the dots representing the difference tones were generally clearer than those representing the generator tones. The correspondence between the tonoscope readings for the generator tones and the vibration numbers engraved on the forks from which the pitches were taken was only approximate. The readings for the difference tones, however, coincide exactly with the calculated differences between the readings for the generators.

The results from the summation tones were entirely negative, no readings corresponding to the summation numbers appearing on the screen, although strong primary tones whose summation effect could have been recorded if sufficiently pronounced were used. Modifications in the apparatus favoring the higher tones or the use of lower primary tones might possibly lead to a different outcome.

In summary: The Seashore tonoscope furnishes a means for the demonstration of difference tones which possesses the following distinct merits. The apparatus furnishes conditions favorable for the generation of strong objective difference tones. Clear visible evidence of the difference tones is present. Objective visible readings are produced for each primary tone separately, and for the difference tone and both generating tones simultaneously. A practically untrained observer can record readings for the three tones accurate to one or two vibrations a second.

References:

1. Helmholtz, H. L. F. Sensations of Tone. Tr. by A. J. Ellis, 1895.
2. Seashore, C. E. The Tonoscope. Psychol. Monog., No. 69, 1914, XVI, 1-12.

BOOK REVIEWS

The Elementary Nervous System. By G. H. PARKER. Philadelphia, J. B. Lippincott Co., 1919. 229 p.

This interesting volume is one of the series of monographs on Experimental Biology which are being prepared by a number of American biologists. Its purpose, according to the author, is to portray by means of experimental and quantitative methods the elementary nervous system as it exists in the simpler animals and in the simpler parts of the more complex forms.

In the introduction a brief comparison is made between the ancient and modern ideas of the nervous system wherein it is shown that the complex mechanism of higher animals, according to modern research, is composed of relatively simple elements, the neurones, arranged upon a comparatively uniform plan which from a physiological standpoint falls into three distinct categories. The first is a *receptor* system which is found in the sense organs that receive the external stimuli and produce the sensory impulses. The second is an *adjustor* system or the central nervous organ that receives the stimuli from the receptors and directs them to the appropriate muscles, glands, etc., for an orderly response. In the more highly organized animals the impressions may be made upon the adjustor system as memories and become a permanent part of the animal's nervous equipment. The third category consists of the *effectors* such as muscles, glands, etc., which enable an animal to react to changes in its environment. These three categories are not found in the nervous system of the lower forms but are shown to have developed from a simple, primitive condition consisting of nothing but an effector system.

The main body of the book is divided into three sections. The first one deals with experiments on the behavior of sponges where no nerve tissue is found. Movements are accomplished by means of cells comparable to a primitive form of smooth muscle fiber or others that represent a stage of differentiation between amoeboid motion and simple muscle contraction. No coordinating system is present anatomically or physiologically. All the evidence collected shows that sponges contain the beginnings of a neuromuscular mechanism with the most primitive constituent, muscle, around which the remainder of the system is supposed to have been evolved. Independent effectors, similar to those found in sponges which are not innervated but are activated by direct stimulation, are shown to occur in the higher animals. The examples cited are the iris of the eye, heart muscle, amnion of embryo chicks and acontia of sea anemones. Also sluggish forms of transmission exhibited by the ordinary tissues of the sponge are found in the more complex organisms as in their ciliated epithelium.

The second section of the book is concerned with receptor-effector systems that mark the second step in the evolution of the nervous system. In the sea anemone the neuromuscular structure is composed of a dense meshwork of neurofibrils extending from the outer body layer to the more important systems of muscles in the mesenteries. In these animals the effector systems are more numerous and complicated than in the sponges. They consist of mucous glands, ciliated epithelia, nematocysts and muscles. Of these, however, the first three are without nervous influence and are, therefore, independent effectors. In the jellyfishes a marked increase in sensitivity is found over that manifested in sponges due to the acquirement of sense organs or receptors and the establishment of nerve connections that create a quicker and more efficient system by increasing the sensitiveness of the new member, the receptor, rather than on a considerable change in the original member, the effector. From the anatomical standpoint this primitive nervous system as seen in the Coelenterates is diffuse, in the form of a nerve net, but it shows some polarity by forming bands or trunks with some resemblance to nerves that permit receptive cells to control muscles or effectors situated some distance from them. Physiologically, the parts are relatively independent with no central control.

In the third section of the book the author discusses the anatomical and physiological relation of the elementary nervous system of the sponges and Coelenterates to the central nervous system of the more complex animals where a new element arises in the form of a central organ or adjustor in the region between the receptor and effector. It is this portion that, in the higher animals and man, has given rise to the central nervous system with all its complexity. In conclusion he says, "Thus the system that arose secondarily around an independent effector, muscle, has in the end gained such supremacy as to take to itself a number of independent effectors, any one of which might in the beginning have served as the nucleus around which the first nervous tissue could have taken origin."

The argument is based upon minute anatomical evidence and a great mass of data collected from numerous ingenious experiments, most of which are very simple in nature but at the same time furnish evidence that leads to definite conclusions. The subject is presented with unusual clearness and accuracy of detail and in such a simple and interesting manner that persons with no more than an elementary training in biology can read this monograph with profit. It is a contribution of utmost importance to the origin and evolution of the nervous system.

I. A. FIELD.

El Psicoanalisis. By HONORARIO F. DELGADO. Lima, 1919. 58 p. Also *La Psicologia de la Locura.* Madrid, 1919. 20 p.

Dr. Delgado's monograph on psychoanalysis is very inclusive and shows a thorough theoretic knowledge of the subject. His reading of both German and English literature has obviously been very extensive, and has not been limited to any one school, so that in his own synthetic presentation, although he follows Freud very closely, he does not neglect the contributions of such men as Adler, Jung, Maeder et als.

The first chapter of Dr. Delgado's *El Psicoanalisis* is devoted to an outline of the Freudian views of the rôle of the sex instinct in human life. Broadly speaking, it deals with the subjects of infantile sexuality, narcissism, Oedipus and Electra complexes, arrest of the libido, repression, the dream mechanism, symbolisation, etc. The application of these principles in the explanation of such perversions as homosexuality is adequately set forth, and the necessity of a natural development of the libido for the formation of a normal personality is emphasized.

The second chapter of the monograph is a brief but clear presentation of the theory of the sexual etiology of the psychoneuroses and allied mental diseases. The anxiety neurosis, neurasthenia, hysteria, compulsive neuroses, paranoia, dementia praecox, and manic-depressive psychoses receive separate consideration in a manner which shows that the author has grasped the full implications of the psychoanalytic doctrines. A little peculiar, however, may appear the inclusion in the same category of the *defence mechanism,* which is usually considered with the other Freudian mechanisms described in the preceding chapter.

The third chapter gives an account of the methodology and technique of the psychoanalytic therapy. The last two chapters are of a more general nature, and are devoted to a defence of the psychoanalytic theory and practice, and to a rather philosophical discussion of the new dynamic conception of the psychic life which the psychoanalytic statements involve.

The small pamphlet called *La Psicologia de la Locura* (The Psychology of Madness), is a brief statement of the new psychological data concerning this problem which the psychoanalysts have given us. It is written in popular style, and does not pretend to have the scope of the larger monograph. The latter is to be highly recommended to students best acquainted with the Spanish language both as a summary of the subject and as a guide to other literature in the same field.

PHYLLIS BLANCHARD.

BOOK NOTES

The Psychology of Musical Talent. By CARL EMIL SEASHORE. Boston, Silver, Burdett and Co., 1919. 288 p.

This comprehensive monograph is addressed to students of applied psychology. Its content must appeal to all interested in music, and teachers and students of childhood should also find much material of value in it. For years the author has devoted himself to this subject, and he is our chief American authority upon it, so that psychology may well congratulate itself that it has here the carefully drawn-up statement of the results of long and constant study.

After treating of the psychology of the musical mind the author then discusses in subsequent chapters the sense of pitch, of intensity, time, rhythm, timbre, consonance, auditory space, voluntary motor control, musical action, musical imagery and imagination; musical memory, intellect, and feeling; and the individual and training in the art.

Human Psychology. By HOWARD C. WARREN. Boston, Houghton Mifflin Co., 1919. 460 p.

This book has no preface or introduction. It has a long list of acknowledgments. It is evidently the material of the author's course of instruction well digested by many years of active teaching. It is abundantly illustrated and has a great many tables. The following are the chief chapters treated: The Science of Psychology; The Organism; The Neuro-Terminal Mechanism; Physiology of the Neuron; Stimulation, Adjustment, and Response; Behavior; Conscious Experience; The Senses; The Components of Mental States; Primary and Secondary Mental States; Succession of Mental States; Attitudes; Character and Personality; Organized Mental Life; with an Appendix on The Mind-Body Relation, Mechanism and Purpose, Neural Activity, and The Visual Process.

The Psychology of Nationality and Internationalism. By W. B. PILLSBURY. N. Y., D. Appleton and Co., 1919. 314 p.

The writer's work is more or less suggested by Graham Wallas, and his position is a compromise between that of MacDougall, with his insistence on immediate instinct, and that of Trotter, who explains all social phenomena by the fear of the individual for the social whole, with the consequent dominance of convention. The chief chapters are: The Problem of Nationality; The Nation as a Psychological Unit—Social Instincts; Hate as a Social Force; Nationality in History; Nationality in the Process of Naturalization; The Nation and the Mob Consciousness; The National Mind; The Nation as Ideal; Nationality and the State; and Nationality and Super-Nationality as Expressed in a League of Nations.

An Outline of Abnormal Psychology. By JAMES WINFRED BRIDGES. Columbus, R. G. Adams and Co., 1919. 126 p.

Nothing could be more admirable, timely, or needed than an outline of this kind. Perhaps no two people would have made the same tabulation and classification of subject matter, and some would take exception even to his division of it into three parts: (1) The Abnormal Phenomena; (2) The Mental Syndromes or Symptom-Complexes of Insanity; and (3) The Borderline Diseases: Psychoneuroses and Epilepsies. It would be also easy to quarrel with his bibliography, which indicates that he is warped a little by his partisanship for introspection on the one hand and that he is not acquainted with much of the best recent German literature on some of the topics. This perhaps is the chief defect of such an outline. But after all this is said, while the author's classification of abnormal phenomena is not that to which most adhere now, everyone ought to be thankful for this handbook, which is so timely and so conscientiously wrought out.

Sinnesphysiologische Untersuchungen. By JULIUS PIKLER. Leipzig, Johann Ambrosius Barth, 1917. 515 p.

Here we have an admirable and comprehensive survey of the results of the experiments in sense physiology up to the present time with an attempt to synthetize and balance accounts as to what is known and unknown, valuable and otherwise. It is a work of great value, and we hope to have it more fully reviewed at some future time in these pages.

The Voyage of a Vice-Chancellor. By ARTHUR EVERETT SHIPLEY. N. Y., G. P. Putnam's Sons, 1919.

These are extracts from a private diary the author wrote while on an extensive tour of the United States in 1918 as a member of the British University Mission. It is difficult to see any *raison d'être* for such a book. Most of it consists of jottings calendar-wise, giving crude impressions of his trip to this country, while a chapter ambitiously headed "University Education in the United States" gives the crudest and most superficial of all views. To not a few American educators upon whom the Vice-Chancellor made any impression at all he seemed to carry an English atmosphere so dense and impervious as to make him almost inaccessible to new impressions, a man who loved to talk and did so with an English accent delightful to many, but tedious and prolix. It is significant and ominous to our minds that this much-heralded British commission should have devoted so much time to matters so entirely irrelevant to the purpose of the commission, although, so far as we can see, this trivial book may be one of the chief results of the pedagogic expedition.

The Field of Philosophy. By JOSEPH A. LEIGHTON. Columbus, R. G. Adams and Co., 1919. 485 p. (Second revised and enlarged edition.)

This work, the publisher tells us, "gives a concise, clear and comprehensive account of the principal views of the nature of the world and the human soul, and of the meaning of thought and human life, advanced by thinkers in the chief ages of European and American thought down to the present time." Although comprehensive itself, it "furnishes ample guidance for further study."

The first part, which is historical, is rather full, down to and including Scholasticism, with which it stops. Part II consists of the problems of modern philosophy, with a chapter to each of the following: Reality, Dualism, Materialism, Kant, Spiritualism or Idealism, The One and the Many, Evolution and Teleology, The Self, Concepts of Metaphysics, Epistemology, Criteria of Truth, Values, Philosophy of History, Philosophic Progress and Discipline, and Current Issues in Regard to Consciousness, Intelligence, and Reality.

The Health of the Teacher. By WILLIAM ESTABROOK CHANCELLOR. Chicago, Forbes and Co., 1919. 307 p.

The purpose of this book is to guide teachers in the care of their own health while teaching, which is a serious problem. It means increased strength and efficiency for the day's work, overcoming tendencies to disease, increasing the joy of life, and postponing death. The thirty-eight chapters are divided into parts: (1) Principles of Diagnosis and Cases, and (2) The Rationale of Health Control. Part I includes chapters on The Instinct to be Well, Differential Diagnosis, and on eighteen different troubles with case illustrations of each. Part II is on sleep, diet, exercise, etc., and on the care of the different parts, organs, and functions.

Brightness and Dullness in Children. By HERBERT WOODROW. Philadelphia, J. B. Lippincott Co., 1919. 313 p.

The chief chapters are: The Measurement of Intelligence; Brightness and Dullness; Brains; Physical Defects; Anatomical Age; Pedagogical Age; Simple Mental Processes; Association, Memory, and Attention; Complex Mental Processes; Mental Organization; Heredity; The Organization of Education; and Educational Methods. This book is in the Lippincott Educational Guides series edited by W. F. Russell, Ph.D.

Etude-Critique sur L'Evolution des Idées Relatives à la Nature des Hallucinations Vraies. By RAOUL MOURGUE. Paris, Jouve et Cie., 1919. 66 p.

This somewhat voluminous author writes here rather systematically his conclusions, based largely upon the French writers, that there is great difference in the degree of the hallucinated image and that this is favored by every anatomical reference. He is evidently a rather extreme disciple of Moreau in believing that crepuscular states play a great rôle here. The discussion, on the whole, does not bring us into very close relation with the subject; nor does it suggest much that is new.

Aces for Industry. By RUFUS STEELE. Boston, Houghton-Mifflin Co. 93 p.

This is an indexless, chapterless, and so far as we can see rather aimless but sprightly, gossipy booklet with considerable conversation and plenty of idealism, a book about which it is impossible to form any opinion without reading it all; and the reviewer, after many attempts, can find no incentive to do this. The publisher calls it "an absorbing story of a big new idea in American life," viz., that the chief need of industry is workers with the spirit of the aviator, not content to do a day's work in routine fashion but eager to improve everything.

Aesthetics: A Critical Theory of Art. By HENRY G. HARTMAN. Columbus, R. G. Adams and Co., 1919. 250 p.

The writer here attempts to answer the question, what are the elements of individual interest in painting, music, and poetry? He does not aim so much to increase our appreciation as to correct the old methods of determining substance, origin, and value of art. He does not deem it unitary, and thinks the psychological or subjective factors have been forced into one-sided prominence, so that our art psychology needs reconstruction. The chapters are the following: Beauty and Art; Art and Theory; What is Art? The Substance of Art; Formulas and Methods in Art-Theory; Painting; Poetry; and Music.

Business Inspirations. By CHRISTIAN D. LARSON. N. Y., Thomas Y. Crowell Co., 1919. 163 p.

Of the twenty-seven chapters of this book the typical ones are headed: Follow the High Vision, The Magic of Sincerity, It Pays to Look Well, What Makes Men Great, Giving Your Whole Self, How to Work, Creating the Right Idea, The Will to Do More. It is written in a sort of hortatory way and is related to other books on business somewhat as the work of a good revivalist is related to that of a stated pastor.

Proceedings of the American Medico-Psychological Society. Vol. XXV, June, 1918. Baltimore, American Medico-Psychological Assn., 1918. 397 p.

Prehistoric Villages, Castles, and Towers of Southwestern Colorado. By J. WALTER FEWKES. (Bureau of American Ethnology, Bulletin 70.) Wash., Govt. Printing Office, 1919. 79 p. (33 plates.)

Handbook of Aboriginal American Antiquities. By WILLIAM H. HOLMES. (Bureau of American Ethnology, Bulletin 60, Part I.) Wash., Govt. Printing Office, 1919. 380 p.

PRIZE IN PSYCHOPHYSICS

The prize of $100 offered in 1914 for the best paper on the Availability of Pearson's Formulae for Psychophysics (this JOURNAL, xxv., 148), has been awarded to Dr. Godfrey H. Thomson, Armstrong College, Newcastle-upon-Tyne, for a paper entitled "On the Application of Pearson's Methods of Curve-Fitting to the Problems of Psychophysics, especially to the Data of Urban's Experiments on Lifted Weights: in four parts, together with Part V. On the Use of Compound Curves in the Analysis of Heterogeneous Material, and Part VI. On an Outline of an Attempt to Make a Generalised Psychometric Function."

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SIR THOMAS WRIGHTSON'S THEORY OF HEARING[1]

By E. G. Boring and E. B. Titchener

The Helmholtz theory of audition, in spite of Ebbinghaus' gallant effort to save it, seems to have sunk beyond recovery. The historian of science must always regard it with admiration, since for combined range and detail and directness of correlation it is still without a rival; and indeed, as model of what a theory 'ought' to be, it will probably figure for many years to come—with the more or less emphatic warning that it need not be believed—in textbooks of physiology and psychology. Helmholtz himself, eminent in physiology and yet more eminent in mathematics and physics, was (we used to think) a living guarantee of the rightness of his theory, a physical theory of a mechanical organ: we might mistrust his theory of vision, but we could trust the physiology and physics of the *Tonempfindungen*. Little by little, however, acceptance has given way to doubt; the modern physicist will not be pinned down to Helmholtz' 'nothing but' sympathetic vibration, and objects positively that sympathetic vibration is put out of court by the dimensions and structure of the internal ear; the modern physiologist looks in vain for Helmholtz' resonators; and the modern psychologist, oppressed by the number and complexity of his facts of observation, wishes to clean his slate of theory and to start afresh from a thoroughgoing study of the sense-organ, biological, anatomical and physiological.

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[1] An Enquiry into the Analytical Mechanism of the Internal Ear, by Sir Thomas Wrightson, Bart., Memb. Inst. C. E., with an Appendix on the Anatomy of the Parts Concerned by Arthur Keith, M. D., F. R. S. London, Macmillan & Co., Ltd., 1918. Pp. xi.+155+Appendix (156-254)+9 Plates. See also the account in *Proc. Roy. Soc. Med.*, 12, 1919, Otology Sect., 80-94; and the discussion by Bayliss, Keith, Wrightson, Perrett and Rayleigh, *Nature*, 102, 1918, 124f., 164f., 184f., 225, 263f., 304, 325.

Meantime, of course, theories flourish, and must be considered as they come. The theory set forth in the book before us is that of an engineer, who approaches the ear "as a piece of working machinery." First conceived as long ago as 1876, it has now been wrought out in detail; and the author has sought to clinch its application by calling in the aid of an anatomist. The combination of forces is unusual; the disregard of psychology is—to a psychologist—refreshing; and we begin to read with a hopeful curiosity. May not this frankly teleological attitude toward the sense-organ, as a machine in being, put us all, physiologists and psychologists alike, on the right track?

Whether or not this hope is eventually to be fulfilled, we soon discover that the writer has not troubled to make things easy for us. His style appears at first to be clear and simple, but in fact it is neither the one nor the other. His physical terminology, in particular, is at best misleading and at worst incorrect. The terms pressure, force, power, work, and momentum are used loosely despite their precise physical meanings;[2] and discussion that begins thus inexactly ends, as it must, in positive error.[3] Moreover, the exposition of the theory is annoyingly unsystematic. We find digressions, repetitions that are sometimes almost verbal, and a general absence of the coordination that might have been effected by literary organization or by cross-referencing. Again and again the reader fails to divine the author's purpose; when is he seeking an account of the analytical mechanics of the internal ear, and when the establishment of a theory of hearing? Much of the description belongs only to the former context, and much of the discussion only to the latter, and the resulting confusion has to be cleared up by re-reading.

The burden of comprehension is thus heavier than it need be. We do not desire, however, to make too much of this criticism. The book is a war-book; the preface is dated February, 1917; and the writer may have had scant leisure for polishing his material. The looseness of terminology is a more serious matter. We proceed to show, in a number of instances, that it means a fatal looseness of thought.

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[2] *Cf.*, *e. g.*, the confusion of work and power, pp. 134 f. The reader will find plentiful instances of a like kind.

[3] Consider *e. g.*, the application of the energetic principle of conservation to forces which should be dealt with as equilibrated, and the consequent discovery of a supposititious residual force effective in the cochlea: p. 94, lines 9 ff.

1. In his preliminary analysis of compound wave-forms[4] Wrightson shows that, if the simple wave-forms be represented by straight lines forming a series of V's, the algebraic addition of two simple wave-forms respectively of frequency x and frequency y will give a compound wave-form, which has $(x+y)$ and also $(x-y)$ approximately equidistant crossing-points (intersections of wave-form with horizontal axis). These two periodic series show no significant interrelation. Wrightson attributes to the $(x+y)$ series the summational tone, and to the $(x-y)$ series the differential tone. The correlation is, however, unsatisfactory. It is to be presumed—and Wrightson seems to imply—that similar properties in the wave-form underlie both simple tones and combinational tones. But the tone x is heard for a wave-form that has $2x$ crossing-points (a complete wave crosses the axis twice); and a tone y is heard for a wave with $2y$ crossing-points. If $(x+y)$ be the summational tone, then it might be expected to have $2(x+y)$ crossing-points. The differential, in like manner, should be conditioned upon $2(x-y)$ crossing-points. Wrightson's $(x+y)$ and $(x-y)$ are each of them an octave too low. The $(x+y)$ crossing-points, moreover, represent an average frequency, and not a sum.

2. This discussion of triangular wave-forms is preliminary. We come presently to the compounding of tones of sine-form. Here we learn that, if we regard every crest, hollow, and crossing-point as an " impulse point," we have possessed ourselves of a key to an understanding of the ear's analysis of compound waves. More concretely: if we build up a compound wave-form by the algebraic summation of sinusoidal components, and note in the complex form the positions of all crests, hollows, and crossing-points, we discover that a great many of these critical points are separated by distances which are approximately the same as the wave-length of one component; that many other pairs of points are separated approximately by the wave-length of the other component; and that

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[4] Pp. 12-17; Plate I.

still other points show separations which correspond with the summational tone or the differential tone. This discovery justifies, we are told, the initial assumption that crossing-points, crests, and hollows all function in the same way.

A little reflection, however, raises the question why Wrightson, in these graphical experiments, should wish to find the *wave-lengths* of the components reproduced in the compound. In a simple tone there are two crossing-points, one crest, and one hollow, in every single wave; four impulse points in all. A tone of x vibrations per sec. has $4x$ impulses within the second. It would seem, therefore, that Wrightson should have made his analysis of the compound wave by seeking out recurrences of the quarter wave-length rather than of the wave-length. Whether an analysis with respect to quarter-waves would fail we can not tell. The original curves in the publication of 1907,[5] which unfortunately is not at present available to us, must be analyzed again (as they were originally) by a cut-and-try method.

3. There are more difficulties of the same order, but they appear at higher levels of the argument. We must observe next a fundamental assumption of a different kind.

Wrightson, in his analysis of the hydraulics of the inner ear, ordinarily uses hydrostatic and not hydrodynamic principles. He employs the terms power, work, and momentum; but the dynamic principles that these terms imply do not dominate the discussion. There is no reference to the inertia of the moving parts, although the difference in phase of the curve of air-pressure and the curve of air-displacement[6] is a matter of inertia. Wrightson draws the analogy to the steam-engine as if the work transmitted to the hair-cells were the vehicle of sensation.[7] The engineer's interest in the steam-engine, however, lies ultimately, not in its work, but in its power. Power is rate of doing work; and both the frequency and the amplitude of a tone must be considered if mechanical power is the key to hearing. An engineer determines mean effective pressure by putting a steam-indicator on the steam-cylinder, just as Wrightson wishes might be done with the inner ear;[8] but the engineer does not then forget to measure the speed of the shaft.

The substitution of static for dynamic principles begins in Wrightson's argument when we pass from the sounding air to the mechanical system of the middle and inner ears[9]. Wrightson shows the waves of pressure and displacement separated in the air,—a dynamical separation. He then notes that the stapes moves as the result of air-pressure upon it, and assumes that it will therefore move *with* the air-pressure.[10] In a series of hydrostatic equilibria it would so move; the position of the stapes would be a function of the pressure upon it. But hydrodynamically there would be a lag due to inertia. An imponderable stapes in air would move with the ponderable air and not with the pressure of the air. An actual stapes weighted by a mass of liquid would similarly be out of phase with air-pressure.

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[5] P. 31.

[6] P. 76.

[7] Pp. 94, 134.

[8] Pp. 1, 134 f.

In another passage we read: " As liquid pressure in a closed vessel exerts itself equally in every direction the unit pressure at any one moment which enters the cochlea is not affected by the shape of the vessel." [11] This misstatement is symptomatic of the entire hydraulic account of the inner ear. The law of equal pressures is the hydrostatic law. Engineers make use in the Venturi meter of the fact that pressures are not everywhere equal within a moving liquid, and that the shape of the containing vessel determines the rate of movement and thus the pressure.

The fundamental principle of hydrostatics is the equilibration of forces. Every body is considered in equilibrium and the sum of the forces acting upon it equated to zero. This mode of working is not adequate in hydrodynamics; but it is the mode that Wrightson uses;[12] and it makes difficulty later on.

If it is argued that the masses involved are so minute that their inertia is neglible, we reply that the masses are not disappearingly small and that their rates of motion are very great. Moreover, we must remember that Wrightson's theory stands or falls with the assumption that the ordinary principles of mechanics apply.

4. If we grant the dynamical nature of the problem, we must ask why Wrightson supposes that the movement within the inner ear is a mass-displacement of the lymph and not a transmission of sound-waves, since water does in fact conduct sound. Bayliss has already raised this question.[12a] Wrightson's answer is misleading. He holds that the lymph is " (at

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[9] P. 76.

[10] Fig. 30.

[11] P. 47.

[12] *Cf.* pp. 80-87, esp. 83 ff., 86; 124.

[12a] W. M. Bayliss, *Nature*, 102, 1918, 124, 263.

the minute pressures dealt with) inelastic and incompressible, and will therefore be moved instantaneously in the passages of the cochlea."[13] He quotes Helmholtz as authority.[14] He is especially insistent that a liquid is inelastic.[15]

As a matter of fact, however, liquids are not absolutely incompressible and they are highly elastic. Elasticity and compressibility are *inversely* related;[16] a liquid is highly elastic because it is relatively incompressible. The true answer to Bayliss' question has been given by Rayleigh; the cochlea is so short with respect to the wave-lengths of ordinary audible tones that there could be very little difference of phase between the pressures at the oval window and at the round window, and transmission would therefore be practically instantaneous.[17]

5. We may include as a separate item in this list the fact noted above, that the curve of displacement of the stapes can not be, as Wrightson believes,[18] synchronous with the curve of air-pressure. His assumption that the two are synchronous sets the stage for the difficulties of all the discussion that follows.[19] Here the neglect of the dynamic principle is especially unfortunate because of the critical part played by the conclusion.

6. Thus we come to the most important part of the theory, the demonstration that every complete wave of pressure in the air, consisting of a positive and a negative phase, is correlated with the two positive phases and two negative phases of a pressure-curve within the cochlea. The reader must refer to Wrightson's book for the details of the argument.[20]

If we take Wrightson at his own level of exposition, we are at once entitled to object to the development of Fig. 34. Here (by analogy to Fig. 33) " C" must be the mid-position and " B " the extreme position of the stapes. The generating time-circle is struck with " B " as a center; so that the stapes, in moving from " C " to " B," is shown as starting from rest at " C " and moving faster and faster until it reaches " B," where it changes direction and moves back on the return stroke. Such a movement of the stapes is, however, manifestly impossible. The stapes can not reverse the direction of its movement at maximal speed; it must, like any other object in pendular motion, slow down and pass through rest at the moment of change. Moreover, Wrightson elsewhere notes that it halts in the extreme position.[21] Yet Fig. 34 shows it moving over a maximal space in the last increment of time!

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[13] Pp. vii, 80, 90, 115.

[14] Notes on pp. 56, 74; p. 130; and *Nature,* 102, 184.

[15] Pp. vii, 77, 121, 128, 130.

[16] Elasticity=stress/strain=compressing force/amount of compression. The less the compression produced by a given force the greater the elasticity. *Cf.* A. Winkelmann, *Handbuch der Physik,* I, 1906, 497-503.

[17] *Nature,* 102, 304.

[18] P. 76.

[19] Pp. 76-96.

[20] Pp. 80-84, Fig. 34 and Plate V.

The remedy is simple. The time-circle of Fig. 34 must be struck with its center at "C." (Where else indeed should the center of the generating circle for simple harmonic motion be placed than at the center, "C", of the total displacement?) The stapes, like a swing, moves fastest at its mid-position "C," slows down, and reverses direction at "B." It does not, as Wrightson maintains, stop momentarily at "C" as well as at "B." What, we wonder, has led Wrightson to believe in this arrest of motion at "C"? The examples of reciprocating motion with which the engineer is familiar furnish no ground for the belief. It can not arise in consequence of the invocation of Hooke's law; for it is formulated prior to the mention of Hooke's law, in order that the combined effect of Hooke's law and the twofold arrest may be worked out. Probably the error springs from the assumption that stapedial displacement is an immediate function of pressure (the fifth point mentioned above); for the pressure is zero in the mid-position, and Wrightson seems to think that the resultant velocity must also be zero. He forgets, it appears, that a swing moves fastest in its mid-position, when the effective component of gravity is zero.

If we dissent from Wrightson's reasoning, and consider the stapes as moving fastest in the mid-position, we must strike the generating circle about "C." The curve of elastic resistance, as thus developed, becomes a sine curve and not a versine. If we then take the difference between the imposed pressure (sine curve) and the opposing elastic pressure (now shown to be also a sine curve)[22] we get as the curve of transmitted pressure, not Wrightson's two positive phases followed by two negative phases,[23] but zero pressure. This is exactly the result which could have been foreseen. We are dealing implicitly with hydrostatics and forces in equilibrium. Under Hooke's law and these assumptions the elastic membranes will stretch until they give back a reacting force equal to the impressing force. Thus we demonstrate Newton's third law. Action and reaction are discovered equal!

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[21] *Cf.* pp. 87, 121, 142.

[22] *Cf.* Plate V.

[23] The red curve of Plate V.

It stands to reason that no pressure could be transmitted through the *fenestra ovalis* if there were nothing beyond to press against,—granted always that the pressures due to inertia and change of velocity are, with Wrightson, left out of account. But in point of fact there are elastic pressures arising within the inner ear. Wrightson admits them,[24] but regards them as neglible in comparison with the tensions of the tensor tympani and the stapedius. They are the elastic pressures from Reissner's membrane, the tectorial membrane, the basilar membrane, and the *fenestra rotunda,* all of which are displaced with the stapes. Far from being neglible, they are the determinants of internal pressure within the cochlea, which is of their order of magnitude. The pressure in any canal is the pressure necessary to displace the membranes lying beyond it. In the cochlear duct, the hydrostatic pressure would be atmospheric pressure *plus* the pressures due to the distension of the basilar membrane and round window. In the tympanic canal the pressure would be atmospheric pressure *plus* the pressure from the round window only. In a dynamic system the pressures would depend in addition upon the rate of motion and the inertia of the cochlear lymph. In any case, however, they would be approximately sinusoidal, and they must be accounted for by conditions within the cochlea, and not as a residual left over from the operation of the mechanism of the middle ear.[25]

7. The central feature of the Wrightson theory is this complete cessation of pressure and motion at four points within the complete period.[26] These critical points are points of zero pressure in the doubled cochlear wave, and they are correlated with the crests, hollows, and crossing-points of the air-wave. Thus we get an inkling of the significance of impulse points in Wrightson's analysis of mechanically compounded curves in the earlier part of his book.[27] We saw, however, that his analysis is faulty because the four-fold total wave, instead of the significant quarter wave-length, is laid off among the impulse points of the compound curve. We see here that the mechanical significance of the impulse points fails of establishment. We still can not deny the graphical coincidences of the curves, but we may deny that they have brought us far toward an understanding of hearing.

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[24] Pp. 144, 154.

[25] This whole discussion of a residual pressure which is delivered to the elastic membranes and not taken up by them, so that something remains over for further use, sounds very much as if work or energy were in question. For there is no principle of the conservation of pressure! But the main argument applies to pressure; and if Wrightson shifts his meanings without changing his words, the reader can not be held responsible.

[26] Pp. 87, 121, 124, 142, 196, 212, 218.

[27] Pp. 26-35, 60-71.

8. If the cochlear curve of pressure is not doubled, there is no "subjective octave."[28] "Subjectively," *i. e.* in the cochlea, there are just as many crests and troughs and crossing-points as there are "objectively" at the tympanic membrane. But even if the cochlear curve is doubled, as Wrightson would have it, we should not expect to find, as he does, that both the objective tone and its subjective octave are effective for hearing.[29] Wrightson seems to assert that for the tone m the cochlea receives impulses corresponding both with m and with $2m$. Moreover, the subject is expected to hear both m and $2m$ together; at least both m and $2m$ are held to be simultaneously effective in the production of combinational tones. This argument is untenable both in fact and in logic: one does not hear in a pure tone its octave, and one does not eat one's cake and still have it. If the cochlear determinant of tones is doubled, then it can not still be single.

9. With the subjective octave goes Wrightson's explanation of all differential tones but the first.[30] One of his differential tones, $(2m—n)$, does not exist, to our knowledge, for any other theory. The first differential is supposedly grounded in the nature of the compounded curves; but we have seen that the preliminary discussion indicated a differential tone an octave too low, and that subsequent discussion was based upon an improper unit. Hence there is as yet little promise of a revised theory of differential tones.

10. The summational tone is in no better case. Wrightson does not condition it upon the subjective octave, though he might have done so had he allowed differential tones of the second order.[31] The summational tone goes the way of the first differential tone; the arguments in its favor that we have noted in our first and second paragraphs are not convincing. Even were they final, they would prove embarrassing; since they would indicate that the summational tone is a prominent component of all fusions; whereas, introspectively, the summational tone is weak. Wrightson concludes with an attempt at

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[28] P. 143.

[29] Pp. 147 f.

[30] Pp. 148 f.

[31] *Cf.* W. Wundt: *Physiol. Psych.*, i., 1893, 465.

diagrammatic establishment of the summational tone;[32] but he fails here for the reason that we have given, and for another reason that we must now mention.

11. The final account of the mode of treatment of the cochlear pressure-curves[33] raises new queries. Wrightson obtains a cochlear curve for a compound of two tones by adding algebraically the cochlear curves of both tones The result is not what we should expect (*cf.* Plate VIII); since the sum of the cochlear curves of the separate tones is not what the cochlear curve of the sum of the separate tones would seem to be. Presumably, since compounding actually occurs in the air, we should first compound the sine curves, and then find a cochlear function. Be this as it may, Wrightson now proceeds by the cut-and-try method to lay off between impulse points the wave-lengths of the separate generators, of the differential tones, and of the summational tone; and *he takes as impulse points the crests, hollows, and crossing-points of the cochlear wave.* Up to this point we had thought that the reason for calling the crests, hollows, and crossing-points of the air-wave " impulse points " is their correlation with the points of supposedly zero-pressure of the doubled cochlear wave. Now we are suddenly confronted in a simple wave with *eight* impulse points; for in a single period the cochlear wave can have four crossing-points, two hollows, and two crests. Still, however, this gratuitous doubling of the number of impulse points results in graphic concidences! It is strange that the theory, if we take the diagrams at their face value, should thus insist on proving itself. Moreover, there is something disconcerting about a theory that will come right on any assumption. We ask in bewilderment *what* it is that the theory theorizes,—whether the facts that it is supposed to be concerned with, or some irrelevant character of graphic forms.

It is a pleasure to turn from criticism to appreciation. And if the summary statement of our debt to Wrightson is shorter than the catalogue of objections to his theory, that is only because it may be set down without comment or argument. The points to which we desire to call attention lie for the most part at the level of observation,—naturally, since the conclusions at the higher level of the theory are dependent upon reasoning to which we have been obliged to take exception.

1. We must note in the first place Wrightson's discovery

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[32] Pp. 151 f. and Plate IX.

[33] Pp. 150-152 and Plates VIII, IX.

that the compounding of two periodic V-shaped wave-forms of frequencies x and y gives a compound curve of $(x+y)$ and of $(x-y)$ intermediate and approximately equidistant crossing-points; and that this characteristic of compounding is independent of the phase-relation of the components.[34] Wrightson apparently made his discovery by an inspection of plotted samples, and without having the ultimate form of his theory in mind. The cases presented in Plate I are convincing samples: there seems to be no reason why the validity of the generalization should be doubted. It would be interesting to work over some of the cases accurately, by analytics or graphics, in order to determine the mean variation of the points from exact periodicity and thus to arrive at a definition of the approximation.[35] The meaning of the relation, as we have seen, is not obvious; but it is a novel idea that compound waves may be understood by a study of the occurrence of critical points within them.

2. Wrightson makes a similar discovery in the compounded sine curves, plotted by the ohmograph,[36] the invention of which is in itself a contribution to acoustics. If the hollows, crests, and points of crossing are taken as critical points, the distribution of these critical points shows simple relationships to the component wave-lengths and to the wave-lengths corresponding with the sum and difference of the component frequencies.[37] Again we are reminded of the possibility that we may solve the problem of hearing by reference to a periodic recurrence of critical points, rather than by reference to the total wave-form or the wave-form of the components. Here the analogy of the siren, where the ratio of size of hole to space between holes (wave-form) is much less important than the rate of recurrence, is peculiarly suggestive.[38]

3. The conception of the conditioning of tone on the periodic recurrence of momentary impulses raises the question of the effect of aperiodic impulses. Wrightson finds critical points that are 'aperiodic,' except that they recur at long intervals; for a wave-form compounded of two commensurable components must eventually repeat itself and therefore every point within itself. He regards these additional points of long period as the correlates of noise,[39] and remarks that the

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[34] Pp. 12-17 and Plate I.

[35] *Cf.* p. 30.

[36] Pp. 24 f. and Plate III.

[37] Pp. 26-30, 60-77.

[38] Pp. 6-8.

[39] P. 35.

poorer fusions, where the 'aperiodic' points occur more frequently, are the noisier. It is true that the poorer fusions have upon them a greater roughness or *vibrato,* and it is possible that Wrightson provides a key to this introspective fact. If one can hear in a major seventh two tones *and* a *vibrato,* any theory that provides three elements in the compound wave is so far illuminating. We may point out, further, that Wrightson's characterization of the 'aperiodic' recurrences as noise agrees with Jaensch's view, and that both Jaensch's and Wrightson's noises are periodic in a long period.[40]

As with the graphics of wave-form, so with the mechanics of the ear, Wrightson accomplishes more at the level of fact than at that of interpretation. His text and Keith's appendix are full of descriptive statements concerning the structure and mechanics of the middle and internal ears that are necessarily pertinent to any theory and are themselves the material of science. They can not all be listed separately, but we may indicate their general nature.

4. The kinematic analysis of the middle ear, and its conception as a mechanical system slung between opposing elastic tensions and operating under Hooke's law, at once provide valuable data regarding the actual dimensions and the probable displacements of parts.

5. The analysis of the inner ear is similarly informative. It is of exceptional value that we should know the hinge-like motion of the stapes,[41] the dimensions of the cochlea, the relative velocities of the lymph at various points, the size and relations of the various members of the organ of Corti at different regions of the cochlea, the probable displacement of the basilar membrane, the hinging of the arch of Corti, the relation of the tectorial membrane to the hairs, the kinematics of the organ of Corti that translates basilar displacement into transverse bending of the hairs,[42], the approximate amount of this displacement,[43] the elastic resistance of the tectorial membrane,[44] the lack of correspondence between the hair cells, the tectorial fibres and basilar fibres,[45] the peculiar properties of the vascular body,[46] and the other details of like

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[40] Wrightson, pp. 21 f.; E. R. Jaensch, *Zeitschr. f. Sinnesphysiol.* **47,** 1913, 256 ff.

[41] Pp. 42 ff.

[42] Pp. 95-127.

[43] Pp. 189 ff.

[44] P. 209.

[45] P. 247.

[46] Pp. 181 f.

nature. The study of the inner ear of the sparrow shows that certain parts of the human ear are in all probability inessential to a discriminative hearing of tones.[47] The suggestion that the broadening of the basilar membrane toward the tip of the cochlea is a compensation for its less favorable situation is also worthy of notice.

All these facts are of great value, even though they have not as yet eventuated in a tenable theory. They belong to the observational half of science, and the primary half. If we had had more of them and fewer theories, we should know more; or rather, if we had had more of them, we should have fewer theories. Wrightson has given us no new theory, that shall "win the assent and support of physiologists, psychologists, physicists and musicians;" so far as that goes, the Helmholtz theory may continue to rest as comfortably as its intrinsic condition permits. But he has given us more facts than we have had for many a day.[48] And he has thus begun his scientific work where it should be begun,—with observation.

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[47] P. 228.

[48] Let it not be forgotten that Wrightson, when he lacked facts, sought out a competent preparator to make for him three series of microscopical sections of the human ear. See Appendix, p. 157.

ON THE NON-VISUAL PERCEPTION OF THE LENGTH OF LIFTED RODS[1]

By Louis B. Hoisington

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I. Problem

This study has been undertaken as a preliminary to the study of what is known as 'eccentric projection.' We have sought to discover and to analyze the perception of length which arises when light rods, held horizontally, are lifted a moderate distance in the vertical dimension. We have also been successful in synthetizing this perception of length from the data of analysis. We believe that the ground is thus prepared, logically and psychologically, for an attack upon the more complex perception which appears when a rod, held in the hand, is brought into contact with wall or floor.[2]

II. Observers

The observers were Mr. P. Cavanaugh (Ca), Miss C. Comstock (Co), Dr. C. L. Friedline (F), Dr. J. M. Gleason (G), the writer (H), Mr.

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[1] From the Psychological Laboratory of Cornell University.

[2] Since the references to this type of experience are concerned almost exclusively with 'eccentric projection,' we omit an historical review. See, however, H. Lotze, *Microcosmus* (tr. E. Hamilton and E. E. C. Jones), I, 1885, 586 ff.; esp. on the perception of added height due to head and foot coverings, 592 f.

H. S. Liddell (L), Mr. E. S. Neal (N), Miss A. K. Sullivan (S), and Dr. M. J. Zigler (Z).[3] All were graduate students or instructors in the department of psychology. They varied considerably in training: Ca and N were unpractised; Co, L, S and Z were but moderately experienced; F, G and H were trained observers.

H and Z served throughout; F, G and N during the first half of the work; L and S during the second half; Ca and Co during the last quarter only. The observers, except H, worked without knowledge of problem or conditions. The plan of the investigation called for observation under a long series of but slightly varying conditions, and thus made heavy demands upon the patience and skill of the observers. The demands, as the results show, were adequately met.

III. Procedure and Apparatus

The method of procedure was twofold: quantitative, by the method of Constant Stimulus Differences, and qualitative, by way of introspective report. In the method of constant stimulus differences, following Urban, we used five comparative stimuli: objective equality, and two steps above and below; except in one check-series, where all rods were objectively equal.

The experimental work divides itself into four principal sections—A, B, C and F—in accordance with the nature of the rods used or the end sought. In section A (10 series) we used rods, planed from seasoned poplar wood, one-half inch in diameter, which averaged 1 gr. in weight per 2.85 cm. of length. The handles, turned from white pine,, were quite heavy relatively to the weight of the rods. In section B (7 series) the rods, planed from seasoned oak, were three-quarters of an inch in diameter, and averaged 1 gr. in weight to every 0.52 cm. of length. The handles, also turned from white pine, were both absolutely and relatively much lighter than those used in section A. In section C (I series) we aroused the experience of length without recourse to rods, synthetically. In section F (6 series) we sought to determine more exactly the effective factors in the perception of the length of lifted rods. To this end we used the lighter rods with a new set of lighter handles. We added weights to the rods, to make them conform to certain mathematical requirements; hence they were about double the weight of the rods of section A. We give the data for the physical properties of the rods in Table I.[4]

This table shows three characteristic ways in which rods may differ: length, weight and center of mass. All three moments may differ for every rod in a set; or any one moment may be kept constant for every rod, the other two varying; or two may be constant for every rod in the set, the other varying. We thus obtain seven different combinations. Use of the same combinations with the light and the heavy rods gave fourteen series; the first seven of section A and all of section B.[5] In addition, we ran a control-series (A VIII), in which all the rods were equal; a practice-series (A IX), which was

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[3] Thanks are due to Miss M. Kincaid and to Dr. C. W. Bock, who acted as experimenters for H.

[4] The handles for section A were 15.4 cm. long and weighed 70 gr.; for sections B and D, they were 15 cm. long and weighed 30 gr.; for sections E and F they were 10 cm. long and weighed 30 gr. The centers of mass in series A III lay 35, 33.5, 31, 29.5, and 27 mm. back from the front end of the handle.

TABLE I.

Series	Total Length of Rod and Handle	Center of Mass from Handle	Weight of Rod and Handle
A I	74, 79.7, 85.4, 91.1, 96.8 cm.	11, 23, 35, 47, 59 mm.	96, 98, 100, 102, 104 gr.
A II	Same as A I.	Same as A I.	All 104 gr.
A III	All 22.9 cm.	See foot note	Same as A I.
A IV	Same as A I.	All 59 mm.	Same as A I.
A V	Same as A I.	All 59 mm.	All 104 gr.
A VI	All 85.5 cm.	Same as A I.	All 100 gr.
A VII	All 85.5 cm.	All 35 mm.	Same as A I.
A IX	Same as A I.	Same as A I.	Same as A I.
A X	Same as A I.	Same as A I.	Same as A I.
B I	85, 87, 89, 91, 93 cm.	222, 233, 244. 255, 266 mm.	192, 196, 200, 204, 208 gr.
B II	Same as B I.	Same as B I	All 208 gr.
B IIIa	All 25.5 cm.	21, 23, 25, 27, 29 mm.	Same as B I.
B IIIb	All 89 cm.	Same as B I.	Same as B I.
B IV	Same as B I.	All 244 mm.	Same as B I.
B V	Same as B I.	All 244 mm.	All 208 gr.
B VI	All 89 cm.	Same as B I.	All 200 gr.
B VII	All 89 cm.	All 244 mm.	Same as B I.
C I	Synthetic Series.		
D I	Same as B I in all respects. Wt. judgments.		
E I	All 82.5 cm.	All 250 mm.	60, 80, 100, 120, 140 gr.
F I	All 82.5 cm.	74, 76.9, 80, 83.2, 86.7 mm.	208, 204, 200, 196, 192 gr.
F II	All 82.5 cm.	206, 222, 240, 261, 284 mm.	111.6, 105.85, 100, 94, 87.9
F III	All 82.5 cm.	Same as F II.	116, 108, 100, 92.2, 87.9 gr.
F IV	All 82.5 cm.	202, 220, 240, 262, 285 mm.	All 100 gr.
F V	All 82.5 cm.	Same as F IV.	113.5, 106.6, 100, 94, 87.6
F VI	All 82.5 cm.	Same as F IV.	119, 109, 100, 91.7, 84.2 gr.

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[5] The 7 series of section B duplicate the corresponding series of section A in the ordering of the three physical moments.

a repetition of A I, at the end of the year's work; a passive series (A X), in which the support of the rod, after *O* had grasped the handle loosely, fell away gently; the synthetic series (C I); a weight-series, in which the *O*'s judged the comparative weights of the rods; two short series in which a wooden 'spoon,' securely bound to the arm, served as artificial thumb or forefinger to take up the 'kick' or the 'pressure' as the case demanded; and a series (E I) in which center of mass and length were the same for every rod, but weight varied very considerably. Lastly, we ran a somewhat irregular lot of series (Section F) in the hope, as before stated, of finding some further cue to the analysis of the perception. We worked out the weight and the center of mass that would give us a desired ratio of 'kick' to 'pressure,' and increased this ratio by relatively equal amounts for every one of the five variable rods (F I and II). In this way we hoped to find an optimal setting of the rods—i. e., that combination of physical moments which should give the smallest D. L., or whatever corresponds with that determination. We also (F III, V and VI) placed weights on the rods at such points that the 'kick' or the 'pressure' remained constant for every rod in the set.[6] In F III the steps were relatively equal increments of the variable pressure. Finally (F IV, V and VI) we took for our steps relatively equal increments of center of mass. In sections A, B, D and E the steps were absolutely equal units of weight, of length or of center of mass. There were, then, one, two or three sets of equal but disparate increments, acocrding to the number of variable factors for the given set of rods.

In an experimental group 20 pairs of rods were presented in a haphazard order, in which every one of the variables occurred an equal number of times. On the completion of a group, *O* rested for a few minutes. The number of groups completed in an hour of observation varied from three to five. Fifty judgments in all were made with every variable in every series, except the first in A and B, when for the sake of practice the number was increased to 100.

The time elapsing between presentations of the rods was, at first, regulated by a soundless metronome. The use of the metronome was discontinued after *E* had become practised. Six seconds intervened between the giving of the judgment and the presentation of the first rod of the next pair. We found that, with this interval, the tendency for the impression of the last rod of a preceding pair to carry over to the first of the succeeding pair was almost negligible. The second rod was taken up by *O* as soon as he was able after putting the first rod down. The time elapsing between the lifting of the second rod and the giving of the judgment varied considerably. The *O*'s were, of course, urged to give immediate judgments; if they reported an attitude of doubt or uncertainty, the pair was presented again at a later time. The time for lifting the rods and making the judgment, when the report was immediately rendered, averaged about 10 sec. Some *O*'s, however, were slow and deliberate in the act of lifting, while others made the lift very quickly.

We employed the two time-orders with equal frequency but in no regular sequence, so that any effects of warming-up or of practice were equally distributed. *O* was not informed as to the time-order in any series. We found that, so long as he did not take a positive set or attitude toward time-order, there was complete uniformity in results. Some *O*'s showed a tendency to judge the second rod as 'longer' or as 'shorter' with a resulting difference in distribution for the two time-orders.[7] The results given are the averages for the two time-orders.

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[6] Throughout this study we use the term 'kick' to designate the pressure-sensation at the base of the thumb stimulated by the up-thrust of the back end of the handle, and the term 'pressure' to designate the sensation in the forefinger stimulated by the down-pull of the outer end of the rod.

Since it was advisable that *O* should take all rods from the same place, we made a long narrow frame which slid back and forth with but little noise over round metal bearings and between greased guides, and placed it on a low table. We cut shallow notches with sloping sides 6 in. apart in the sidepieces of the frame, and faced the top of the notches with thin felt. The rods, placed in the notches, lay across the frame in the horizontal position with the handles projecting over the edge of the table toward *O*. Thus *O* took all rods from the same position, relatively to himself, since a small movement of the frame, made by *E*, brought the desired rod directly in front of him. We were able by means of this device to avoid a space-error.

The *O*'s sat blindfold during the observations. A counterbalanced screen left only the handles of the rods exposed. This screen remained down, so that the rods were completely covered, whenever the blind was removed. *E* marked the position of the chair for every *O*, so that he should sit, always with the chair in the same position, at full arm's length from the handles. *O* deposited every rod, as lifted, in a cloth-lined tray placed just above and to the right of the rod when in position to be taken up. This tray was shorter than the rods, except for series A III and B IIIa; hence *O* could get no cue to length when he put the rod down.

For the regular quantitative series the written instructions handed to the observers were: "You will be given two rods in succession. You are to raise them in the horizontal position, using a whole-arm movement and a loose grip upon the handle. You are to judge the length of the second rod in terms of the first; that is, you are to judge the second rod as longer than, equal to, or shorter than the first." For the introspective reports we took separate short series at the end of the regular groups. The additional instructions ran as follows: "You will make a full psychological report of the processes set up in the two experiences."

E's task was threefold: he shifted the sliding frame back and forth; he returned the rods from the tray to the frame; and he recorded the judgments and reports of *O*.

IV. RESULTS

Our criterion for the adequacy of the perception of length is the ability to make comparative judgments. If the perception changes in degree with variation of one or more of the physical moments of the stimuli, we shall expect a high percentage of ' shorter ' judgments at the one extreme and a small percentage at the other, provided that the extremes have been well chosen. The opposite will be true of the ' longer ' judgments. If, on the other hand, certain of the physical moments are not adequate stimuli to the perception, or affect it only indirectly through association and reflection, then, with these moments the sole variables, we shall expect a more irregular distribution of the comparative judgments; one with low extremes for either kind of judgment and with inversions, one with a probable excess of the ' equal ' judgment. This is what we found. A certain factor (center of mass), when varied, gave typical cross-curves with low intersections; the other factors (length and weight) as variables gave flat curves with inversions and delayed responses.

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[7] On two different days, owing to a previous discussion with *E*, H assumed, without intending to do so, a time-order set. The results were comparable with those on other days if the set corresponded with the time-order given; if not, the results showed the discrepancy. In both cases *O* took the normal discriminative length-attitude at the next sitting. Following accepted practice, it would seem allowable, if *O* tends to note the time-order used, to tell him what the order is. But it appears best to say nothing about it, and to hold *O* to the attitude set up by the instructions.

The distributions of the comparative judgments for all *O*'s with the different arrangements of the physical moments (see Table I) are given in Figs. 1 to 26, where the abscissae represent the five variables, and the ordinates show the percentage that the number of judgments in each category (longer and shorter) formed of the total number of judgments given.

Length

It is clear that length, in and of itself, cannot affect the rod as a stimulus-object. It is impossible that length alone, apart from its usual connection with center of mass and weight, can affect a rod which functions as a lever. Center of mass and weight are the factors that enter into the computation of the moments of force. The pendular period of a rod is likewise dependent upon its center of mass. It is only in so far as air resists the motion of a rod that greater or lesser length, presenting a more or less extended surface, can be effective in experience. And it is out of the question that such a difference should be effective in the present experiment. In a trial synthetizing apparatus we used, without effect, a rod considerably longer than any of those of the regular series.

We, nevertheless, let length take its turn, along with the other two moments, at varying and remaining constant. The results (Figs. 5 and 15) show that, when length is the only variable, accurate judgments of difference in the length of the rods are impossible. The curves for all *O*'s are flat and irregular. These results should be compared with those obtained when all the rods in the set were equal (Fig. 8).

It must be remembered that, since weight and a center of mass out beyond the hand were present, the perception of length as such was possible and took place. The introspective reports can give us nothing new on this point.

Weight

Since we have ruled out length as a contributing factor to the length-perception, we are free to examine the series in which weight and length were both variables as well as the series in which weight alone varied (see Figs. 4, 7, 14 and 17). Scrutiny of these series shows that the influence of differences of weight upon judgments of comparative length is a complicated matter.

The adjustment of the physical moments in series A IV (Fig. 4) gave a rather large per cent. of 'shorter' judgments for the lightest rod; the per cents. for Z and N are 66 and 65 respectively. All *O*'s except F gave fewer judgments of 'longer' for this series when comparing the heaviest rod with the normal; F gave the same number for 'shorter' and 'longer' at the extremes.[8] The general flatness of the curves together with the inversions shows that here was no sufficient basis for judgments of comparative length. The preponderance of the judgment 'shorter' seemed to have, in large part, a temporal basis. Two time-factors were noted by the *O*'s; the one had to do with the quickness or slowness with which the pressure came on initially, the other, with the 'liveliness' or 'deadness' of the pressure. It was the former, according to the introspections, which came in most frequently in this series. The usual muscular innervation was greater than that required for lifting the light rod. As a consequence the rod came up with surprising suddenness. In almost every such case *O* judged the rod as shorter than the standard. The lightness of the rod came, readily enough, to carry an assigned meaning of length, in the same way that the 'secondary criteria' carry meanings in the domain of visual space-perception. It is to be doubted, however, whether the length-meaning was fully realized. It is much more probable that weight was the first item of perception immediately translated into length-terms; it set off the length-meaning without the full perception of length.

We turn now to the results of series A VII (Fig. 7), where there obtains a greater practice effect (for the effect of prac-

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[8] Owing to a previous injury to the hand and wrist, F lifted the rods in a different manner from the other *O*'s. Her experience differed accordingly. Wrist-torsion made up a large part of the experience.

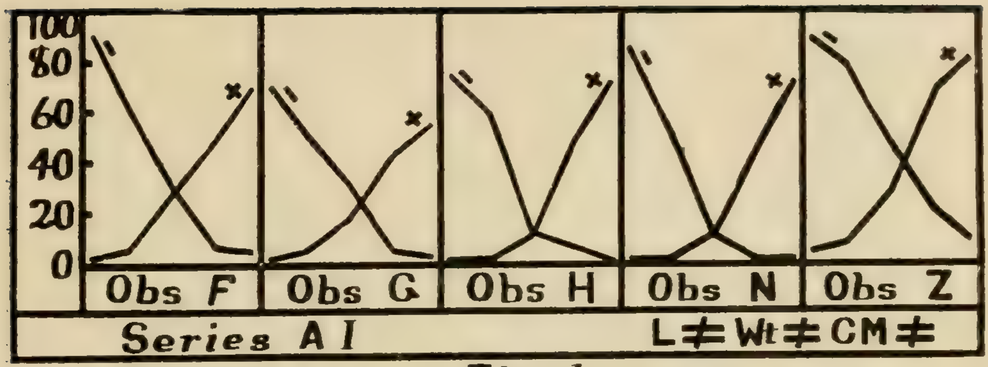


Fig 1

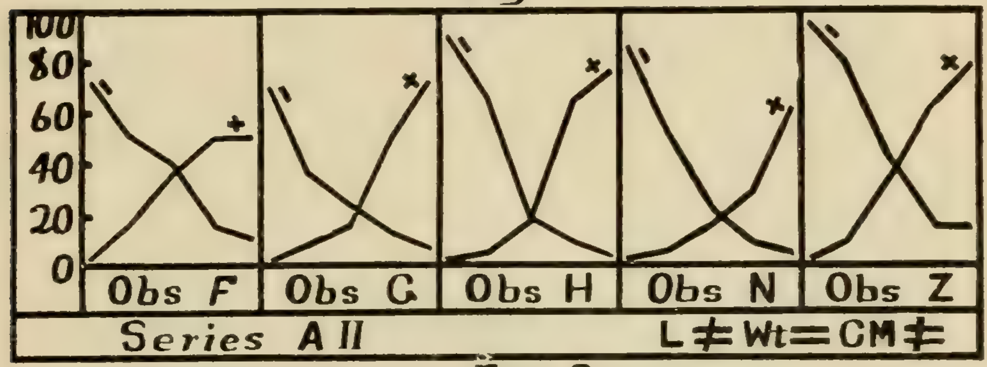


Fig 2

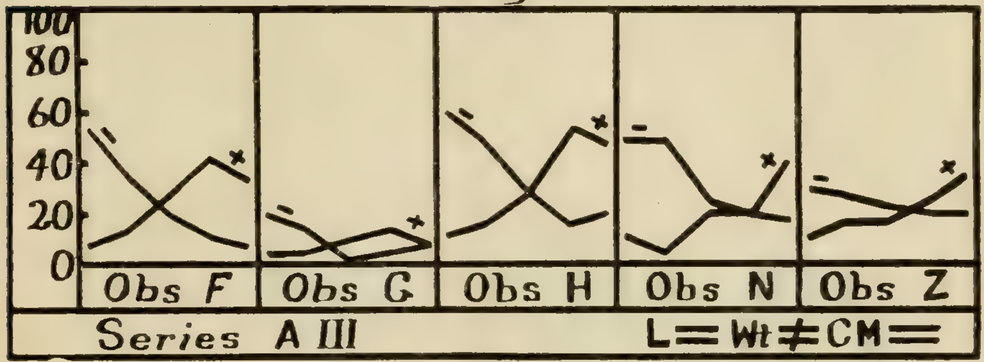


Fig 3

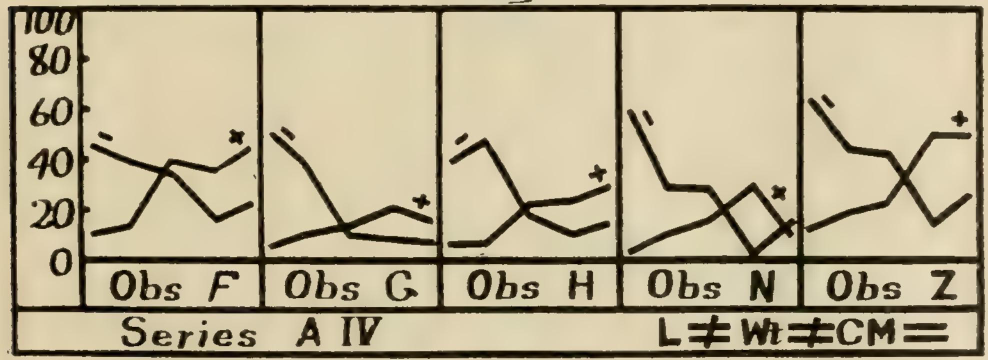


Fig 4

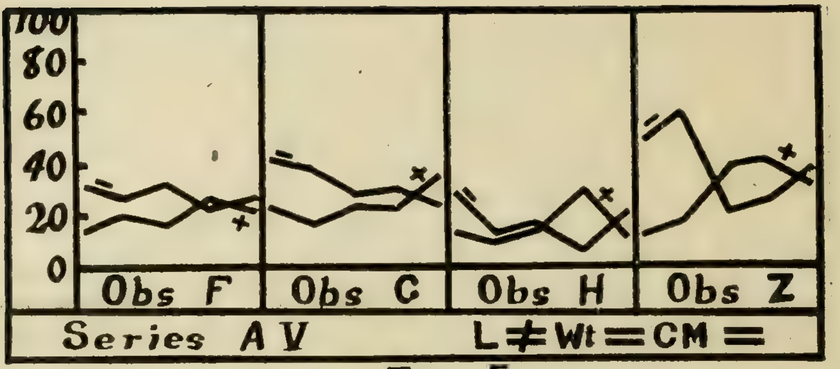


Fig 5

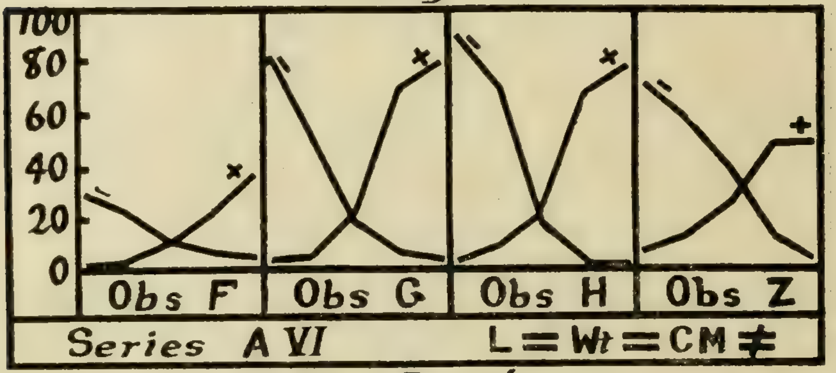


Fig 6

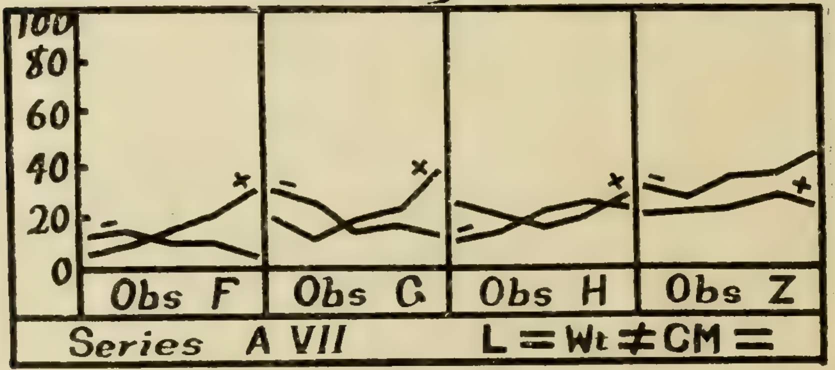


Fig 7

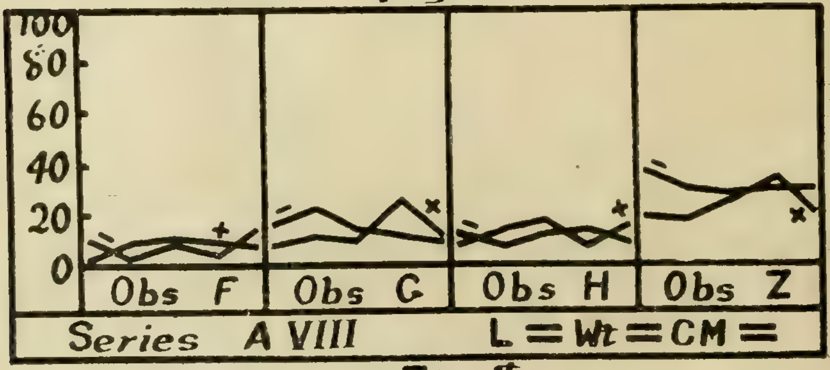


Fig 8

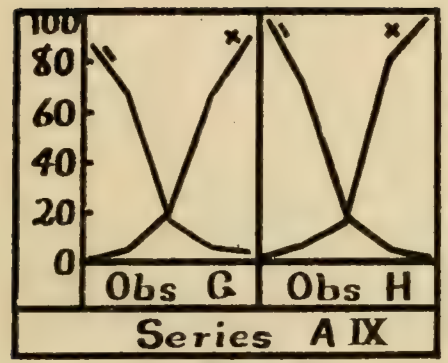


Fig 9

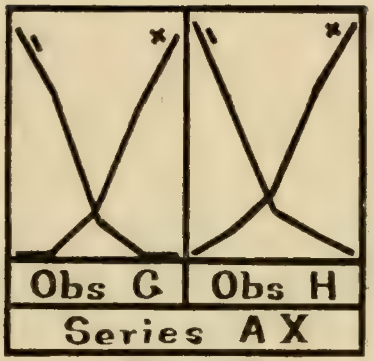


Fig 10

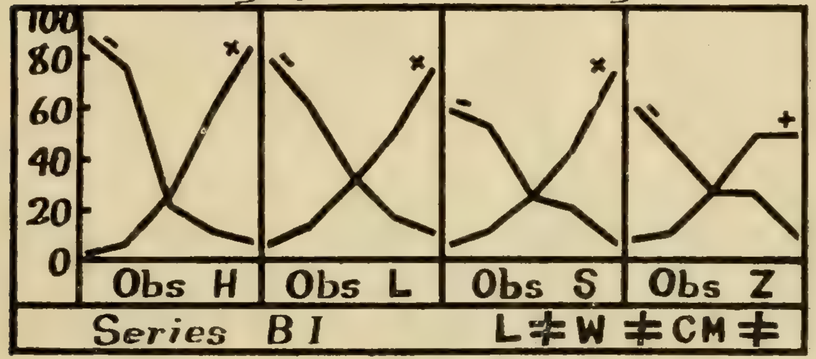


Fig 11

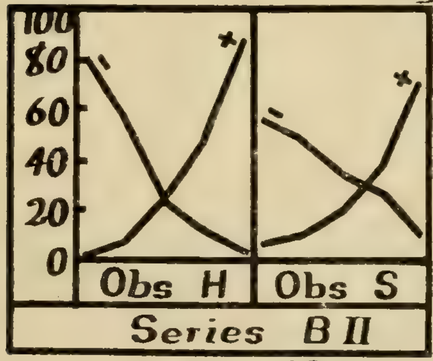
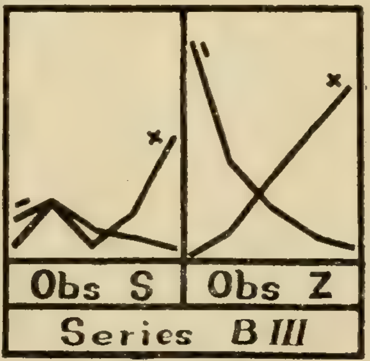


Fig 12



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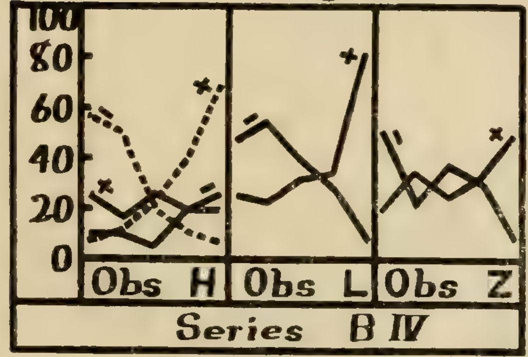

Fig 14

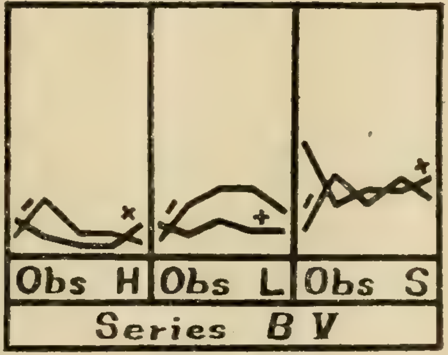

Fig 15

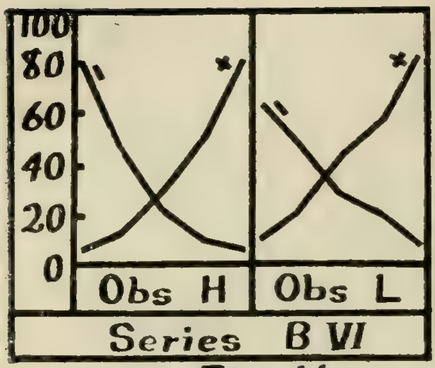

Fig 16

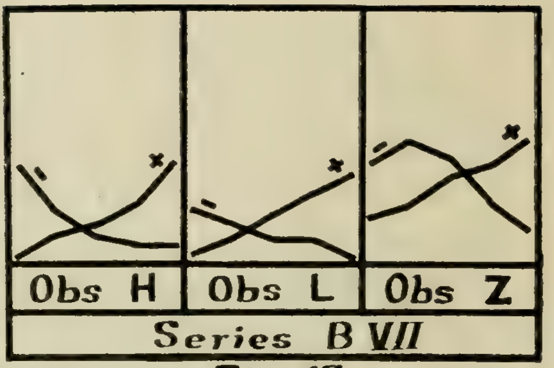

Fig 17

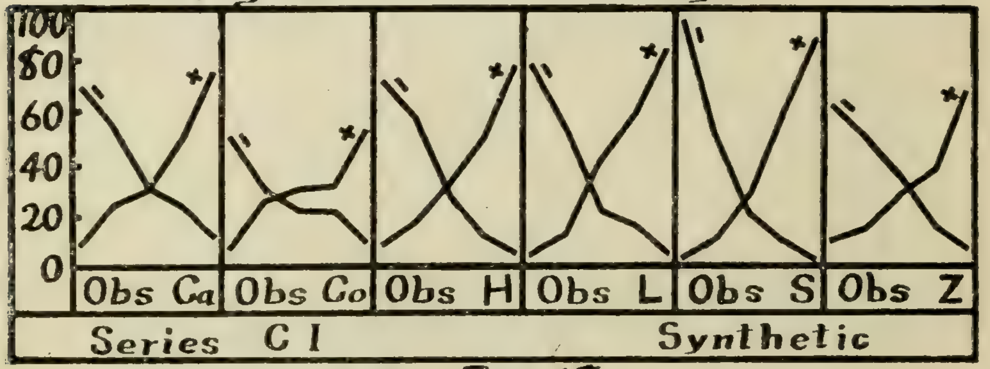

Fig 18

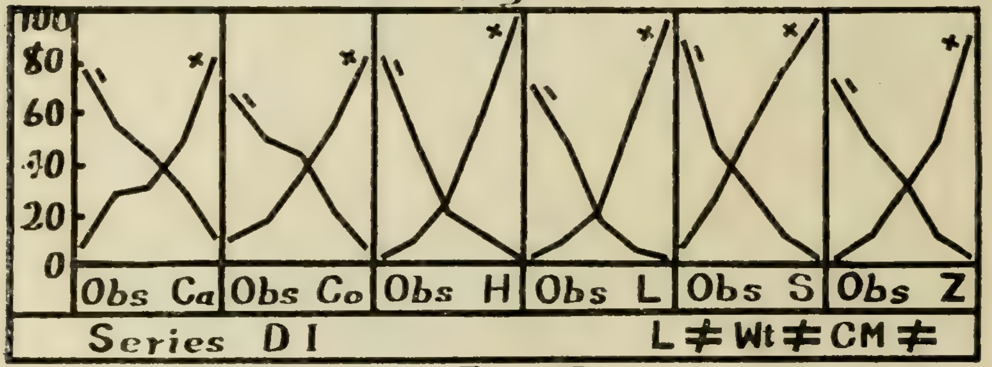

Fig 19

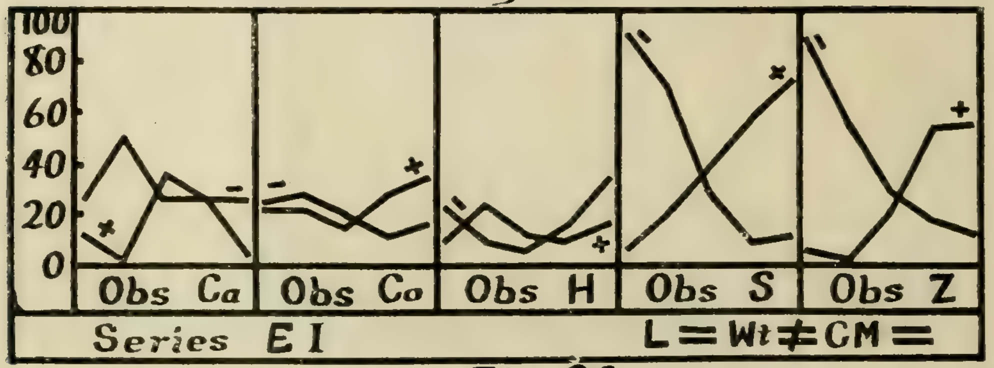

Fig 20

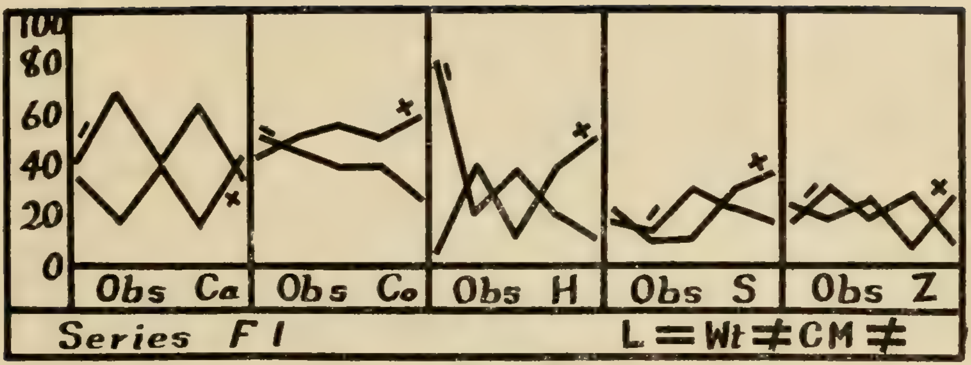

Fig 21

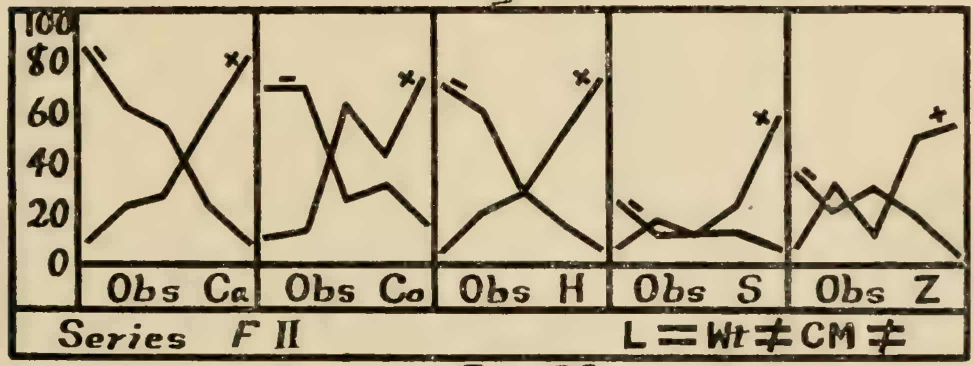

Fig 22

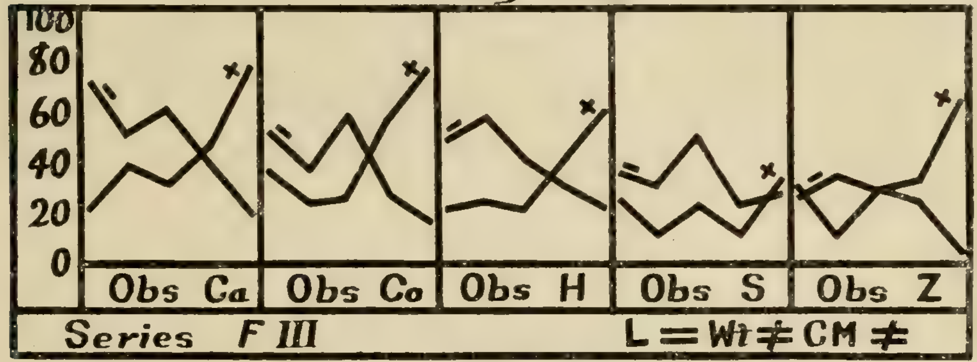

Fig 23

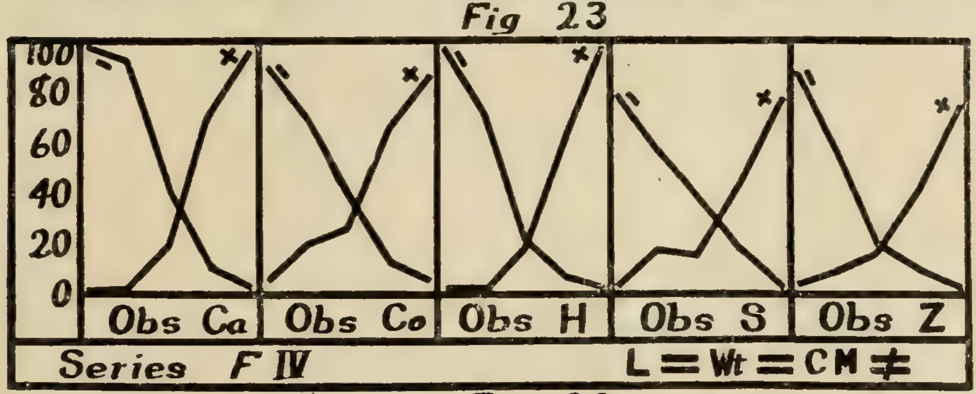

Fig 24

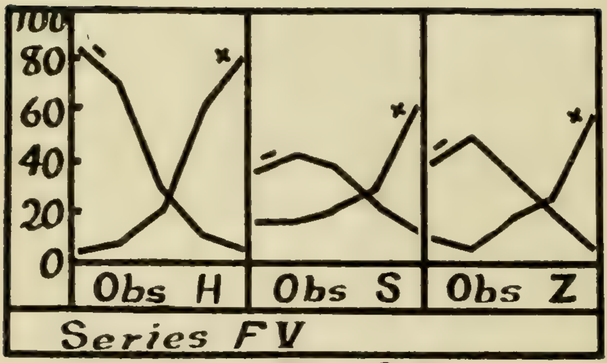

Fig 25

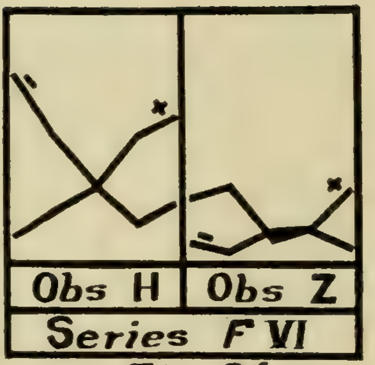

Fig 26

tice cf. Figs. 1 and 9) and where weight is the only incremented factor. The curves are flat; and there are inversions of the first order in every one save the curve for the 'longer' judgments given by F. It is obvious that there were, in this series, no sufficient sensory data for the differentiation of perceptions of length; the weight-increments, which were the same as those used in the preceding series, were not vital for the experience. Hence they cannot be held responsible for the results obtained earlier.

In spite of these curves, however, we believe that it would be a mistake summarily to dismiss the moment of weight as bearing no relation to length non-visually perceived. In Fig. 14, with weight and length varying, and in Fig. 17, with weight alone receiving an increment and a decrement, we have curves showing a tendency toward correlation between objective weight and perception of length; the lighter rods come to the *O*'s as 'short,' the heavy as 'long.' The tendency is especially striking for H in both series and for L with the heaviest rod in series B IV.[9]

This relationship finds its explanation in a gradual shift of attitude on the part of the *O*'s. They came, by degrees, to observe a single aspect, the intensive change, in the total complex (since this was the only variable present), and to report differences of intensity as differences of length. They were, naturally, determined in the direction of finding a difference; the judgment 'equal' did not mean, in many cases, the perception of equality, but the perception of not-different. It may be, too, that the introduction of the introspective series, necessitating the direction of attention upon the sensory experience, made for the translation of sensory cues into the meaning of length. We believe that, whether these series had been introduced or not, the *O*'s would have sought out some sensory cue within the complex as a means for making comparisons. Such a cue could never be adequate to the full perception of length, since this has as its primary condition not a simple attribute, inhering in a single sensation, but a ratio of intensities.

In this dilemma of having to make a judgment of difference, under the determination for differences of length, upon an inadequate sensory basis, the *O*'s seized upon the outstanding feature in the shifting complex mass, and forced that into service. Instead of apprehending the two intensities in relation, which is impossible except as they integrate in the perception of length, the *O*'s took the intensity of the 'kick' or of the 'pressure,' absolutely or in relation, as immediate data for judgment; the intensity of either or both of these pressures is affected by a change in weight as well as by a change in center of mass. The above results, accordingly, find an explanation which at the same time makes clear the true part played by weight in the judgment of length.

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[9] The two curves shown for H in Fig. 14 represent an earlier (dotted line) and later (full line) performance of these experiments. The change of result with shift of attitude is noteworthy.

This shift of attitude is precisely what we might expect to take place; it is in keeping with all that we know concerning the 'natural' life-history of perceptions. The tendency is that, sooner or later, some aspect of a complex experience comes to stand for the whole; that a fragment, tag or remnant of the full sensory complement is effective to arouse and carry the meaning, fully maintained, that once accrued to the total process-complex.

But there is other evidence against the case for weight. In the experiments in which artificial wooden 'spoons' were used to receive the 'kick' in the one case and the 'pressure' in the other, the differences in the weights of the rods were not sufficient to call forth differences in the perception of length; in fact, the perception of length did not appear at all. And in series A III, where the center of mass was very near the forefinger (15-25 mm.) and varied by increments of only 1.5 mm., the perception of length was frequently lacking; the distributions of 'shorter' and 'longer' judgments (Fig. 3) shows flat curves with inversions of the first order. The more regular distributions of F and H are explicable.

F's once injured wrist necessitated a peculiar manner of taking the rods, and a dependence upon wrist-torsion for making the judgments. With a rod set as a lever in the hand, where the forefinger and the base of the thumb act as fulcrum and weight respectively, the weight or force is adequate to produce a torque in the wrist. A failure of F to render judgments of difference in this case would have proved perplexing; just as her objectively poorer results in A II were to be expected. H, unable for the most part to get the perception of length and, when it was present, unable to make judgments of difference, resorted to an individual way of taking the handles. Instead of employing the whole hand, he took the handle in such a way that in front it stimulated the forefinger alone, and at the back a very small area at the base of the thumb. By thus limiting the stimulated parts with the 'kick' and the 'pressure' (so to say) concentrated, he was enabled to make judgments. It is clear that one of two things must have happened; either H singled out for observation the intensive changes of a single sensation; or the enhancing effect of the smaller area made smaller total-complex differences capable of carrying meanings otherwise unattainable. The former alternative accords better with the known facts. Although, therefore, H was set to report differences of length, yet by a process of substitution the inequality of intensity of the single sensation in the total complex came to touch off, directly enough for judgment, the meaning of difference of length.

Finally, series F I, F II, F III, F V and F VI demand a word of explanation. In these series the heaviest rod had its center of mass least far out and conversely, so that the two factors, weight and center of mass, ran counter to each other; they stood in an inverse ratio for the five variable rods taken in order. This result was secured by an addition to, and shifting of, the weight of the rods, in order to make them conform to certain requirements. In each one of these series either the 'kick' or the 'pressure' was the same for every rod, while the other factor increased in amount from rod number one to rod number five; the increments in series F I and F II yielded relatively equal decrements in the ratio of 'kick' to 'pressure;' in series F III gave relatively equal increments in the pressure itself; and in series F V and F VI gave relatively equal increments between the center of mass and the hand.[10] The results show marked differences between *O*'s: for some, the discrepancies of weight and center of mass cut across one another, so that the effect of the one practically nullified that of the other; for others, the differences of weight determined the judgments of length; for still others, the judgments of length reflected but slightly the opposing differences of weight. The same contradiction in results came out in series E, where we have very large increments of weight but equal centers of mass. Co and H gave hardly anything but equal judgments; S and Z gave judgments which correlate highly with weight. The fact that the judgments of two *O*'s do not correlate with weight is conclusive; the two perceptions are of different kind and, under constancy of determination, the one modifies the other but slightly even though they are at variance. The difference is that between an experience in which a cue touches off the meaning and one in which a perception comes to full realization.

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[10] In making our computations, we disregarded the facts that the pressure falls upon an area and not upon a point, and that the difference in the size of the hands of the observers somewhat changed the absolute values.

The following reports[11] taken from a much larger number illustrate *O*'s experience when set to judge differences of length by stimuli varying either in weight only or in weight and length. They supply twofold evidence for the position that differences of weight gave the perception of differences of intensity, and did not give rise to a true perception of difference of length. They show, on the one hand, that differences of weight were noted with contrary judgments of length; and, on the other hand, that differential judgments of length were touched off by differences of perceived weight,—the point being that the *O*'s could say that the judgment was one of difference of weight and not one of difference of length. They also afford proof of the statement that the arrangement of pressures within the pattern was identical for rods of different weight, the only difference being that of absolute intensity. The *O*'s evidently took the attitude of reporting the experiences upon which they based their judgment, although the instructions do not call for such an attitude. The reports are not, in all cases, rendered in strictly attributive terms; there can, however, be no mistaking their import.

Series A III: (3—5+). "There was present this time the notion of something out there beyond the handle. Can make weight-judgment always. The difference from stimulus to stimulus seems to be one of degree rather than one of pattern. This time a foundationless inference on basis of weight" (G). (3—5=) "Perception of greater weight with the second rather obscure. The clear thing was the pressure distribution in the hand, which was the same in both cases" (G). (5—3=) "Pressure-pattern same in both cases. Unequal intensity of pressure-sensations from front and back of hand give idea of length" (N). (3—1=) "First heavier. Idea of length from pressure on back of hand and pull on fingers. Second about the same degree of unbalance but lesser weight. The relation of the back and front pressures was the same in both cases, although the intensity was less in the second for both pressures" (N). (3—4+) "Greater pull in wrist and greater pressure on fingers. End of handle presses up against palm of hand and sets up pull in wrist" (F). (5—3—) "First was heavier; more intense pressure on fingers. Heaviness gave length. Pull on end gave length" (F). (3—1=) "Pressure on fingers and pull on wrist same" (F).

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[11] The experimental series from which the reports are taken is given at the top of each group. The numbers before the quotations indicate the rods used for comparison. For convenience, the shortest rod, or the one with the nearest center of mass or, in case they are of the same length and have the same center of mass, the lightest rod is designated as rod number one, the others coming in ascending order. Rod number three is equal, physically, to the standard. From these numbers and the table of physical moments, it is easy to determine the physical relations between any two rods involved in the reported comparison, since the order of succession in that table is the same. The judgment rendered in every case is shown by the sign +, =or—; + standing for longer, = for equal, — for shorter. These legends are used throughout.

It is to be noted that in these last judgments and in all others of the same kind *O* based his judgment upon the absolute intensity of a single pressure, or of both pressures, independently of their relation.

(3—5—) "I first perceived weight. Sole cue for length was pressure sensations of hand, especially forefinger. Aware of weight from muscular sensations. Attention to sensations from hand gave definite length. With second rod, I was again aware of heaviness at the end" (Z). (3—5+) "Sometimes there is intervention of the weight-factor. I am apt to assign greater length to heavier rods. By closer concentration on the pressures in the hand and especially on the fingers I can usually, on the basis of this criterion, judge the length and hence differentiate between the two factors" (Z). (3—2+) "With first, just bare pressure-complex carried over in memory-after-image. There was a little throat-kinaesthesis that meant medium. With the second experience, the pressure was the clearest part. It was perceived both qualitatively and quantitatively, but the quantitative aspect was prominent and set off the reaction *longer*" (H).

Here again we see the influence of the intensive factor taken in an absolute way. The same thing is true of the next record. We found, in support of Meumann on the comparison of time intervals, that the pattern of consciousness with the comparison stimulus differed in certain essential respects from the pattern with the normal stimulus. The latter usually resulted in a full realization of the perception of length; all the sensory data came forward; the former resulted in a very scanty perception of length, a bare meaning of more or less; the apparatus, set for giving length, responded to any cue.

(3—3+) "Pressure complex the clear thing in the first experience. With the second, the pressure at the two points was immediately perceived as greater and the response came" (H).

Series A IV: (3—1=) "I got length and a notion of rod from both. Second was heavier. Both easy balance. The pattern was the same but pressure more intense in the second" (G). (3—3=) "Got the notion of long rod from the first. Fluctuating pressure. Second less fluctuating but more intense. The two sorts of criteria give the same notion if one is not critical" (G). (1—3=) "No difference in unbalance but the second was heavier" (N). (3—5=) "First was slightly heavier. A definite idea that length was there, but can't give a comparative judgment" (N). (3—5+) "Little heavier pressure on fingers and pull in wrist a little greater with the second" (F). (3—2=) "Pressure on fingers and pull in wrist the same" (F). (3—4=) "With the first rod, I was first aware of a certain weight. Then attention was drawn to the sensations in the hand and I got a definite notion of length. It appears that I am always baffled by the determination of length in distinction to that of weight. If I do not take care, I confuse a heavier rod as being longer. Both weight and length seemed to be determined by pressures in hand" (Z). (3—2=) "The first gave live contact-pressure with deeper dull pressure in forefinger; lively pressure of less intensity on base of thumb. Second experience less intense all round. Perception of length the same" (H).

The next reports also show both that there is a tendency for judgments of difference of length to be touched off by differences of absolute intensity, and also just the contrary: that differences of absolute intensity are ineffective to qualify the perception of length. As we have already remarked, the instruction to report differences of length, even when there were none, laid a heavy burden upon the *O*'s.

Series A VII: (3—1=) "Barest experiences. Just the same. Perception of an evenly balanced rod. When I report 'same' it is the sensory experience and not length that is the basis of judgment" (G). (3—3=) "With the first, a notion of length. Spatial pressure pattern. Second gave notion of heavy rod. Pressure perceived as weight and not as length" (G). (3—5=) "Nothing there to enable me to make the comparative length-judgment. Both uniform, static, dead. Second more intense, but did not carry meaning of length" (G). (3—2=) "With the first rod I had an immediate perception of length. This was clear. I got nothing, at first, with the second but weight" (Z). (3—1—) "First rod gave an immediate clear notion of length. With the second rod I was at first struck by considerably greater weight. Then in verbal-motor terms; 'which is longer?' The extent was limited by the points of weight-concentration" (Z). (3—5=) "The pressure-patterns were the same quantitatively and qualitatively. The second was perceived as equal, the judgment being part of the perception" (H). (3—2=) "First pressure-complex in the hand gave clear perception of length, a moderately long rod. With the second experience the pressure meant weight. Pressure more intense and static in quality. There was a tendency to say shorter" (H).

In series B III, where we used rods whose lengths, measured in terms of center of mass, were in the approximate region of the lower limen for the non-visual perception of length, but whose weight-increments were the same as in preceding series, we obtained more proof of the same kind.

Series B III: (3—1 No judgment) "Pressure on first finger and palm of hand in both. No muscular strain or deeper-lying sensations that I usually got. I seemed to be waiting for a pull downward on first finger; it didn't come, and I could not give a judgment of length" (S). (3—5=) "Meaning of length did not go beyond the handle. But the handle was stretched out. Pressure on finger and palm of hand and thumb. There was a strain-sensation, but it seemed as a bare weight. It came at the same time as pressure. It was more through the hand and a little in the wrist, not on forefinger" (S).

The following, also from the series in which we used the heavy rods, bring out the same points.

Series B IV: (3—2—) "The second experience was less intense and the judgment referred back to the first" (L). (3—5+) "The two experiences did not differ except on the score of intensity. I could not localize intensity or say how it was linked up with 'longer'"

(L). (3—1—) "Experiences were precisely homologous. The first was more intense than the second. This intensity seemed to insure or lead to the judgment" (L). (3—4+) "There was a clearly marked difference in intensity of pressure sensations in the hand. The greater intensity of the second did not carry with it a judgment of length difference" (L). (3—1—) "The second experience differed in absolute intensity and this intensity-difference went directly over into length" (L).

The next report, typical of a small class, indicates the effect upon the perception of length of shift of attitude. *O*, it will be remembered, was under a double determination; that of making a psychological report upon the process-contents of the experience, and that of making a perceptual judgment.

(3—5+) "The first thing with the second rod was sensations in the hand; those at the heel a little more prominent and intense. Almost immediately I noted a 'drawing' in the fore-arm which was more intense and 'drawing' than in the former case" (Z). (3—1—) "With the first rod, the sensations from the forefinger and the heel of the hand equally clear for a moment. The notion of length was very clear. With the second experience, I perceived sensations in the heel of the hand. Fairly definite notion of length. Then sensations in the arm with the verbal-motor 'heavier.' There also came a change in the perception of length. It became shorter. They were about equal before this time" (Z).

As regards these and the next following reports, it is to be borne in mind that, as is shown by the psychophysical results, the *O*'s were *not* able to make judgments of difference of length that have a significant degree of correlation with differences of weight. Failure to take the two sets of results together and to interweave them gives a false perspective.

(3—4+) "Pressure at base of thumb clearest part of experience. This 'live' pressure was more intense in the second case. The obscure dull pressure on the front finger was the same in both" (H). (3—1—) "The judgment had a purely sensory basis. The pressure complex was less intense in the second. The judgment came automatically without length being perceived" (H).

Finally, we cite a few reports from the last series, in which weight was the only incremented factor, to show that the perceptions of weight and length are differently determined, that the primary and secondary sensory processes in the two cases differ as to their meanings.

Series B VII: (3—1+) "The second experience less intense than the first. In the first, distinct strain in the wrist spreading to hand and fore-arm. The second noted because of lack of intensity" (L). (3—5+) "Prominent thing in the first experience was lively kinaesthesis in the fore-arm. The second experience was more intense. Pressure-complex in the hand gave rise to a conflict as to 'equal' or 'longer'" (L). (3—5=) "In both, which were similar, three complexes stood at focus; pressure from finger, dull pressure from base of thumb, and clear but not filled-out sensations from the arm. The three seemed to have a common background; in the second the background was of less intensity. The three factors were equal in all respects" (L). (3—2+) "With the first rod, I had sensations in the forefinger from the beginning. These were the most prominent part of the experience throughout. There was an immediate clear perception of length. There was a little change in the sensations from the arm. These were superficial and disconnected from the experience. With the second rod, there were first sensations in the heel of the hand. A very indefinite notion of length. Then sensations from the forefinger and the elbow came in. Sensations from forefinger clearest. Notion of length cleared up" (Z). (3—4—). "With first, sensations of equal prominence in forefinger and heel of hand. Sensations of lower clearness in fore-arm. Fairly definite notion of length. Second rod gave an immediate notion of length, mediated by sensations in the forefinger. Much less clear sensations in heel of hand" (Z). (3—1+) "Sensations from heel of hand and from fore-arm came in. Sensations from fore-arm clearest. Very definite notion of length. Then sensations in the forefinger became practically the only component. Length increased in clearness. With second, sensations in forefinger most marked but vague sensations in heel of hand and in arm. Notion of length very clear and definite" (Z). (3—5=) "First, sensations in heel of hand most marked. Sensations in arm and forefinger less clear and less intense. Not a definite notion of length. Then attention went to the sensations in the forefinger. Sensations in the arm dropped out and sensations in heel and finger became equal. With second rod, first there was verbal-motor 'heavier.' At the same time there came in marked sensations in the forefinger; lesser sensations in heel of hand and in fore-arm. Sensations in finger more intense and deep-seated than in heel of hand. Perception of length came very clearly" (Z).

Center of Mass

Since we took as our criterion for the conditions that determine the formation of the perception of length the ability, again objectively determined, to make judgments of comparative length, we must look for that factor, if any such there be, between which and the judgments we find concomitant variation. Fortunately we find such a one; the distribution of judgments, in every series where the center of mass noticeably varied, gives an X or cross with no inversions of the first order; in every one where the center of mass was constant the curves are flat and irregular. (See Figs. 1, 2, 6, 9, 10, 11, 12, 13b, and 16.)

But if center of mass plays so important a part, as a moment of stimulus, in the arousal of the perception of length, it becomes necessary to inquire more in detail as to its mode of operation. This enquiry should lead us to an understanding of the psychology of the perception, and should contribute toward its psychological analysis. As has been previously stated, a rod or instrument in the hand or a tall hat on the head acts as a simple lever or couple: if, that is, the rod is held out in the horizontal plane, or the hat is worn slightly tilted.

Lotze has pointed out that the tilting is an essential condition of the 'feeling' of enhanced height and consequent greater significance. This means, first of all, that there are two opposing forces or pressures. If we hold a rod in the vertical, there is nothing but the weight; no notion of length arises. It is only as the instrument becomes inclined that the perception of length comes in; weight without an extended center of mass never gives rise to the perception of length; though we do not at all deny that, given a center of mass 'out there,' variations in weight may set off a kind of derived judgment of difference of length.

The absolute intensity of the two impressions can not be the basis for the judgment of length; for we judge a long light rod, which produces relatively weak pressures, as longer than a short heavy bar, which produces much more intense impressions. The answer to the question: What is the experiential foundation for the perception? thus becomes clear; our process of elimination and our introspective analyses lead us to the same result: *the ratio between the 'kick' and the 'pressure.'* In a short rod, the amount of 'kick' relatively to the 'pressure' is small; in long rods, the two pressures more nearly approach each other in intensity; with every change in center of mass there takes place a change in the ratio of 'kick' to 'pressure.' A sensation-complex of two opposed pressures, more or less fused in experience, differing in intensity within certain ratio-limits, is the sensory pattern fundamental for our perception of length as given cutaneously; though sensory or imaginal elements of other kinds may touch-off or carry the meaning. Thus it may be intensity, perceived as weight or taken as bare intensity; it may be the temporal factor; it may be the direction in which the pressures are perceived as acting upon the hand; it may be the strain sensations in the hand; it may be other processes or perceptions than these which touch off the meaning of length or which determine the judgment when we make a comparison. All such secondary cues influence the comparative judgment more than the absolute; they determine differences of length under conditions that would not admit them into the absolute judgment.

A change of ratio of intensity between our two pressures, then, is at the core of the perception of difference of length; a change realized only by varying the center of mass. A variable weight brings no corresponding change in the ratio of the two pressure-intensities; it increases both in the same relative proportion; the ratio remains constant.

Without going into a detailed discussion of the sets of curves referred to in this section,—they speak for themselves, —we turn to the consideration of some special items of work. We have already found that, with the artificial thumb and forefinger eliminating the 'kick' and the 'pressure' in turn, the perception failed. We found, however, that when we attached the board to the arm in such a way that the band, holding the board to the arm, stimulated a single small area, the sensations from this point combine with those from the forefinger to give a perception of length. This result evidently means that sensations from other and more remote parts of the body, if they are opposed and if they stand intensively within certain ratio-limits to each other, will fuse into a total to give the perception. It is for this reason that the fisherman can feel the pull of the fish on the end of his line as 'out there;' he has the perception of the length of the fishing-rod, even though he hold it over the lower and under the upper parts of his arm. He might just as well hold it over the lower and under the upper parts of his leg; the sensations yielded up by these distant points of contact give the perception as immediately as do the nearer together and more practised parts of the hand. We made no determination of the precision of judgments of length when the sources of sensation are far apart on the body, as compared with the accuracy of discrimination when the parts are near together.

One other modification must be reported. For a short time we told the *O*'s to grip the handles very tightly. With long rods of considerable weight the perception of length was not wholly lacking; the ability to make judgments of comparative length, within the limits employed, was totally nullified. Whether there is or is not a perception of length, with rods of different length, thus depends upon the strength of the grip; if the grip be strong enough, the long rod ceases to be so taken. This can mean nothing more than that the small increments or variations in pressure are too slight to change the ratio in a perceptible degree.

In Table II we give the 'pressure' and 'kick,' their increments, their ratio to each other, and the change of ratio with successive rods, for a few selected series. The series given represent almost all the different combinations of moments employed.

Mention was made in the reports from those series in which weight was the variable of an experience that, while negating the factor of weight, yielded positive testimony to the importance of the two related pressures: it was called the feeling of balance or unbalance. In the following quotations

TABLE II.

Series	Rod	Fore Pr	F.Pr. Incr.	Back Pr	B.Pr. Incr.	Ratio of B.P:F.P.	Ratio Incr.
A I	1	248.23		153.23		1:1.6306	
	2	270.20	21.97	172.20	19.97	1:1.5691	.0615
	3	292.86	22.66	192.86	20.66	1:1.5185	.0506
	4	316.20	23.34	214.20	21.34	1:1.4761	.0424
	5	340.23	24.03	236.23	22.03	1:1.4402	.0359
A II	1	268.91		164.91		1:1.6306	
	2	286.74	17.83	182.74	17.83	1:1.5691	.0615
	3	304.57	17.83	200.57	17.83	1:1.5185	.0506
	4	322.40	17.83	218.40	17.83	1:1.4761	.0424
	5	340.23	17.83	236.23	17.83	1:1.4402	.0359
A III	1	170.06		74.06		1:2.2962	
	2	175.70	5.64	77.70	3.64	1:2.2613	.0349
	3	181.43	5.73	81.43	3.73	1:2.2280	.0333
	4	187.24	5.81	85.24	3.81	1:2.1966	.0314
	5	193.14	5.90	89.14	3.90	1:2.1667	.0299
A IV	1	314.06		218.06		1:1.4402	
	2	320.60	6.54	222.60	4.54	1:1.4402	.0000
	3	327.14	6.54	227.14	4.54	1:1.4402	.0000
	4	333.68	6.54	231.68	4.54	1:1.4402	.0000
	5	340.22	6.54	236.22	4.54	1:1.4402	.0000
B VI	1	787.50		587.50		1:1.3404	
	2	815.00	27.50	615.00	27.50	1:1.3252	.0152
	3	842.50	27.50	642.50	27.50	1:1.3113	.0137
	4	870.00	27.50	670.00	27.50	1:1.2985	.0128
	5	897.50	27.50	697.50	27.50	1:1.2867	.0118
B VII	1	808.80		616.80		1:1.3113	
	2	825.65	16.85	629.65	12.85	1:1.3113	.0000
	3	842.50	16.85	642.50	12.85	1:1.3113	.0000
	4	859.35	16.85	655.35	12.85	1:1.3113	.0000
	5	876.20	16.85	668.20	12.85	1:1.3113	.0000
F IV	1	352.50		252.50		1:1.3960	
	2	375.23	22.73	275.23	22.73	1:1.3633	.0327
	3	400.00	24.77	300.00	24.77	1:1.3333	.0300
	4	427.00	27.00	327.00	27.00	1:1.3058	.0275
	5	456.43	29.43	356.43	29.43	1:1.2806	.0252
F V	1	400.00		286.53		1:1.3960	
	2	400.00	00.00	293.41	6.88	1:1.3633	.0327
	3	400.00	00.00	300.00	6.59	1:1.3333	.0300
	4	400.00	00.00	306.32	6.32	1:1.3058	.0275
	5	400.00	00.00	312.35	6.03	1:1.2806	.0252
F VI	1	418.82		300.00		1:1.3960	
	2	408.99	9.83	300.00	00.00	1:1.3633	.0327
	3	400.00	8.99	300.00	00.00	1:1.3333	.0300
	4	391.74	8.26	300.00	00.00	1:1.3058	.0275
	5	384.18	7.56	300.00	00.00	1:1.2806	.0252

taken from the series in which center of mass varied, we have still further proof of a positive character.

Series A I: (3—4+) "Greater strain on back of hand. Pressure sensations from back of hand greater in second" (F). (4—3—) "Wrist same. Strain factor from fingers to wrist same. Push down on the fingers the same. The thing seems to depend on the pull down from away out there" (F). (1—3+) "Pull in wrist greater and more of 'feeling of going down;' pressure down on thumb and two small fingers. Also pull on back of hand. Pull on back due to end of handle pressing on palm of hand" (F). (3—2+) "The strain-feeling in wrist greater for the second. Also a pull on hand, especially on front finger and back of hand. A sort of stretch" (F). (3—3=) "Pressure alone vivid. Set for pressure. Perceived quantitatively. 'Long' applied to pressure. Notion of rod did not come in. In the second experience, the pressure was the clear part and set off the judgment automatically. Rod context came afterward" (G).

The above report implies, as does many another, a difference between the 'bare length' attitude and the 'length of *rod*' or objective determination.

(3—2=) "Positive judgment set off by hand-complex and slight increase as rod pulls down. This gives meaning that rod is there. Judged on basis of pressure-complexes which were the same in both cases" (G). (3—5+) "Nothing in the first except the perception of pressure and measuring of it. This carried in memory-after-image. 'Longer' came with the second quite automatically with increased pressure. I really judged in terms of heaviness. Length-notion came after" (G). (3—1—) "Resistance less with the second; it came right up. With the first there was more pressure on the back of the hand; the second, even pressure" (N). (3—3=) "I concentrated attention on the shoulder. The shoulder gives criteria for weight. The perception of length seems to come from the pressures in the hand" (N). (3—5+) "With the second, more intense pressure on the fingers and relatively more intense on back part of hand where handle comes against it. This gives length; the length gives pressure at two points" (N). (3—3=) "It is the balance-pattern in the hand that gives notion of extent. It is in this way different from mere weight" (N). (2—3+) "In lifting the first rod there was a lightness of pressure in the hand. The pressure in the hand, with the second, was greater as well as the weight" (Z). "I think in terms of length in relation to compactness or concentration of weight. That is, if the weight is closely compact, it may cause as great a pressure with a short leverage as a more distributed weight on a longer leverage" (Z). (3—4=) "In lifting the former object, I was conscious of an object of considerable length. In lifting the latter rod, I got a notion of weight and pressure in the hand due to leverage. The latter no longer than the former though considerably heavier" (Z). (3—3=) "The clear processes were pressure-sensations from forefinger and base of hand. Judgment was made immediately on basis of comparison of these two sensory complexes. No definite notion of length in either case. Pressure-pattern, that is, the relative distribution of pressure at these two points, was the sole criterion for judgment. They were the same in both cases" (H). (3—5+) "Pressure greater on back of hand. I didn't notice much difference in the forefinger" (H) (3—5+) "Again, pressure at those two points, only, came into the experience.

In lifting the second rod, the pressure seemed to be a little bigger spatially; larger rather than more intense. Gave rise to notion of greater length; greater leverage" (H). (3—4+) "The only mental processes which enter into the judgment at all are pressure sensations from the hand, especially from forefinger and base of hand. In this case, the sensations from these two points, particularly the back, were a little more intense with the second rod. This distribution of pressure gave me the perception of length, although no particular length was assigned to either of the rods" (H).

In the above series weight, as well as center of mass, was a variable component. The following reports were made when center of mass and length were variables.

Series A II: (4—3=) "Quantitative judgment of the first. Verbal-motor, 'great.' With the second there was a tendency to call it lighter. Then the pressures came to clearness and carried the meaning 'equal.' The reaction followed" (G). (3—1—) "Pressure-complex, qualitative and spatial, in hand. There was no notion of rod. In the second, the pressure-complex was clear. Judgment came immediately. Qualitative and spatial pattern and not weight or quantity is significant" (G). (3—3=) "The clear sensations came from the hand. The pull down on the second finger, and the use of the forefinger as guide while it is barely in contact with the handle. Also pressure from the base of hand" (N). (2—3+) "Pull on skin in addition to the greater pressure and pull of the second all gave meaning of greater unbalance" (N). (3—3=) "With first rod, pressure in hand the clearest part of the experience. Length was assigned mainly in terms of pressure in hand. Apparently forefinger acts as fulcrum between the sensations in back part of hand and arm, on the one hand, and length of rod, on the other. Can't differentiate any mark by which I become aware of weight separate and apart from those whereby I assign length: *i. e.,* at the same time that I assign length there is also a definite assignment of weight. When the weight association comes in, the judgment is mediate" (Z). (3—4+) "In first, I was aware of muscular sensations in the arm, then of sensations of pressure in my hand. There was immediate definite length. With the latter rod, there were also sensations in arm and hand. It appeared that the long rod was acting as a lever over the hand. This was assigned length due to the pressure exerted in the hand by the leverage" (Z). (3—5+) "More stretch or pull in wrist. More pull in back of hand with the second" (F). (2—3+) "Little pull in wrist with the first. The pull a little greater in the second. Seemed to be the same weight and the same pressure on the finger in both. Pull in wrist as if the thing reached out longer and gave more pull in wrist" (F).

It may be recalled that the pull in the wrist for F is the sensory experience set up by the back end of the handle pressing up.

(3—1—) "With first, pressure-pattern-complex clear. Second, sensations in forefinger first. Then sensations at base of hand set off judgment 'shorter.' These were less than with the former rod" (H). (3—3=) "Clear thing in first experience was pressure in hand. With second rod pressures in hand very clear and equal to the former experience. Judgment followed immediately" (H).

Series A VI: (3—5+) "Perceived the first as fairly long rod.

Criterion simply intensity of pressure. Pressure fairly intense and a little more so away from body. Second more vivid and alive. Pressure very intense on outer finger. Perceived as 'longer.' Then notion of long dipping rod" (G). (3—1—) "Got notion of length; part of immediate perception. Then sensory experience became clear; live, fluctuating, more intense away from body. Second, perceived immediately as 'shorter.' Perfectly uniform pressure without change" (G). (3—5+) "Greater pressure on fingers and greater pull in wrist with second" (F). (3—2=) "Pressure on fingers and pull in wrist same" (F).

By this time the perception of length came quite immediately, and the *O*'s were often unable to get a vivid sensory experience or a full perceptual realization of length before the meaning itself became clear. There is, moreover, some evidence that the meaning may develop without any process-content at all becoming vivid.

(3—5+) "With first rod an immediate definite notion of length in motor terms and it remained stable throughout. With second, much greater relative pressure and in a flash seemed very much longer with verbal-motor 'longer'" (Z). (3—2=) "The first gave immediately, in motor terms, a fairly definite notion of length. Sensations in hand then became clear. With second, again all at once, upon lifting the rod, I got notion of length in motor terms. Verbal-motor 'equal.' The pressure-sensations again became clear and were equal to the former. The critical comparison or criticism of the automatically rendered judgment based on pressures in hand" (Z). (3—4+) "Judgment made on quantitative basis. Second, pressure at base of hand more intense relatively to pressure on forefinger, and the entire pressure-pattern more intense than the first. First, like well balanced rod; second good deal of pull down to it" (H). (3—1—) "In first, pressure moderate, evenly distributed. A well balanced rod. An immediate perception of length. In second, pressure was much less *in toto* and pressure at base of hand very slight. A little rod tipping up. 'Shorter' came as a part of the perception of length" (H). (3—5+) "In all these cases, the perception of length comes immediately. Sensory basis becomes clear afterward. Pressure in first that of a well balanced rod; liveness gave meaning of moderate length. With second, pressure more intense; strong pull down. Tendency after judgment to call the second heavier. Intensity of pressure at base of hand meant 'longness'" (H).

The series in which we used the heavy rods gave us reports of the same general bearing.

Series B I: (3—5+) "Pressure more on three fingers and muscular sensation in fingers. In the second, intensity of pressure on palm of hand and base of thumb. More muscular sensation in the whole hand, in the second" (S). (3—5+) "Pressure and muscular sensations greater in second. Also deep pressure-sensation in thumb and greater tension in second" (S). (3—4+) "All in hand. Pressure a little in second. Muscular complex in hand. Hand adjusts itself a little differently to different lengths" (S). (3—2—) "Muscular tension in fingers. Pressure on thumb. Second, pressure on thumb and muscular tension in fingers. Pressure less in last experience" (S). (3—1—) "Second came up easier, and it didn't drag or pull the forefinger down as a long one would" (S).

The strain-sensations set up in the hand as the organism tends to resist the two opposing pressures and to hold the rod in the horizontal position, enter now as a new secondary factor for carrying the meaning of length.

(3—2—) "The meaning, length, is placed upon the differential pressure-sensations in the hand; *i. e.*, the greater or less pressure on the forefinger *versus* that on the heel of the thumb carries meaning of length somehow, not visually, projected beyond the hand. The hand has come to mean fulcrum" (L). (3—5+). "In the first rod the pressure-complex was clear. Deep pressure was especially prominent. In the second case, the strain-sensations in the arm seemed coordinated with the pressure-sensations in the ball of the thumb" (L). (3—5+) "The handle, in this experience, was grasped less tightly, therefore the differential pressure-complex was more definitely analyzed: the pressure-complex of thumb and finger from that of the palm of hand. Torque was localized in hand. How torque functioned in connection with the pressure-complexes, I can not say. This torque is all in the hand" (L). (3—2—) "Experience centered about sensations in the hand. In the first, a clear differentiation. Light pressure and contact from thumb clear. Also sudden and intense deep pressure from palm of hand. The second experience was the same in the main, only less intense, especially on the thumb. Judgment immediate" (L). (3—1—) "Experience of the usual sort in which the judgment followed immediately. Difference that demanded attention was localized in hand and was of temporal nature. Complex of pressure and kinaesthetic sensations whose course was slower in the first experience than in the second. As to region of hand, contrast between inner side of hand just behind forefinger, and the base of the thumb" (L). (3—4+) "First rod gave clear sensations in forefinger and less clear on back part of hand. Experience in lifting second, sensations of forefinger more intense. Also more extensive. Immediately judged it as 'longer'" (Z).

It often happened, as in this case, that *O* noted the difference of intensity at a single point, say the forefinger; a fact which might seem to contradict our general conclusion. The contradiction would be real, however, only if we were in face of something other than distribution of attention; if *O* did not in some way take account of the relative as over against the absolute variation.

(3—1—) "With the first rod, I experienced, first, fairly clear sensations in the hand, especially on forefinger and at base of thumb. With the second rod, I had sensations in hand, wrist and fore-arm. Attention not demanded at any one point" (Z). (3—4+) "I first noticed, in the first experience, sensations in the index-finger which were very intense and pressed the forefinger down tensely against the wrist. There followed, soon, sensations in ball of thumb. Second rod, somewhat similar experience for the first moment. Pressure on forefinger following, down to wrist, and lesser sensations at base of hand. These accompanied by slightly greater strain-sensations through the hand." (Z). (3—1—) "Dull pressure-sensations at base of thumb especially clear in the two experiences. Much less intense with a somewhat 'bright' quality in the second" (H). (3—3=) "The pressure from the forefinger and base of hand the same in the two experiences. The clear observation of the sensory complex in the second followed the judgment, which seemed a part of the perception" (H). (3—2—) "The pressure on the forefinger was, in every observable respect, the same for the two experiences. The pressure at the base of the thumb was less intense in the second case. Also it had the meaning of 'liveness' upon it, due to a 'bright' quality and a temporal fluctuation" (H).

Since section B in a sense duplicated section A, only a part of the *O*'s were required to perform both series. It is for this reason that we have introspections from two or three only.

Series B II: (3—4—) "Strain in skin, forefinger and hand. The second more intense, with increased pressure on the thumb" (S). (3—1—) "Pressure in palm of hand, in first. A little strain in first finger. Less strain and less pressure in the second" (S). (3—4=) "In first strain in fingers and down through palm of hand all as one sensation. Second, same in distribution and intensity" (S). (3—1—) "Strain-sensations in finger and palm of hand. Pressure-pattern at base of thumb. Second, lack of strain-sensation in hand, less intense in fingers" (S). (3—5+) "Pressure on forefinger and at base of thumb the clear processes. Less clear muscular and strain-sensations through hand. Marked increase in intensity at base of thumb; less marked increase on forefinger. The second pressure more extensive, giving rise to notion of its pulling down more" (H). (3—2—) "The two experiences were qualitatively the same. The second was a little less intense at the base of thumb. The second also less extensive. Strain-sensations through the hand the same" (H).

Series B VI: (3—4+) "Most prominent distinction between the two, and which seemed to carry meaning 'longer,' was the temporal sequence in sensations at base of thumb. In the second they rose more slowly. Temporal course in first more rapid. This may have been confused with amount of change passed through. Greater range in second; *i. e.*, the relative amount of increase of intensity was greater with the second for this particular complex" (L). (3—5=) "The two experiences as usual, except for a peculiarity in second. Began by intense pressure on thumb, and this brought at once the meaning 'longer.' But pressure decreased and then increased on forefinger. This negated the earlier intense pressure against base of thumb" (L). (3—1—) "The sensation-area from forefinger constant and in background of consciousness in both. In first, the pressure at base of thumb showed a transitional change and rose to high intensity as a block of experience lifted out. In the second, the whole thing seemed a flat pattern with only a vaguely traced boundary between pressure on forefinger and thumb" (L).

All these reports from the introspective data corroborate the findings from the psychophysical series as presented in graphical form.

Synthetic Series

The best test of any hypothesis regarding the analysis of a perception is that of reconstruction under the observed conditions. If our assumptions are well founded, then the perception of an extent beyond the hand should be aroused by any two pressures, so long as these are kept between certain limits of ratio of intensity, of spatial relationship, and of direction. Taking these facts as our point of departure, we set out to produce the perception synthetically.

The matter of thus setting up the length-experience with no length present reduced itself finally to the problem of devising a means by which the two opposing pressures could be applied to the hand (since this was the part of the body with which we had worked) in a way that should be under perfect control. We found that when we took two short pieces of wood like the handles, and pressed the one up against the base of the thumb and the other down upon the forefinger, our *O*'s gave immediate judgments of length: in fact they were unaware, according to their own report, of the substitution of the two short pieces for the one long rod. Further preliminary trials proved that the length, as perceived, could be made 'longer' or 'shorter' by alternately increasing or decreasing the 'kick' and decreasing or increasing the 'pressure;' and that the perception broke down if *E* increased either 'kick' or 'pressure' beyond certain limits, when *O* perceived the two pressures separately as two unrelated experiences. So far, however, our pressures were not subject to rigid control.

The problem demanded two physical pressures, which were to be resisted independently, to operate in opposite directions, to be stable in duration, definite in amount, and independently variable. The first apparatus consisted of a handle, with an oval hole cut through lengthwise, slipped over a small rigidly supported rod set in the horizontal plane. A short pin, set into small holes bored through handle and rod at right angles to their principal dimension and at close intervals, served as a pivot, so that the handle had a considerable latitude for movement up and down about the pivotal point. By altering the point of pivoting, the length of the two lever arms changed inversely, while at the same time the down-pressure of the fore end and the up-kick of the back changed in an inverse ratio. Since the effective physical pressure is a function of the length of lever arm and of the weight or force applied, these pressures could be determined and varied by assignable amounts; the weight, hung at the front end of the handle, tended to actuate rotation of the handle in the hand of *O*, who gently resisted the movement. The device was a success so far as the evoking of the perception of length was concerned. It was, however, ill-adapted to the evoking of judgments of comparative length, since *O* might, by using the pivot as a fulcrum, resist all force of the weight at either the fore or the back point of contact.

Next a split handle was arranged, with the top half pivoted in front and moving upward through a small arc at the back, and the bottom half pivoted behind and moving downward; the movement was produced by weights attached to cords which ran over pulleys. The two pressures could be varied at will, and must be resisted independently. The perception of length did not invariably result, for the *O*'s were immediately aware of the division in the handle, and might allow the one half to move through a relatively large arc while they held the other from moving at all.

This apparatus was given up as a failure, and in its place we made a handle with a smooth horizontal hinge at the middle; the back end being pushed up and the fore end pulled down by easily variable and definite amounts. *O* took the handle loosely and the weights released caused the handle to dip down in front and up behind, just as a rod would behave. So long as the handle did not 'break' at the hinge, the perception of length was definite and controllable; but a 'break' destroyed the perception, since the two pressures were no longer 'in line.'

Finally we hit upon a simple and satisfactory arrangement; a handle suspended on a cord attached at the front end. Fastened to this handle were two weights; the one, fixed to the lower side at the front end, pulled down; the other, fastened to the back and by means of a cord run over a pair of pulleys, pulled up. There remained one disturbing factor; the handle was unstable; it seemed unattached. By inserting a light rod into the handle, the possible length-effect of which we overcame by a weight on a heavy wire set into the rod and bent back under the handle in such a way that the center of mass was at the point of the forefinger, we secured better results. But the rod was presently dispensed with. We suspended the front weight, which had been rigidly attached to the front of the handle by a short wooden piece, on a cord and so eliminated the rotation-effect due to the swing of the weight. After lengthening this cord which held the front weight, so that its pendular period was about the same as that of a normal rod which would give the same 'kick-pressure' ratio as the two pressures from the handle, we took out the rod; a bare handle remained.

The front weight was 250 gr.; the back pull was 140 gr., increased and decreased by 20 gr. increments. To be sure, the two pressures were not wholly independent of each other; if the pull or 'kick' did not come directly under the point of contact between the hand and handle, any increase or decrease of the pull would be reflected by a change in the amount of pressure necessary to counteract the weight in front. We can not be absolutely sure that with *O*'s whose hands differed considerably in size we met this objection. But even if the front pressure varied somewhat from the amount of the weight, it could have done nothing more than change the ratio of 'kick' to 'pressure;' it would have been a constant error for any one *O*. According as it increased or decreased the absolute intensity, it would have made the small changes less or more easily perceived.

Two features connected with the lifting of the handle called for special practice; *O* must lift the handle in such a way that the two pressures came on simultaneously, and must oppose the back pull sufficiently to prevent that end of the handle from moving upward.

Fig. 18 gives the graphical representation of the results; the abscissae correspond with the five settings of the back pull which alone varied. Here are comparative length-judgments made under the same instructions as the judgments with actual rods. The results speak for themselves. There can be no doubt of the actuality of the perception, or of the effectiveness of small changes in the intensity-ratio of 'kick' to 'pressure' for altering it: the perception of length can be synthetically produced, and is so accurately conditioned that very small changes in a single pressure bring about corresponding changes in the meaning of length. A comparison of the results of series F IV with those of F V and VI (Figs. 24, 25 and 26) shows that variation of a single pressure is less effective for determining the perception of length than is a variation in both pressures, changes in center of mass being the same; and a comparison of F V and F VI shows that, when the fore pressure is constant (see Table II), the back pressure variable, and the center of mass still the same for corresponding rods in the two sets, we get higher percentages of the 'longer' and 'shorter' judgments than when the reverse is true. We had not the results of section F before us when we performed the synthetic work.

The reports from the synthetic series show that the same two pressures demand attention under the synthetic situation as in the case of true rods.

Synthetic Series: (3—5+) "Strain-sensations run down wrist and are fused with the hand-sensations. The pressures come on forefinger and at base of thumb. These latter are objective" (Ca). (3—5+) "Pressure on index-finger most intense. More intense in second. Pressure on other fingers less intense. Pressure on thumb was uniform" (Ca). (5—3—) "In first, pressure on forefinger. Irregularity in change of intensity of pressure. In second, pressure was constant in intensity. Pressure on thumb and ball of thumb. Constant sensations in other fingers" (Co). (3—5+). "Pressure-sensations on forefinger and thumb, more extensive on thumb with second. Pressure on forefinger slightly preceded that on thumb. The two must be combined to give the total experience" (Co). (1—3+) "Pressure on forefinger and in palm of hand, at base of thumb and at tip of little finger. More intense pressure on palm of hand and little finger in second" (Co). (3—1—) "Very intense pressure at base of first finger and intense pressure at base of thumb. The pressure in the second case less intense. They came in simultaneously" (S). (3—5+) "Pressure deep at base of first finger and pressure on thumb, about the middle. Clearer and more intense on finger. Area shifted outward a little. In second, deep pressure on finger, more intense and changed in area, more spread out than first. Pressure on thumb less clear, more intense than in first" (S). (3—2—) "Dull pressure on forefinger and at base of thumb in both. Vague sensations in lower part of arm. Less intense in second case. The pressure at base of thumb less in second, gave meaning of slightly less unbalance" (H). (3—4+) "With the second, the pressure at the base of the thumb was more nearly equal in intensity to that on the forefinger, than in the first. No qualitative difference noted" (H). (5—3—) "Marked pressure sensations on base of thumb, in the first. Less clear sensations on forefinger. With second, the sensations on forefinger clearest. Those at base of thumb less intense" (Z). (3—1—) "First experience carefully noted. No definite idea of length. Judgment part of second. When analyzed, it was less intense at base of thumb and less strain in hand" (L).

V. SECONDARY FACTORS

Visual Imagery

All *O*'s reported visual images, more especially during the early series; Z reported images of this kind, more or less, throughout the whole of the two years. In all cases, however, the image was an immediately accruing process which derived its meaning from the sensory complex in the hand. Frequently it was instable, fluctuating with changes of eye-convergence. Frequently *O* volunteered the statement that it came after the judgment and served merely a confirmatory purpose; it was his justification. Furthermore, the coming in of visual images bears a close relation to the giving, under the comparative determination, of judgments of absolute length and of reflective judgments. In fact, visual imagery was never more than a secondary factor in the perception; it never came alone or even first in temporal sequence; it was far more likely than not to occur in the case of the reflective judgment or to follow the judgment; and it never appeared when the judgment was a part of the perception.

The following reports show the symbolic character of the images, the temporal order of their occurrence, and the relation they hold to the notion of length and to the comparative judgment.

"Image and eye-movement. No notion of length. Just rod lying out there going into third dimension. This image carried right over into the second experience. But it had nothing to do with the judgment. Judgment made on basis of pressure solely" (G). "As soon as judgment was rendered, a visual image meaning length came in fragmentarily. Also eye movement. All as I am reporting and after." (G). "After judgment, visual gray spot for a second. Some eye-movements entered into spacing. Confirming" (G). "Process, gray thing, developed by eye-movement. When sighting where I thought the thing would be, image came in" (G). "Visual image. Eye-strain determined distance" (G). "Accompanying judgment was a composite visual schema of two lines, one longer than the other, in a vague way connected with the hand. Visual image not at all connected with the judgment" (L). "Slight visual imagery, very schematic. Not at all a reproduction. Visual image supplementary" (L). "Visualization not important. Weak, hazy, no clear outlines, insignificant" (F). "Visualization. Second, lighter gray. Spatially related, eight inches between them. Darker one farther out. Second lighter, nearer in" (F). "Visual image of a flashing up of the end of the rod gives a rough outline of length" (N). "Scrappy bit of visual imagery came in after the judgment. It meant the end of the rod" (H). "When I had lifted the last rod, I had a visual image of hand lifting two rods in succession. At first the two were not definitely outlined as to length. In a moment the image of the first rod appeared as extended about two inches beyond that of the second. This image was vague except at the hand" (Z). "In this experience I had very clear visual images. Clearest part of image connected with lifting the last rod was at distant end of rod. End of rod was much heavier than first. It seemed as if it were bent down at the end" (Z). "As soon as I lifted the first rod there appeared a definite extent in space from me. The nature of this I do not know, for almost simultaneously I had a visual image of a rod the distant end of which was the clearest part. I rather think there was something there before the visual image appeared" (Z).

The introspections show a relation, more or less definite, between the quality of the image and the perceived weight or length. For F the darker gray always stood for the rod producing the more intense pressure; Z distinguished the dark, solid-grained oak from the soft, light colored poplar; and H occasionally correlated 'light and short' with bright tint. Always, however the perception of weight or of length was primary. There is also a relation between the tendencies to visualize and to objectify the stimulus-object. So long as *O* perceived length as bare length, unrelated to an object, the visual image did not enter; when he perceived length as the length of a particular object, the objectification often took place in visual terms.

Muscular, Strain and Articular Sensations

James[12] says of projected movements that all the intervening space must be represented in consciousness; that when the projection is to the end of a walking stick, the extent of this stick must be represented in visual, tactual or kinaesthetic terms. James is concerned with projection and not with bare length; but in the case of judgments of absolute length, with objectification of the experience, we have a very similar condition. We found muscular, tendinous and articular experiences reported by our *O*'s, for the most part as sensory and not as imaginal. Z reported them more consistently and for a longer time than any other *O*; Ca reported them during the weight-series. These sensations compelled attention by their intensive changes. The arm seemed to be the seat of the only variable experience; and hence to afford the psychologically adequate basis for making differential judgments. It may be that Z had recourse to visual and motor imagery and to the reflective judgment more than any other *O* because for him the successive perceptions were differently determined. That is, the clear and, for him, the significant part of the first experience might be arm sensations, for the second, the hand-complex. Then the visual images evoked by the two would come together as the common element for making the comparison. One of his reports will make this state of things clear.

(3—1—) "With the first rod, I had, first, sensations in the hand. I did not get a definite notion of length from the sensations in the hand. Then there were very marked sensations from the arm. Meaning of length took on more definiteness. These sensations from the fore-arm remained clear throughout the experience. With the second rod, I had sensations in hand and also given at the same time the notion of 'lighter.' Visual image of picture given by Ebbinghaus in connection with a discussion of looking at two grays. Notion of 'lighter' pretty well marked. Verbal-motor: 'Is it shorter?' Notion of length seemed to vacillate. Again sensations in arm came in, and seemed to help in the attainment of definiteness of length. Visual image of the two rods of about the same length. Heavier one dark and of heavy wood; the light one, a poplar rod. Judgment based on the visual images" (Z).

Experiences of such elaborate pattern were rarely reported; the imagery was more often of the motor or kinaesthetic than of the visual type. But almost without fail, when the successive perceptions had a different sensory determination, *i. e.,* when different sensory processes were clear, some type of imagery served to represent at least one member of the two experiences which came in for reflective comparison. It is significant that the other *O*'s seldom reported kinaesthetic sensations, but they almost never gave the dual determination of the two experiences, and Z rarely reported kinaesthesis in both experiences. Though, when sensations from the arm were clear, the perception of weight almost always intervened, it has yet to be shown that sensation from this source can function in any other than a vicarious manner; it is more probable that the strain and muscular sensations from the hand and, to a lesser degree, those from the wrist are the psychological correlate of the meaning of length; the part they play would still be secondary and not primary.

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[12] W. James, *The Principles of Psychology,* 1890, II, 196.

When we used the wooden 'spoon' to take up the 'kick' of the rod, L reported a 'thinness' or 'emptiness' of the experience, which he attributed to the elimination of the hand and wrist kinaesthesis; this kinaesthesis had served as a background for the total experience. With the heavier rods, S repeatedly reports muscular and strain sensations from the hand aroused by the leverage of the rod held in the horizontal position. All these things are very different, however, from the kinaesthetic image an *O* would derive from having laid the rod along the body and leg, or from having run the hand along its extent.

The Temporal Factor

There were at least three, more or less distinct, ways in which the temporal factor entered into the complex of experience to affect the judgments both of length and of comparative length. These were the quickness with which a short or light rod came up, especially when lifted after a longer or a heavier rod; the time, independent of the lifting, within which the pressure-pattern formed in the hand; and the degree of steadiness exhibited by the pressure, as 'dead' and unchanging or as 'live' and fluctuating. When the first of these modes was an element within the complex of experience, the judgment 'shorter' or a tendency toward that judgment followed, if the rod came up quickly, and contrariwise if it came up slowly. The former happened much more frequently than the latter. If *O* postponed judgment until after the rod became stable, he frequently reversed his judgment, if it was a matter of difference of weight.

Some of the pressure-patterns, in accordance with the second principle given above, formed immediately when the lifted rod became an effective stimulus, and remained unchanged throughout the experience; others showed a well marked course of development in intensity, extent, or (less commonly) quality. In general, an increase of intensity or extent carried with it the meaning 'longer,' as did a shift of quality in the direction of ache or strain.

The 'liveness' of the pressure, required by the third principle, differed from the change mentioned under the second heading in that it was not a growth in a constant direction. As a rule, an unusually 'dead' static pressure carried the meaning 'long;' the 'live' fluctuating pressure, the meaning 'short.'

These three aspects of the temporal factor hardly ever occurred in combination in a single experience. Illustrative reports follow.

"As pressure creeps it becomes more intense on index finger and less intense on little finger. Second pressure intermittent, up and down, on forefinger" (Ca). "Second gave very slow intermittent pressures" (Ca). "Sensations on forefinger varied in intensity, also in a temporal way" (Co). "The difference is largely a temporal one in the forefinger. In the first, there was fluctuation; in second, constant. Especially fluctuations in muscular and tendinous sensations of the forefinger" (Co). "Resistance less. Came right up" (N). "Longer ones more springy; shorter ones come right up quick without any springiness. Greater pressure on the base of the hand with the longer rods is there immediately upon picking them up.

As soon as they are raised the distribution of pressure equalizes and remains constant" (N). "Second pressure at base of hand variable in time. It had a 'lively' quality, go and come" (H). "A sudden intensity means weight; length comes slower" (F). "Strain-sensation in fingers and palm of hand. Also deep pressure, mixed together, and it grew in intensity relatively slowly. This gave the meaning 'longer'" (S). "Second, slower strain-sensation in fingers and on thumb. Rose to maximal intensity more slowly than first and carried meaning 'longer'" (S). "The temporal course of processes seemed to be the most important. Torque and temporal bound up very closely" (L). "Second experience, whole experience less in intensity and showed none of the temporal course. Second, 'shorter'" (L). "Temporal sequence in this case clearly observed. Slower in second case; *i. e.*, the one to which 'longer' attached. The thumb-complex rose slowly to full intensity" (L). "Arm went up so much faster than it ought. Too much energy. It was surprisingly short" (C). "First, pressure 'alive,' 'gay.' Perception bound up with greater intensity that meant 'dead' weight and not fluctuation" (G). "Second, got more intense and spread out as if rod was bending down" (G). "I thought both long because of temporal thing, gradual increase in intensity of pressure as they were picked up. The proportional relation of amount of intensity and the rate of increase were noted" (G). "There seemed to be something like a resistance, a give or vibration or something, that gave the notion of length as distinct from the dead pressure from heaviness" (G). "Second seemed to go up. Sudden change in pressure, lightening of pressure, gave notion of 'short'" (G). "Fluctuation, more and then less intense, gave perception of fairly long dipping rod" (G).

Pressure Gradient

There are, no doubt, other secondary criteria upon which the perception of length, in the sense of this study, may be based. We have found, *e. g.*, that if a ring is made fast to the skin by means of a gummed tape, and a pull is exerted on this ring in a direction away from the body while at the same time a pressure of slightly different intensity operates upon a nearby spot on the skin, the perception of length follows: the extent is in the direction of the 'pressure' if this is more intense than the 'pull;' in the opposite direction, if it is less. Gradients in either direction are thus adequate to evoke the meaning of length, provided always that they are opposite and slightly different in amount. Moreover, the steepness and direction of the gradient, in the hand at least, are factors that on occasion play a part in determining the meaning of length.

The Absolute Length-Judgment

As in the case of lifted weights, the judgment of absolute length appeared; all *O*'s reported it at times. The judgment usually occurred with rods of the one extreme or the other; practically the second one was judged before it was lifted. The general instructions failed to the extent that a special disposition or attitude might develop which prevented *O* from doing his assigned task. Suppose, *e. g.*, that *O* judges the first rod as 'very long' in absolute terms; then he will expect, pre-judge, the next as 'shorter:' the consequent 'set' for the next rod is 'short;' the characteristic intensive, temporal and other differences are anticipated.

This absolute judgment is, however, of little concern to us in our analysis of the non-visual perception of length, since there is no indication that its determination is in any way different from that of the perception of bare length or of the perception of difference of length. We note only that the visual image, more frequently than any other process, carries the meaning of absolute length. Visual image, absolute length and the reflective judgment are bound up together. The two absolute meanings, visually determined, must be brought into a logical relationship before a comparison is possible. The situation is somewhat different in the case of judging the length of a single rod. Illustrative reports are:

"Visualized two points in space. First, far out; second, near. Two absolute judgments. Relative judgment an inference" (F). "I got the meaning of absolute length with the first. This judgment of absolute 'shortness' for the first gave me a 'set' for 'longer.' I expected the second to be longer. I can't say whether that imposed judgment or not" (H). "Absolute judgment of first as 'long.' Set to expect 'shorter'" (G). "Absolute judgment of first 'long.' Set off by pressure-pattern" (G).

VI. On the Computation of a Limen

The method of Constant Stimulus Differences may and usually does give a *DL*. Still following Urban we sought to use our data for this purpose. We computed the h' and L' for the results of the several *O*'s in the early series; but there we were halted. What is the unit of stimulus in terms of which a *DL* might be expressed? We had three sets of variables, all of which varied together in direct proportion. Which should be used in correlation with the psychological experience? At first we thought that the *DL* should be expressed in terms of weight; for applied weight is the mechanical stimulus which produces sensations of pressure, and resisted weight, muscular and strain sensations. But the results shown in Fig. 2, as well as those of the other series in which the weights were the same for all rods, prove that the *DL* cannot be found from the objective differences in the weight of the rods. So the graphs given in Figs. 4 and 17 show flat and irregular distributions; the 'kick' and 'pressure' were not equal or constant, but their ratio did not change. Weight is out of the question.

Before the work was far advanced we had decided that length, in and of itself, could in no way affect differentially the arousal of sensory experience; hence we could not correlate length with judgments of length. The results (Figs. 5 and 15) prove the justice of this conclusion. Nor are weight and length combined as co-variables adequate so to determine the perception that accurate comparative judgments are possible (see Figs. 4 and 14). Judgments of length were possible, since the adequate stimulus for the perception of length was present, but they were undifferentiated. Length, too, is out of the question.

Can we, then, state a *DL* in terms of center of mass? We have already remarked that the intensity-ratio of 'kick' to 'pressure' varies with every change in center of mass; and we have said that it is precisely this ratio that determines, psychophysically, the non-visual perception of length. The distance, in absolute terms, from center of mass to fulcrum-point at the forefinger varies directly with the intensity-ratio of 'kick' to 'pressure,' and the relation of this ratio to another of the same kind in two successive experiences determines the perception of comparative length. Have we here a moment in terms of which we may express a correlation between experience and stimulus-object?

In the first place, we note that changes in this intensity-ratio are effective for modifying the perception only within certain limits, the lower of which we have fairly established. The *O*'s failed to perceive length when the intensity of the 'pressure' became twice that of the 'kick' (series A III, B IIIa and F I). The upper limit we did not determine. In the second place, a number of other secondary factors enter in to influence the perception of length. We have already discussed at length the relation of absolute intensity, as a correlate of weight, to the perception. In addition there are the elements of time, the character of the pressure-gradient, and the general sensory background from the muscles, joints and tendons of hand, wrist and arm, all of which not only touch off the meaning of relative length but may actually modify the perception.

The results from the 'spoon'-series prove, nevertheless, that there can be no perception of length without the two pressures. Here the perception appeared only if there were movement of the rod; as the rod swayed or vibrated there was generated a duality of impression within the limited area of the single contact-point. There is one instance in which the two sensations seem at first sight to come in succession rather than simultaneously: the case of the rod balanced on the end of the finger. As the rod begins to tip over, there comes the knowledge that it is an extended object and not a compact mass. The pressures are, however, not really successive; pressure becomes intensified in one part and weakened in the opposite part. If the end of the rod be pressed obliquely against the skin there is no perception of length. It is only as movement begins, causing the inverse change of intensity in opposite parts of the stimulated area, that we perceive length.

Thus the primacy of the two related pressures forces itself upon us once more.

We thus arrive at the conclusion that the primary item of experience, the underlying and essential condition for the perception, is a ratio between two opposing pressures held together by certain directional and temporal relations. But *for this ratio there is no directly corresponding element or moment in the stimulus-object.* Center of mass, it is true, bears a constant direct relation to this ratio; only, if we have recourse to it, the physical terms in which we express the limen will be two steps removed from the essential item of direct experience. The primary psychological condition of the perception transcends the stimulus-object.

VII. On the Application of Weber's Law

As regards Weber's law, there is a characteristic of our curves of distribution which may have an indicative value. In a majority of cases, when we consider those series only which gave X's, the curves for the judgment 'longer' do not rise as high as the curves for 'shorter,' and most of the inversions and plateaus lie on the upper side of the standard. Our steps, whatever their nature, represent (except in section F) an arithmetical and not a geometrical progression. Stimuli which vary in this way should, under a strict working of Weber's law, give psychophysical results such as we obtained. Our interpretation of the perception of length as based upon a ratio of intensities is thus, so far, confirmed.

Conclusions

Our analysis shows that the non-visual perception of the length of a lifted rod is based primarily upon the experience of two opposed pressures whose intensive ratio falls within certain limits.

There are at least four factors which join with the two opposed pressures, singly or in combination, to complete the perception. These are absolute intensity of the impressions, their temporal course, the pressure gradient formed, and the muscle and strain sensations from hand and arm.

Visual and kinaesthetic images serve to establish the length in absolute terms. They may serve the end of comparison, but then imply a reflective judgment.

The perception is given as accurately in the passive as in the active state.

The psychological substrate of the meaning of length has no direct physical correlate in the stimulus-object.

The perception can be synthetically produced by the application, under certain conditions, of two opposed pressures.

A NEW FORM OF STIMULI FOR LIFTED WEIGHT EXPERIMENTS

By SAMUEL W. FERNBERGER, Clark University.

The questions regarding the form and material of the stimuli to be used in lifted weight experiments have been variously answered by different experiments. Fechner [1] placed his weights in holders which were grasped and lifted with the palm of the hand upward. Galton [2] used simple shotgun shells loaded to the proper intensity with successive layers of shot, cotton wool and wads. These weights were grasped by the subject " between his finger and thumb, the finger pressing against the top, the thumb against the bottom of the cartridge." There is a set of brass weights loaded with paraffin made in this form by the Cambridge Scientific Instrument Company, Ltd. Sanford [3] suggested the use of envelopes weighted with sheet lead, to be lifted vertically suspended between the thumb and forefinger. Presumably weights of this type were intended only for use in an elementary laboratory course. Fullerton and Cattell [4] employed flat wooden boxes weighted with shot and cotton, which were lightly grasped on the side with the thumb and fingers. Urban [5] used weights of the same general form made of hollow brass cylinders loaded with shot and paraffin, which were also lightly grasped on the side between the thumb and fingers. Inasmuch as paraffin is not entirely anhygroscopic, solder was substituted in a second set of weights exactly similar in form and size to those used by Urban.[6] A set of weights made of hard rubber and weighted with shot, advertised in 1894 by the Garden City Model Works, is mentioned by Titchener.[7] They consisted of 9 cylinders, 1½ inches in diameter and 4 inches high.

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[1] G. T. Fechner. *Elemente der Psychophysik.* 2nd Ed., 1889, I., 97f.

[2] F. Galton. *Inquiries into Human Faculty and Its Development.* N. Y., 1883, 34ff, 370ff.

[3] E. C. Sanford. *A Course in Experimental Psychology.* Boston, 1898, 413.

[4] G. S. Fullerton & J. McK. Cattell. *On the Perception of Small Differences.* Phila., 1892, 118f.

[5] F. M. Urban. *The Application of Statistical Methods to the Problems of Psychophysics,* Phila., 1908 1ff.

[6] S. W. Fernberger. On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli. *Psychol. Rev.,* XXI., 1914, 337f.

It seems curious that experimenters have overlooked the obvious fact that the size and form of the stimuli employed in this sort of experimentation may have an effect upon the measures of sensitivity. Experimenters have directly compared the results for lifting weights by the Fechner handles with the results for lifting by grasping the weight itself. It seems obvious that the manner of lifting a weight must affect the results. The processes underlying the formation of judgments in lifted weight experiments are exceedingly complex and involve pressure, muscular, articular and tendinous sensations with widely different points of origin,[8] which might be expected to differ with the various forms of weights and the manner of lifting them. In the face of ignorance the safe course is to use a single standard form. Weights of the size and form suggested by Urban have had a large and satisfactory usage in the hands of a number of recent investigators. They are comfortably grasped between the thumb and forefingers.

The materials of which the weights are composed also require standardization. In the first place the physical weight must be kept constant within the limits of error of the experiment. Urban[9] found that the wooden weights of Fullerton and Cattell varied, with differences in atmospheric temperature and moisture, sometimes more than 15 mg. The brass weights constructed by Urban and the writer overcome this difficulty. Such a set, loaded with solder, in use in the Clark Psychological Laboratory for the past eight years, has not shown for any single weight as much as 10 mg. variation.

Ide[10] has recently found that warm or cold stimuli are overestimated when compared with stimuli which do not arouse temperature sensations. Ide compared cold stimuli of 46° F and warm stimuli of 147° F. with neutral stimuli of 70° F. The resulting variations in the points of subjective equality for the method of constant stimuli are considerable; in some cases the difference is more than three grams.

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[7] E. B. Titchener. *Experimental Psychology.* N. Y., 1905, II., Part 2., 265.

[8] cf. S. W. Fernberger. An Introspective Analysis of the Process of Comparing. *Psychol. Mono.,* XXVI (Whole No. 117), 1919.

[9] Urban. *op. cit.,* 2f.

[10] A. L. Ide. *The Influence of Temperature on the Formation of Judgments in Lifted Weight Experiments.* Univ. of Pennsylvania Thesis, Phila., 1919, Pp. 25

Ide has investigated this problem systematically but the general observation appeared in the literature many years ago. In 1846, E. H. Weber[11] observed that a cold coin at —4° to 7° C. placed on the forehead seemed as heavy as two superimposed coins of the same size but of a temperature of 37° to 38° C. Some years later Szabadföldi[12] repeated this experiment but extended the range of warm temperatures beyond that employed by Weber. Szabadföldi found that both very cold and very warm stimuli are overestimated when compared with neutral stimuli which do not arouse temperature sensations. These results are entirely in accord with Ide's findings although in one case the experimenters were dealing with passive pressure and in the other case with active lifting.

In some recent experiments performed by the writer during the winter, the subjects sometimes complained that the brass weights felt cold. Metal readily gives rise to thermal sensations because of its high thermal conductivity. For this reason we have constructed a set of weights from hard rubber which has a low coefficient of conductivity, and which further recommends itself as anhygroscopic, durable and easily worked.

The coefficient of thermal conductivity (calories per cm. per sec. per degree) for yellow brass is about 0.20 and for vulcanite about 0.002.[13] In terms of stimulus and of sensation, if thermal conductivity were the only factor, this difference would mean that the range of temperatures which do not arouse temperature sensations would be 100 times as great for hard rubber as for brass.

Hard rubber seems to have been little used for lifted weights. We noted above one set of rubber weights. The Fechner weight holders listed by Spindler and Hoyer of Göttingen in their catalogue of 1910 have hard rubber handles. But neither of these forms of weights have been used in important experimental work. On the other hand, it has been almost universally recognized in aesthesiometric work that metal points arouse temperature as well as pressure sensations and that they are therefore to be avoided. Titchener,[14] for example, remarks in discussing the Griesbach aesthesiometer, —"The points are of metal. This is a disadvantage as it introduces the temperature error."

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[11] E. H. Weber. *Der Tastsinn und das Gemeingfühl.* 1846 and also Tastsinn in Wagner's *Handwörterbuch d. Physiol.,* Braunschweig, 1846, III., Part 2.

[12] M. Szabadföldi. Zur Physiologie des Tastsinns. *Unters. z. Naturlehre d. Menschen.* IX., 1865. For an account of these experiments *cf.* O. Funke. Der Tastsinn und die Gemeingefühl in Hermann's *Handbuch d. Physiol.,* Leipzig, 1879, III, Part 2, 320f. and also G. T. Ladd. *Elements of Physiological Psychology,* N. Y., 1887, 347f.

[13] *Cf.* A. Winkelmann. *Handbuch der Physik,* Leipzig, 1906, III, Part 1, 489 & 505.

It is difficult to discover in the literature the origin of the use of non-metallic materials to eliminate thermal sensations. Hall and Donaldson[15] in 1885, in discussing the influence of temperature in the perception of movement on the skin, say: —"It was soon found, however, that these limits overlapped, *i. e.* that an intermediate temperature of a metallic point could be so chosen that it would stimulate both heat and cold spots. We therefore had recourse to non-conducting cork points of such shape that the smooth and rounded edge of a right-angled triangle was applied to the skin transversely to the direction of motion. Thus the sensations of temperature appeared to be entirely eliminated." In 1887, Hall and Motora[16] quite casually speak of the use of hard rubber for the same purpose. In the manufacture of aesthesiometers, bone and ivory have also been employed as non-conducting materials to eliminate temperature sensations.[17]

The new set of weights in the Clark Laboratory conform to all the requirements which we have noted. They are hard rubber cylinders, 2½ inches in diameter and 1 inch high. Hard rubber can be purchased in round bars, just 2½ inches in diameter. The cylinders are cut off and hollowed out at one end to a depth of ½ inch; a wall at the sides ⅓ inch in thickness is left. A round brass plate, ⅛ inch in thickness, is inserted into this cavity and is held in position by two bolts and nuts which pass through holes bored in the top of the weight and in the brass plate. The heads of the bolts are counter-sunk into the top of the weight so that they are below the surface. A small number is stamped with a steel die on each weight for purposes of identification.

When the weights are calibrated, the brass plate is unscrewed and solder is melted on it until the rubber shell, the brass plate, the nuts, the bolts and the solder weigh just a little more than the desired amount. Some of the solder is then carefully scraped away until the desired weight is obtained.

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[14] E. B. Titchener. *Experimental Psychology.* N. Y., 1901, I., Part 1., 382.

[15] G. S. Hall & H. H. Donaldson. Motor Sensations on the Skin. *Mind.* O. S. X., 1885, 568.

[16] G. S. Hall & Y. Motora. Dermal Sensitiveness to Gradual Pressure Changes. *Amer. Jour. of Psychol.*, I., 1887, 74.

[17] *Cf.* A. Gemelli. Un nuovo estesiometro. *Atti. d Soc. Ital. d. Sci. Nat.*, LII., 1913, 193-200.

This present set of weights has been tested a number of times during a period of two weeks in which there have been great variations in temperature and humidity. In no case did any weight vary as much as 5 mg., a variation which is far within the limits of accuracy required in the lifted weight experiment.

Under experimental conditions these weights actually do not arouse temperature sensations. Even when placed for several hours on a very hot radiator or outdoors in air at 20 F., several subjects found that the hard rubber weights were only slightly cool and warm, whereas the brass weights were almost painfully cold and painfully hot. Presumably even unusual changes of room-temperature with the hard rubber weights will not arouse sensations of warmth or cold at all.

THE PSYCHOLOGICAL EXAMINATION OF CONSCIENTIOUS OBJECTORS[1]

By MARK A. MAY, Syracuse University

Psychological examining in the United States Army yielded a large body of results that may be considered as by-products. Most of these by-products will not appear in the official report which is now in the course of publication. In order that these incidental results be made available it has been thought wise to publish them as separate studies. The present study is a report made by the writer to the office of the Surgeon General, December 1918. It is an attempt to present in a summary fashion the available information concerning the intelligence, education, grounds of objection, social and political history of conscientious objectors..

The first psychological examinations of conscientious objectors were made in compliance with a letter from The Adjutant General of the Army dated March 6th, 1918. At that time there were no psychological examiners on duty at most of the camps. These first examinations were made by the psychiatrists and medical officers or by Special Boards. The reports of these early examinations are very diverse in character and extremely difficult to summarize. On April 22, 1918, Major R. M. Yerkes summarized these data in a report for the Secretary of War. The essential facts of this summary are embodied in this report. As soon as psychological examiners were on duty at most of the camps and cantonments a letter was sent out from the Surgeon General's Office, Division of Psychology, directing how the examinations should be conducted and the reports should be made. In June, 1918, a special examination for conscientious objectors was sent out to the camps. A copy of this examination appears in an appendix to this paper.

The data presented here are taken from about 30 reports sent to the Surgeon General's Office by the various psychological examiners. Some camps made several reports, others made one and some made none. The reports available for this summary cover about 20 camps and represent about 1000 objectors. This is a little less than one half of the total number of conscientious objectors in the army. From a careful examination of the available information the writer is of the opinion that the 1000 cases presented here is a fair sampling of the total number.

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[1] Published with the approval of the War Department.

The intelligence of conscientious objectors, as measured by the army mental tests, is on the average above that of the white draft of the army as a whole. By the army mental tests intelligence is rated on a scale of A, B, C+, C,C—, D, D—. The following table compares the intelligence ratings of the white draft of the army and the conscientious objectors.

		White Draft[2] (94,000 Cases)	Conscientious Objectors (1,000 Cases)
A	" Very Superior "	4.1%	8.7%
B	" Superior "	8.0%	15.2%
C+	" High Average "	15.2	22.6
C	" Average "	25.0	24.8
C—	" Low Average "	23.8	16.8
D	" Inferior "	17.0	8.7
D—	" Very Inferior "	7.1	3.1

These figures show that 46.5% of the conscientious objectors are above the grade of " C " which is *Average,* while in the army as a whole only 27.3% are above " C." And conversely 28.6% of the conscientious objectors are below " C " while 47.9% of the army as a whole are below " C." Another interesting fact about this table is that the ratio of A & B men of the army as a whole to the A & B men of the conscientious objectors is about 1:2. Conversely this ratio for D and D— men is about 2:1. There is absolutely nothing in these data to justify the hasty conclusion that objection to war is due to the low mentality of the objector. It may be true that some objectors can be classed as fanatics, others may be said to be stupid, but it can safely be said that at least 97% of the conscientious objectors reported here have sufficient intelligence to know what they are doing.

Educational data were available on 880 objectors. The following table shows the percentages of these men who reached the various grades in school.[3]

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[2] These figures are quoted from page 1076 of the Report of the Surgeon General to the Secretary of War, 1919.

[3] It is regretted that the educational data of the army as a whole are not available at this time. They will appear in " Psychological Examining in the U. S. Army," Memoirs of The National Academy of Sciences, vol 15 (In press).

Grade Reached	Per Cent.	Grade Reached	Per Cent.
0	100	H. S. I	42
1	99	H. S. II	29
2	98	H. S. III	24
3	97	H. S. IV	18
4	94		
5	88	Col. I	12
6	80	Col. II	9
7	70	Col. III	6
8	58	Col. IV	3
		Professional	.5

It will be pointed out presently that about 50% of the men in this report are Mennonites. Less than 10% of the Mennonites went beyond the 8th grade. The 12% who reached college were either Socialists, Dunkards or Friends.

The religious denominations of 1060 conscientious objectors are as follows:

Mennonites (Old and New)	342
Mennonites (Defenseless)	13
Mennonites (Swiss)	12
Mennonites (Amish)	99
Mennonites (Old Amish)	88
	554
Friends (Quakers)	80
Brethren, Church of the	67
International Bible Students (Russellites)	60
Dunkards	37
Israelites of the House of David	39
The Church of Christ	31
The Church of God and Saints of Christ, Etc	20
Seventh Day Adventists	20
Methodists	14
Pentecostal Assembly	13
Catholics	9
Assembly of God	9
Missionary Church	8
Pentecostal Church of the Nazarene	8
Lutherans	8
Christians (Campbellites)	7
Christadelphians	7
Holiness	6
Apostolic Christian	6
Baptist	5
Brethren (Plymouth)	5
Church of the First Born	4
Congregationalists	4
Presbyterians	3
Zionists	3

Other denominations of less than 3 are: True Light Church, Christian Science, Brethren in Christ, Baptists (Free Will) Evangelists, Baptist (Austrian), Baptist (United), Brethren (United), Christian Lights, Gospel Hall, Universalists, Stundists, Rosacrucians, Mazdazman, Brotherhood of Mystics, Ethical Culture Society. In addition to this list 55 cases are reported as having no denominational affiliations at all.

Attention has already been called to the fact that about one half of the conscientious objectors are Mennonites of one branch or another. The Friends, Brethren, Dunkards, International Bible Students (Russellites), and Israelites of the House of David constitute about 25% of the total. These taken with the Mennonites make up fully 75% of the conscientious objectors.

It is interesting to observe that all of these sects, with the possible exception of the Friends, believe in the literal interpretation of the Scriptures. As will be seen later their appeal is usually to the Bible and to their church creed. Moreover they are all more or less clannish. This is especially true of the Mennonites who live in colonies in the middle west. In some instances they constitute a large part of the population of one county.

The list of religions "organized prior to May, 1917 and well-recognized, whose creeds forbid participation in war" are relatively few. Such a list has been prepared by Mr. Hunt of the Census Bureau and is as follows:

Mennonites, 16 bodies (including the Amish);
Friends, (Quakers) 4 bodies;
Dunkards, three bodies;
Brethren, four bodies;
German Baptists; two bodies;
Churches of Christ;
Christadelphians;
United Zionists;
Assemblies of God;
Pentecostal Church of Nazarene;
Apostolic Holiness;
Fellowship of Reconciliation (not a religious sect);
International Bible Students Association (Not an organized sect).

The denominational lists have interest to the psychologist because in conducting an examination it was found necessary to know something of the religious background of the objector. (See the examination at the end of this article).

The grounds of objection to war are in general three, religious, social and political. The religious objector makes his appeal to the Bible, church creed and to conscience. The social objector makes his appeal to individual freedom. The political objector usually bases his objection on the ground of alien citizenship. Out of 958 cases, 90% object on religious grounds; 5% on social grounds; 3% on political grounds and 2% on ethical grounds. It is quite obvious that the problem of the conscientious objector is a problem of dealing with religions.

In many cases records were kept of the specific objections which were made. Such records were available of about 500 cases. Object Because:

War is forbidden by church and creed	115
War is forbidden in the Scriptures in general	125
War is forbidden by Christ	95
War is forbidden by the commandment	60
War is forbidden by conscience	120
War promotes evil	16
War is wrong in and of itself	21
War is capitalistic and Imperialistic	10
War is getting autocracy	7
Have friends among the enemy	7
Not citizens of this country	6

Other objections not occurring very often but which are rather interesting are:

Objector believes that he will lose his soul and go to Hell if he should disobey his church and his conscience and go to war.

Objector says he cannot obey God and Mammon. Thus he could not obey God and the army sergeant or captain at the same time.

Objector refuses to use carnal weapons. Prefers the sword of the spirit.

Objector refuses to participate in war because it involves hatred toward the enemy.

Objector believes that war is punishment of the unjust and the just are, therefore, exempt.

Objector fears that the temptations of army life will lead him astray.

Objector refuses to be "yoked with unbelievers" and thereby jeopardize his chances of salvation.

Objector believes that war is legalized murder.

Objector refuses to use physical force to the extent of separating soul from body.

Objector promised parents and pastor that he would not fight and feels that he must remain true to his promise.

Objector says " war makes the rich richer and the poor poorer."

Objector doubts America's motives.

Objector says he was not allowed to vote on the war.

Objector says that by staying out he will encourage socialism in Germany.

The political affiliations of about 340 objectors were secured. Of these 340 there were 45 Republicans, 62 Democrats, 22 Socialists, 26 Prohibitionists, 5 Independents, 2 Progressives, 3 not citizens, 3 other parties, and 75 no political affiliation at all. It is interesting that about one half this list have no political affiliations. Nearly all of these non-voters are either Mennonites, Dunkards, Brethren, or I. B. S. A., or Friends. But this is not as significant as it seems to be since it has been pointed out that about 75% of all the objectors belong to these denominations. The writer is of the opinion that there is no causal connection between this apparent disinterest in political affairs and objectors to war. It should be remembered that some religious denominations actually forbid the communicants to take any part in politics on the grounds that they must keep " unspotted from the world."

As far as the information goes very few objectors belong to fraternal organizations. The reports show that more than 90% have no fraternal affiliations. The ten per cent that do belong are scattered over a large number of organizations. Speaking generally one might say that the average conscientious objector is a non-voter, belongs to no social organizations, is not interested in community enterprises. The writer has examined more than fifty objectors and has not found a single individual who had even been superintendent of a Sunday school or held a responsible position in the community.

At the time of the psychological examination the examiner in some instances asked the objector what kind of service he would be willing to render. Reports in 718 cases show the following choices:

Willing to accept regular military service	16
Willing to accept non-combatant service	160
Willing to accept farm furlough	275
Unwilling to accept any kind of service that is any way connected with the military machine	267
	718

The Board of Inquiry with whom the final disposition of these men rested did not in all cases give the objector the kind of service he wanted. It did, however, consider the objector's preferences. The following table is quoted from W. G. Kellogg's book, The Conscientious Objector, page 127. It shows the final disposition made of 2100 cases.

Recommended for farm or industrial furlough......	1500
Recommended for Friends' Reconstruction Unit....	88
Recommended for noncombatant service...........	390
Assigned to general military service (insincere)....	122
	2100

The factors determining the kind of service an objector was willing to accept are hard to find. They appear to be neither religious nor intellectual. There is little difference in the average intelligence ratings of the men choosing these various forms of service.

Many psychological examiners made an effort to get at the objector's occupational history. In many cases these data would throw some light on the case. The occupations of 888 objectors were obtained. The more frequent ones are listed here.

Farmers	548	Printers	11
Clerks	40	Salesmen	11
Laborers	27	Miners	10
Machinists	20	Chauffers	10
Students	24	Bookkeepers	6
Teachers	22	Engineers	5
Carpenters	17	Factory Workers	5
Ministers	16	Electricians	4

A total of 90 different occupations is represented by these 888 objectors. They run the whole gamut from accountants to zinc workers including one saloon-keeper and a gunsmith. The most striking fact about this list of occupations is the large number of farmers. This in turn is due to the preponderance of Mennonites of whom 90% are farmers.

The available data on the birth and ancestry of conscientious objectors show that out of 720 cases 90% are native born. No accurate information is available on the ancestry of objectors but what is at hand seems to indicate that approximately one third were of American parentage, one third German, and one third English, Scotch, Russian and Scandinavian. The writer is extremely doubtful if parentage or birthplace has anything to do with an objector's refusal to take part in war. The large percentage of German parentage can easily be accounted for by the fact that most Mennonites are of German descent. Conscientious Objectors cannot be intelligently disposed of by simply calling them pro-German.

The degree of sincerity of a conscientious objector is a thing almost impossible to determine. It is difficult enough to find a test that will identify him, much less to find a measure of his sincerity. Of course, it was not the business of the psychologist to pass final judgment on the sincerity of an objector. But since the psychologist was often called upon to testify in courts-martial proceedings, to pass on an opinion to company commanders, and to furnish testimony to the Board of Inquiry it was necessary that he be able to formulate some opinion as to the sincerity of the objector. One of the chief purposes of the examination herewith attached was to aid the examiner in making up his mind. This examination seeks (1) to determine the objector's intelligence and to find out if he has any mental abnormalities. (2) His educational and occupational history is gone into with a view to getting at his interests. (3) Questions are asked about his church creed and his religious experiences to see how well informed he is. The assumption is that if an individual bases his objection to war on a church creed he ought to know something about it. (4) His moral habits and his social outlook are reviewed. In short the psychologist attempts to get as many lines as possible on the objector's character and social trustworthiness. All this evidence is put together when the psychologist makes his report. Such questions as "What would you do if a German should attack your Grandmother?" are carefully avoided. Questions of this sort are not tests of sincerity and show poor judgment on the part of the examiner. The non-resistance issue was not raised. It leads nowhere.

The Board of Inquiry passed on approximately 2100 cases.

The disposition of these cases was as follows:

Found to be sincere objectors and recommended for farm or industrial furloughs	1500
Found to be sincere objectors to combatant but insincere as to noncombatant service	219
Found to be sincere objectors of combatant but willing to accept noncombatant service	156

Found to be sincere objectors to combatant service but willing to work in reconstruction hospitals.........	15
Found to be insincere both as to combatant and noncombatant service	122

(This table is quoted from Mr. Kellogg's book cited above).

Unfortunately no reports are available showing how well the psychologists' recommendations correlated with the findings of the Board. It was the writer's experience in examining objectors that after carefully applying the examination little doubt remained in the mind of the examiner as to the objector's sincerity or insincerity.

Some general conclusions regarding the psychology of conscientious objectors may now be given. In the first place conscientious objection to war cannot be attributed to inferior intelligence. While it may be true that some religious sects are below the average intelligence of the army, yet they are not far enough below it to have any effect on objection to war. Taken as a whole the conscientious objectors are far superior in intelligence to the white draft of the army taken as a whole. Neither can objection to war be laid at the door of abnormal mental states. It is true that some objectors exhibit mental instability, possibly some are religious fanatics, yet a great majority of them are sane. Moreover conscientious objection to war is not a special case of the " blind leading the blind." Many objectors adhere rigidly to their creeds and follow the instructions of their pastors and so on, but many of them are keen thinkers. They are quick in debate and logical in their arguments. Mental questioning rather than unquestioning is more often the cause of objection.

From a survey of the available data three types of conscientious objectors stand out rather clearly. First there is what may be called the *religious-literalist* type. To this class belong most of the Mennonites, Dunkards, some branches of the Brethren and many of the obscure denominations mentioned above. If an objector of this type were picked at random he might be characterized as follows: He is a Mennonite, born on a farm in a middle Western community. His parents together with others of their faith settled there many generations ago. He went to the country church school. There he was taught the Bible and some simple rules of living. He quit school at the 8th grade and went to work on the farm. He attended church more or less regularly. He was taught to respect his minister and his views of life. He perhaps read the county newspaper and the Bible, outside of this he has done little reading. His information is limited to the happenings of his own community. He knows that we are at war with Germany but has no idea why. He has no social vision, no national consciousness. So when creed, minister, parents and friends tell him that war is wrong and that he must not fight, what could be expected of him ? For a man like this not to be a conscientious objector would violate all the laws of heredity and environment that operate to make men pursue certain courses of action. This type of man constitutes fully 75% of the conscientious objectors.

Secondly, there is the *religious-idealist* type. His appeal is not to church or creed, although he is usually a church member. He appeals directly to conscience and to the teachings of Jesus. He was probably born in a city or an enlightened community and is probably a high school or college graduate. Some men of this type hold advanced degrees from large universities. He is a true Christian and has for years tried to follow closely the example and teaching of Jesus. He is very intelligent, in fact, he may measure up with many officers on the army intelligence scales. He is a thoroughgoing idealist and is entirely unwilling to sacrifice his ideals by resorting to expediency. He perhaps does not have the support of his pastor or friends. He has but one principle of life and that is the principle of love. He believes in applying this principle even in dealing with his enemies. In most cases he refuses to make any compromises and for this reason he is the type usually found in the Disciplinary Barracks. The fault with his type is not that he does not have social vision, but that he has too much.

Thirdly, there is the *socialist* type. He is likely to be well educated and very intelligent. He is well informed on the vital social and economic problems of the day. His patriotism knows no national limits. Here again it is not that he is not patriotic, but he is unusually so. He believes in the Brotherhood of Man, a United States of the World, a League of Free Nations, an ideal state of society in which all shall have equal rights. He is not willing to fight the proletariat of Germany just to get at the ruling classes. He may even believe in the use of force and in fighting, but it is capitalism and imperialism that he wants to fight. The problem with him is that he entirely fails to appreciate "national" problems. The last two classes of objectors constitute about 25% of the whole.

APPENDIX

EXAMINATION OF CONSCIENTIOUS OBJECTORS

This examination outline is supplied to every examining staff in order that it may be used as seems desirable. Its use is not ordered but it is believed that it will greatly assist examiners who lack experience.

Purpose:

1. To determine the degree of intelligence of the conscientious objector, with a view to finding out whether his objections are due to any abnormal mental conditions.
2. To obtain the *specific grounds* of the conscientious objector's objections, and to see how well grounded he is in them (not how consistent).
3. To ascertain the extent of the conscientious objector's information concerning the history and tenets of his faith, and of the organization (if any) to which he belongs. This is for the purpose of ascertaining his objections in somewhat the same way as that employed by Personnel Officers with reference to skill in occupations.
4. To determine the degree of the conscientious objector's objections; just what he is and is not willing to do in the war; e. g., wear the uniform, take the military oath, enter non-combatant service or non-military service, reconstruction, etc.
5. To determine the degree of his sincerity.

Note:—The following examination is worked out with a view to covering as many kinds of cases as are likely to occur. It is not necessary that the entire examination be given to any one man. As soon as the examiner has reached the point where he can be reasonably sure that he has sufficient information to satisfy the above requirements, the examination should cease.

I. *Mental Age Examination.*

If the subject speaks English, use Stanford-Binet or Point Scale; if illiterate or non-English speaking, use Performance Test. In any case give as many tests involving verbal responses as possible. Write down all responses in full. Watch carefully for all signs of malingering, for responses that indicate a psychopathic condition, and for responses that have a definite religious trend. Calculate the mental age at once, and let your subsequent questions be governed by the degree of the subject's intelligence.

II. *Personal and Family History.*

1. Are your parents living? If not, state cause of death.
2. Any mental disease or peculiarities existing in family?
3. What blood relationship exists between parents?
4. Have you ever been troubled with sleeplessness, excessive worry, discontent, or any nervous disability?
5. Have you ever had any serious disease or physical defects?

III. *Educational History.*

1. Where did you go to school? How long?
2. How old were you when you quit? Why did you quit?
3. What subject interested you most?
4. How did you get along in school?
5. What books and magazines do you read most now?

IV. *Occupational History.* (Important, if subject is Socialist.)

1. What was your occupation just before joining the Army? How long did you work at it ?
2. How many different jobs have you had in your life? Where? Why changes ?
3. Do you belong to a trade union ? Why or why not ?
4. Did you ever participate in a strike ?
5. Did you earn enough money to live comfortably ?

V. *Religious History and Practices.*

1. What is your denomination ? What branch ? How long have you been a member? Do you have certificate of membership?
2. To how many different denominations have you belonged? Why changes?
3. When, where and how converted? How many times? (Note especially specific religious experiences, such as visions, divine messages, etc.)
4. What is religion of father, mother, brothers and sisters ?
5. Have you ever studied for the ministry ? Every preached ? How long ? Where ?
6. Do you read the Bible daily? How much? What parts interest you most ?
7. What religious books other than the Bible do you read ?
8. How often do you go to church ?
9. How often do you pray ? For whom and for what ? Do you pray for the Kaiser ?
10. Have you ever got direct answers to prayer ?
11. Did you attempt to convert others to your religion ?

VI. *Moral History and Habits.*

1. Are you a sex offender? (Reformed?) (Why?)
2. Do you smoke or drink? (Reformed?) (Why?)
3. Do you swear? (Reformed?) (Why?)
4. Do you gamble? (Reformed?) (Why?)
5. Do you contract debts? (Reformed?) (Why?)
6. Do you pay debts?
7. Do you lie? (Reformed?) (Why?)
8. Do you steal? (Reformed?) (Why?)
9. What do you consider your worst habits ?

VII. *Social History.*

1. Have you ever held an office in your community or church ?
2. Do you belong to any secret societies? Ever held office?
3. Are you interested in community enterprises ? (Good roads, etc.)
4. In what ways have you wanted to make your community better ? Or are you satisfied to let every man take care of himself ?
5. Have you ever been before a court on a charge? In jail?

VIII. *Grounds and Degree of Objection.* (If the objections are on religious grounds, follow A.; if on social or political grounds, follow B.)

A *Religious Objections.*

1. Degree of objection.
    a. Do you object to all wars or only to this war?

- b. Would you take the military oath?
  wear the uniform?
  work in reconstruction?
  do non-military work, such as farming?
  enter a non-combatant military organization?

2. Specific objections.
   - a. Do you object to war because your church forbids it?
     - i. Why does it forbid it?
     - ii. How long has it forbidden it?
     - iii. Have any members of your church ever taken part in war?
     - iv. Do you know any of your fellow-members who have been drafted? What did they do?
     - v. Would you take part in a war to defend your religion?
   - b. Do you object to war because the commandment says "Thou shalt not kill?"
     - i. Do you literally obey the other commandments?
     - ii. Do you consider all killing murder?
     - iii. How do you account for divine sanction for war in the Old Testament?
   - c. Do you base your objection on passages of Scripture found in the New Testament? What are they?
     - i. Do you live up to the teachings of Jesus in all respects?
     - ii. What do you think Jesus would do if he were drafted? Why?
     - iii. What is your favorite passage?
   - d. Do you object to war because you think that if you went to war and were killed, you would go to hell?
   - e. Do you object to going to war because you are chosen of God not to fight with carnal weapons?
     - i. Do you think it is wrong for me to fight?
     - ii. How do you account for this war in God's plan for the world?
   - f. Do you object to war because war involves hate? Makes deception legitimate? Impedes thereby the coming of the Brotherhood of Man?

3. Information concerning the History and Tenets of his Faith.
   - a. How many members has your church as a whole ? where mostly?
   - b. Who was the founder of your faith ?
   - c. When were they first called by their present name ?
   - d. What sacraments do they believe in ?
   - e. Is the creed of your church written down outside the Bible?
   - f. What does your church do to discipline disobedient members?
   - g. What is its attitude towards the civil law ?
   - h. Does your church believe in education ?
   - i. Does your church co-operate with other religions ? Y. M. C. A.?

1. Degree of objection.
   - j. Does your church permit marriage outside the church ?
   - k. Does it believe in the Resurrection of the Body ?

B. *Objections on Social, Economic and Political Grounds.*
    a. Do you object to all wars or only to this war ?
    b. Would you take the military oath ? wear the uniform ? work in re-construction? do non-military work, such as farming? enter a non-combatant military organization?
2. Specific objections.
    a. Do you object because this is a capitalistic and materialistic war? In what ways?
    b. Do you object because the war is not worth the expense?
    c. Do you object because the war is not getting Democracy, or rather is bringing about a German Autocracy, in this country?
    d. Do you object to being drafted for a defensive war, or is your objection to being drafted and sent to Europe?
    e. Do you object because the people did not vote on the war question?
    f. Do you object because you refuse to be swayed by mob psychology and prefer to be in the minority?
3. Questions to determine how well grounded subject is in his views.
    a. How long have you held these views ?
    b. What social organization are you a member of ? How long? What is its peace program?
    c. How did you come to be a Socialist ?
    d. What books and magazines do you read ?
    e. As a Socialist what do you consider your personal duty towards the war?
    f. Do you believe that by staying out of the war you encourage your social brethren in Germany in a social revolution? Why has there not been such a revolution, already?
    g. Do you believe in police force and enforced arbitration?
    h. Do you claim protection under the law, the right to vote, freedom of speech ? Do you think that such liberties were obtained by war ?
    i. Are you in accord with President Wilson's peace aims?
    j. Why do not all Socialists oppose the war ?
    k. What has been the attitude of Socialist parties here and elsewhere on past wars ?
    l. What efforts do you make to spread the Socialist view ?
4. Questions to ascertain the extent of subject's information concerning Socialism.
    a. What is the Socialist view as to the cause of the war ?
    b. What are the Socialist war aims ? Peace program ?
    c. What practical things have the Socialists done to end the war ?
    d. What is the attitude of the German Socialist towards the war ?
    e. Name some prominent Socialists here and abroad and give their viewpoints.
    f. What is the Russian peace formula ? Attitude of Germany, England and France thereto?
    g. What do you know about the Stockholm conference?
    h. To what extent has the war furthered the cause of socialism here ?

THE VOWEL CHARACTER OF FORK TONES

By A. P. Weiss, Ohio State University

Introduction

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Introduction. This set of experiments was suggested by a series of observations made by Koehler (2) in 1910 while he was investigating the motions of the tympanic membrane during auditory function. In the analysis of the photographic records of the vibrations secured by this method it was found that when the vowel ō (fold)[2] was sung at about 500 vibrations. the form of the sound wave approached a pure sine curve. At about 200 vibrations the sound wave for ō (fold) became very complex, but the partial corresponding to the frequency 500 became prominent while the fundamental 200 almost entirely disappeared. This fact led Koehler to conclude that pure tones had an attribute or property which he designated as '*Vokalcharacter*' and that this attribute changed in quality at intervals of about an octave. Thus the '*Vokalcharacter*' of the tone 265 is ū (soon), that of 526 is ō (fold) and that of 1066 is ā (far).

In going over Koehler's results in a limited way it was not possible to differentiate these vowels unequivocally. From his familiarity with the work of Meyer (5) the writer wondered whether Koehler had not merely made a more intensive study of what Meyer had described as 'quality' and which was regarded as a continuous series ranging from the 'mellowness' of the lowest tones to the 'shrillness' of the highest tones with intermediate degrees of mellowness and shrillness for the intermediate vibration frequencies.

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[2] The designation of vowels will be in the Revised Scientific (National Education Association) Alphabet Key. **For those unfamiliar** with this alphabet short English words in parentheses indicate the sound of the vowel. For reference to key see (1) in list of references.

Experiments were planned bearing on this question. One investigation aimed to determine the vowel character as judged by untrained or partially trained subjects to get some idea of the acuity of discrimination in general. The orientation seemed desirable so as to plan a second experiment using refined apparatus and specially trained subjects with an intensive technique. The experiment with the untrained subjects was started and finished while work on the intensive apparatus was progressing. The latter was brought to a stop by the war which made it impossible to secure the quality of tuning forks necessary for the work. The writer regrets that the intensive results as originally planned cannot be included in this study; but since there is no prospect of getting the forks and the apparatus within a reasonable time, the qualitative results are presented separately for the benefit of those who already have the facilities and interest for the quantitative aspect.

Statement of the Problem. The experiments were designed to reveal the extent to which tuning fork tones resembled the vowels used in language. As a supplementary phase of the experiment there was also opportunity for considering the terminological aspect of the psychology of audition. The teaching of the attributes or simplest discriminations in hearing presents considerable difficulty to students, a difficulty which is perhaps only due to ambiguous or unfamiliar terminology. The divergences between musical and psychological practices in describing auditory phenomena are so great that some investigators think it hopeless to try to reconcile them. Among acousticians themselves the situation is not much better. A random list of the terms used by various investigators to describe the attributes of audition are: pitch, quality, tonality, vocality, tone-color, intensity, duration, volume, brightness, extent, clearness and local signature. Does this list of a dozen terms mean that twelve attributes may be discriminated? No investigator would maintain this. Does any particular term (say quality) mean the same for all investigators? Again the answer is negative. Indeed the pessimistically inclined would maintain that any one of the dozen terms may mean any one of the other eleven.

Surely if experts cannot agree, then to learn the opinion of the naïve subject cannot add appreciably to the confusion. Whether the ability to detect a resemblance to vowels in fork tones is best described by the term quality, vocality, tone-color, brightness, volume, or any other term, will be determined in part by the ease with which untrained subjects can consistently discriminate vowels. If more consistent results are secured when the subject is asked to discriminate the vowel character than when he is asked to discriminate the brightness, quality or tone-color, supposing these all refer to the same fundamental attribute, then obviously vowel character or vocality is much the better term to use.

To test the value of a term, untrained subjects may often be used more effectively than trained subjects with special habits and prejudices toward a terminology developed under circumstances deviating very widely from the average class room conditions under which psychology is taught. Because of the lack of agreement on terminology no attempt will be made to determine whether the discriminations made by the subjects are attributive or cognitive in character. It is maintained only that an habitual or practical method of discrimination is demonstrated to the degree that there is consistency in the vowels that are assigned to the various fork tones.

Since performing these experiments Meyer (4) has suggested the term *vocality* as a substitute for his earlier term *quality* and as a shortened form of *vowel character.* It is in this sense that we shall use the term vocality in this article. Titchener (8) seems disposed to adopt the term vocality in the sense in which Meyer suggests, but Titchener has retained the term *volume* which is regarded as an attribute independent of the vocality. Rich (10) regards *pitch* and *volume* as independent attributes, but does not indicate the relationship between vocality and volume.

Simply stated our problem is to determine how consistently relatively untrained subjects can discriminate vocality in tuning fork tones and what utility the term vocality has in describing a fundamental fact in audition.

I. EXPERIMENT WITH ELEMENTARY STUDENTS

CLASS EXPERIMENT

A class experiment was given the last week in November, 1913 at Ohio State University to 150 students mostly freshmen and sophomores taking the course in elementary psychology.

Experimental Method. Ten Koenig tuning forks mounted on resonance boxes were placed at the front of the lecture room. The vibration rates of these forks were 128, 256, 320, 384, 512, 640, 768, 896, 1024, 1152 double vibrations. The subjects were instructed as follows.

> Each fork will be sounded twice. The first time it is bowed merely listen, eyes closed, and form a provisional judgment as to which vowel in the English language the tone most nearly resembles. The second time the fork is sounded make a final judgment and record a short English word which contains the vowel sound. The list of vowels and words on the board are to be regarded as merely illustrative. They do not include all the vowel sounds in the language nor all that may be used in reporting the vowel character of the fork tones.

The following is the list of vowels and words that was placed on the board:

e as in feel,	*a* as in sale	*o* as in tone	*u* as in moon,
a as in far,	*oi* as in boy,	*yu* as in cure.	

The tones lasted about three seconds. The recording was done on a narrow strip of paper, beginning at the bottom. As soon as the word was written it was folded out of sight. The next word was placed on top of the folded portion and it in turn was folded down. In this way there was no copying or referring back to previous judgments. In the presentation high and low forks alternated indiscriminately.

The term *series* as used in these experiments means a single presentation of each of the ten forks. A *discrimination* or *judgment* means that a fork tone has been sounded one or more times and that the subject has reacted to the tone by reporting, either orally or in writing, that vowel which in his opinion most nearly resembled the fork tone.

While performing the experiment it was noticed that the reactions were delayed and hesitating. Judging from the facial expressions the discriminations were difficult to make during the earlier part of the experiment but toward the end the reactions were more prompt.

After one week the experiment was repeated. This repetition is designated Series B; the original presentation as Series A. The second series was not expected by the students. The repetition was made to determine whether a second series would reveal greater uniformity and thus indicate some practice effect. The method was the same as for Series A. The results of both series are recorded in Table I. To enable direct comparisons with the subsequent experiments in which the number of subjects and the number of series are different, all results have been converted into percentages.

TABLE I

Class Experiment. Two Series

First Series = A. Second Series = B.

One hundred and fifty elementary students. The values in the table represent percentages. Thus the first value 48 means that for the fork 128 in Series A, the vowel ū (soon) was reported in 48 per cent of the 150 judgments.

Forks	128		256		320		384		512		640		768		896		1024		1152	
Series	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B
ū (soon)	48	86	10	18	12	20	8	16	3	0	6	0	5	..	10	0	8	0	2	0
iū (cure)	6	0	10	0	10	0	4	0	6	0	11	0	8	..	8	0	6	0	5	0
ō (fold)	8	12	14	34	11	19	8	34	10	0	7	0	8	..	6	0	5	0	7	0
oi (boy)	4	0	7	0	9	0	10	0	14	0	10	0	14	..	8	0	5	0	8	0
ä (far)	3	0	4	12	9	31	9	43	3	3	4	2	9	..	5	0	6	0	6	0
ai (fine)	1	0	3	0	9	0	15	0	24	29	22	25	18	..	14	33	16	42	18	31
ē (they)	4	0	22	34	9	27	14	7	10	35	7	20	10	..	14	34	9	12	12	13
ī (see)	15	0	15	0	14	0	15	0	14	33	14	50	15	..	17	33	21	46	18	53
failed	11	2	15	2	17	3	17	0	16	0	19	3	13	..	18	0	24	0	24	3

Last line 'failed' refers to the cases in which no judgment was given.
Average failures for Series A = 17 per cent; Series B = 2 per cent.
Average number of vowels assigned per fork: Series A = 8; Series B = 3.
In Series B, fork 768 was not available.

Table I. In Series A the failure to assign a vowel to the fork tones was nearly eight times as great as in Series B. The 17 per cent failures in Series A are not excessive however. If the same subjects had been asked to discriminate between ten degrees of saturation in colors, irregularly and successively presented, the failures probably would have been greater. That the failures decreased to 2 per cent in Series B indicates a rapid increase in skill with practice. In Series A the forks 128, 256, 512, 640 and 1024 have all of the eight vowels assigned to them, but some one of the vowels was used in more than one-fifth of the total number of judgments. In Series B the average number of vowels per fork was only three and every fork except 320 has more than one-third of the judgments given to some one vowel. This again implies a rapid increase in the uniformity of the reactions.

Nature of the Vowel Reaction. In Series A the percentage of ī (see) for all the forks was unusually large. The experimenter could not at first account for this fact. By bowing the forks gently the tones are so pure that no unpracticed subject would detect overtones and since the percentage was practically the same for all the forks it did not seem likely that the difficulty was in the source of the sounds.

It occurred to the experimenter that the subjects were not only discriminating the particular vowel character of the tone but were also introducing an additional ī (see) vowel because they did not pronounce the vowel character of the fork tone orally. If for instance a fork whose vowel character resembles ō (fold) is sounded and an attempt is made to pronounce or sing the *o* silently, some consonant will be spontaneously supplied by the whispering and any vowel component of this consonant would be especially emphasized because of the instructions to differentiate the *vowel* character. Thus if the consonant *h* is supplied to the *o* to make *ho,* the *h* would not be differentiated as *h* because of this emphasis on the *vowel* character. If *h* is regarded as a semi-vowel, its silent pronunciation would resemble the oral pronunciation of *heee* and this in connection with the emphasized vowel *o* would give *heeeooo.* The *heee* would form a part of every fork tone and thus increase the frequency of the ī (see) reaction for all the forks.

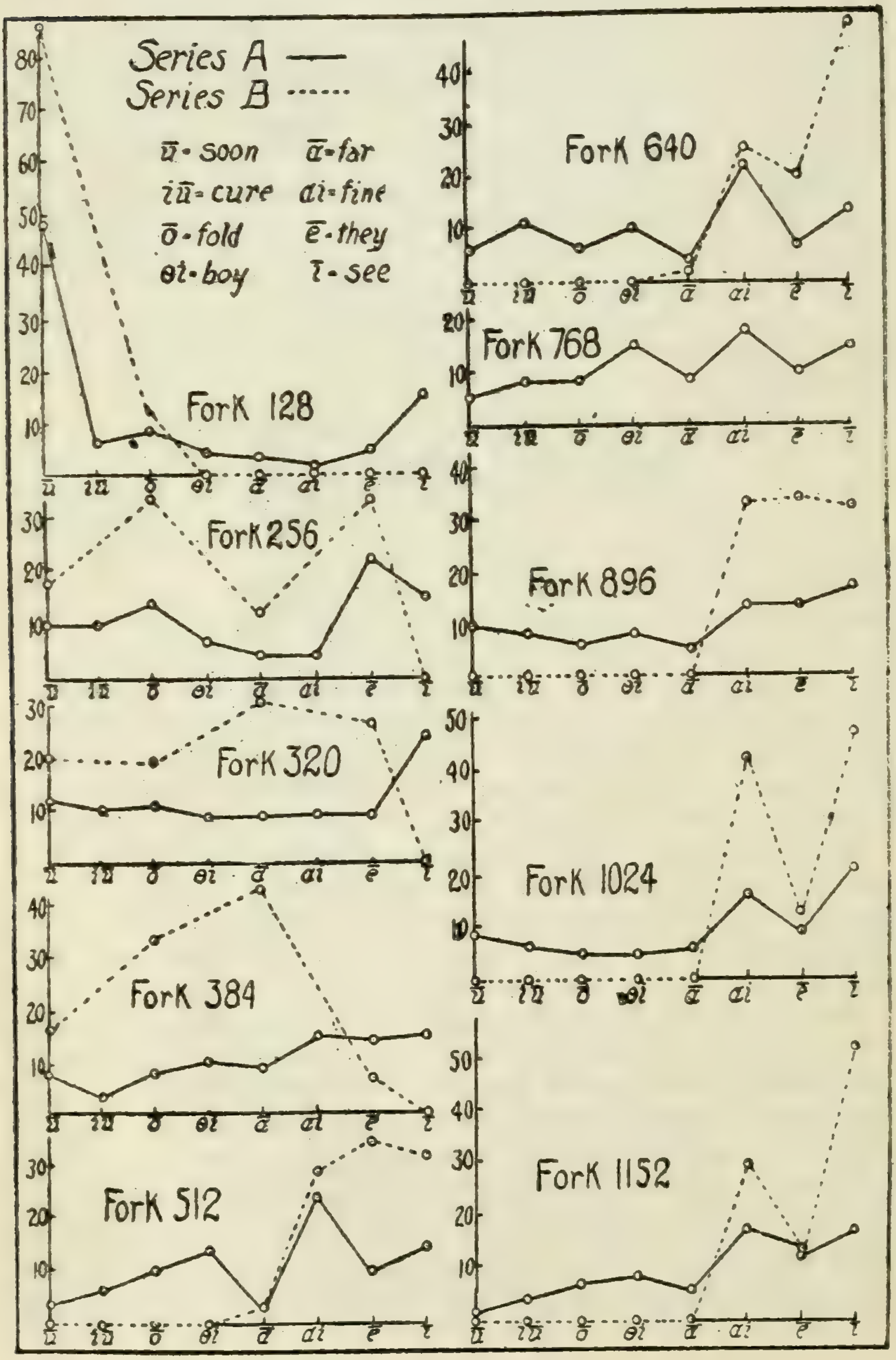
To restrict the analysis to the vowel character only, the experimenter, before Series B was given, called indirect attention to the consonants by stating that the vowel character of the fork tones may often be more easily analyzed if the subject will add consonants to the vowel sounds as is done in language. Thus we have pop, ish, peep, ho, ah, chirp, etc. It was hoped that by indirectly introducing the consonants their effect would be abstracted from the discrimination of the vowel character. The device seems to have been successful since the anomalous ī (see) distribution of Series A is absent in Series B. It was also pointed out that the sounds oi (boy) ai (fine), were not single vowels but diphthongs. This resulted in the elimination of oi (boy) and a restriction of ai (fine) to fewer forks.

Curves of Table I. The relation between Series A and B with respect to uniformity is best indicated by the curves. Each of the ten forks is plotted separately for both series. The vowels given on the base line are in the same order for all the forks.

If the vocality is independent of the vibration rates, the curves should approach the equation $y = k$ as a limit, approximately as is shown by the Series A curve for fork 320. Considering the Series A curves as a whole however, they would not be classed with the horizontal line type of curve. Series B is still further removed from the straight line type and the curves begin to give evidence of having definite maxima which in general augment the maxima of Series A. The maxima are toward the left for the lower forks; nearer the

CURVES I

ELEMENTARY STUDENTS. CLASS EXPERIMENT. VALUES TAKEN FROM TABLE I. EACH FORK PLOTTED SEPARATELY. VERTICALLY ORDINATE REPRESENTS PER CENT. VOWELS INDICATED ON BASE LINE



center for the intermediate forks; and toward the right for the higher forks.

The order of the vowels follows the series given by Koehler (3) : *m*, ū, ō, ā, ē, ī, the diphthongs not given by Koehler were interpolated.

Flexibility of the Vowel Series. In brief supplementary experiments not reported in this article it was found that when the subject is asked to determine the vowel character of a series, he usually assigns ū (soon) to the lower tones, and ī (see) to the higher no matter what the range of the series may be. There seems to be a tendency to condense or expand the vocality series to conform to the vibration limits of the tones used in the particular experiment.

If more intensive work will substantiate this it would mean that vocality is only partly dependent upon the absolute vibrations. Builders of musical instruments have adopted this partial dependence in the principle of 'voicing.' An organ builder may take two reeds both of 512 vibrations and voice one of them mellow and the other shrill, but he cannot voice the 512 reed so that it will be as mellow as a 128 or as shrill as one of 2048 vibrations. Similarity in voicing is restricted to a limit beyond which it is impossible to go. More space is devoted to this phenomenon under IX Discussion of Vocality.

II. Experiment with Laboratory Students

Individual Experiment[3]

To determine how the uniformity in vocality discrimination increases with subjects somewhat better trained in psychological observation, twenty-five series similar to those given the elementary class were given to six students taking the course in experimental psychology and two instructors in the department.

The ten forks were placed on a table in a room free from noise or other distractions and the subject sat about three feet away with his back to the forks. Before beginning, the nature of the experiment was explained and the subject instructed as follows:

> The pure tone from the fork is to be analyzed as to its resemblance to the vowel sounds used in language. If a fork tone does not resemble any

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[3] Mr. R. F. Bird, recently Fellow at the Carnegie Institute of Technology conducted the experiments with the laboratory students. His critical and painstaking tabulation of their data made it possible to add his results to those of Experiments I and III.

vowel with which you are familiar, the vowel or combination of vowels which most closely approximates the tone is to be given. The fork tones will be repeated as often as desired to enable you to make a judgment. The series of vowels on the card which has been given you is not to be regarded as complete, but only as indicating some of the sounds. Any other vowels may be used. If a vowel character seems to resemble two vowels, this is to be indicated by giving both vowels.

The following is the list of vowels and words that were placed on the card:

m as in hum, *a* as in ball *u* as in cue, *a* as in sale,
e as in feel, *o* as in tone, *i* as in high, *a* as in father,
i as in sit.

when the subject stated that the fork sounded equally like two vowels, as ū-ō for instance, this was counted as one-half for each of the vowels. Such double vowels occurred in less than one per cent of the total number of discriminations, and since the number of possible vowel combinations is large, each combination occurred so few times that no attempt was made to tabulate them separately. The most frequent combination ā-ē occurred only fourteen times.

Five series were given at a single sitting of about a half hour. The order of sounding the forks was varied for each series so the subject could not anticipate the next tone. The recording was done by the experimenter on previously prepared data sheets, the subject merely calling out the vowel and word.

When the first five series had been finished for the six subjects, the results were tabulated and it was found that all the vowel sounds that had been given could be grouped under the following vowels:

um as in hum
u as in cue
o as in hoe
a as in father

a as in sale
i as in sit
i as in high
e as in feel

a as in ball
e as in met
a as in cat
u as in full

The eight vowels in the first two columns were placed on a new card. The four vowels in the last column were omitted because they had been reported in less than two per cent of the cases. The revised card was then used for the rest of the experiment and the subject was requested to restrict his judgment to the vowels given on the card unless none of these were adequate either singly or in combination to express the vocality. In justification of this restriction it was found that the first eight vowels were in common use by all subjects including those who made use of the four vowels omitted.

During the early trials the subjects reported that the number of vowels at their disposal seemed too limited to describe all the fork effects. This difficulty was partly due to the unequal emphasis on the intensity and duration. Thus e (set) ē (they) are the same vowel differing only in length. A little practice however, soon enabled the subject to make due allowance for such variations but the more critical observers maintained to the last that when, for instance, two different forks were both reported to have the ō (fold) character this did not mean that the two *o*'s were the same. It implied only that the differences between the *o*'s could not be described in a consistent and satisfactory manner. Under IX Discussion of Vocality, further consideration is given to this limitation in the vowel analysis of fork tones.

TABLE II

EIGHT LABORATORY STUDENTS—TWENTY-FIVE SERIES FOR EACH SUBJECT—TOTAL OF 200 DISCRIMINATIONS FOR EACH FORK—THE VALUES IN THE TABLE REPRESENT PERCENTAGES—THUS THE FIRST VALUE 55 MEANS THAT FOR THE FORK 128 THE VOWEL UM (HUM) WAS REPORTED IN 55 PER CENT OF THE 200 DISCRIMINATIONS.

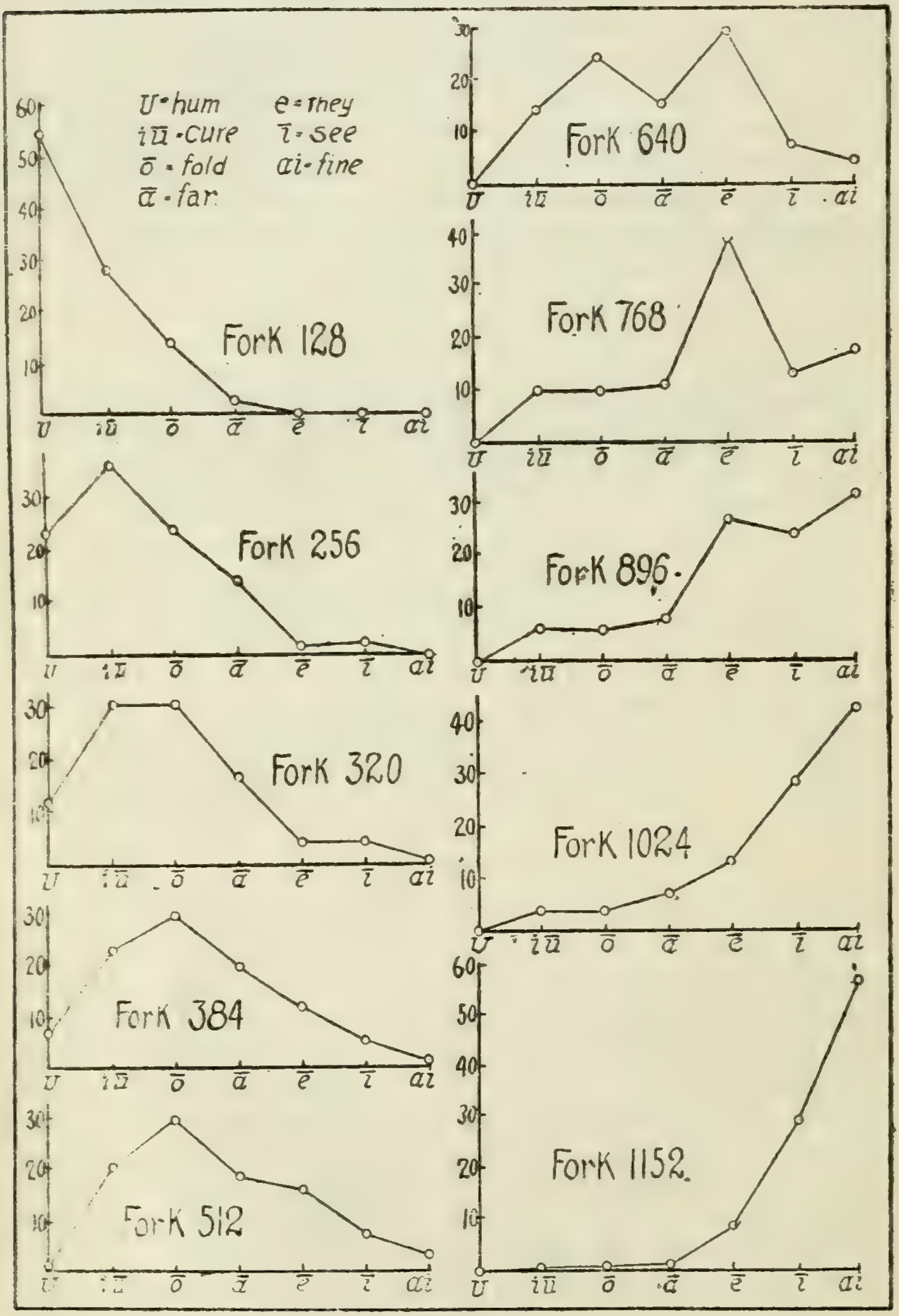
Forks	128	256	320	384	512	640	768	896	1024	1152
um (hum)	55	23	12	7	1	1	0	0	0	0
iū (cure)	28	36	31	23	21	15	10	5	4	1
ō (fold)	14	24	31	30	30	25	10	5	4	1
ā (far)	3	14	17	20	19	16	11	7	7	2
ē (they)	0	1	4	12	17	30	39	27	13	9
ī (see)	0	2	4	6	8	8	13	24	29	30
ai (fine)	0	0	1	2	4	5	17	32	43	57

Table II. There are no failures since each subject was free to have the tone repeated until he was able to assign some vowel. The percentages of any fork when read from top to bottom, or for any vowel when read from left to right, form a much more uniform series than for Experiment I with the elementary students. While the maxima are not as high as for Series B in Table I the geometrical relations between the series are more clearly shown. The maxima begin at one end for the lower forks (55 for 128) passing toward the center for the intermediate forks (30 for 512) and ending at the other extreme for the higher forks (57 for 1152).

The fork 640 seems to have two maxima, one at ō (fold) and the other at ē (they). This however does not interrupt

CURVES II

Laboratory Students—Individual Experiment—Values Taken from Table II—Each Fork Plotted Separately—Vertical Ordinate Represents per Cent—Vowels Indicated on Base Line.



the progression except in a way that might be expected when the intermediate vowels are less clearly differentiated.

Curves for Table II. The plan of plotting the curves is the same as for Curves I. The graph for fork 128 has its maximum at the extreme left; for fork 1152 the maximum is at the extreme right; and for the intermediate forks it approaches either the left or right according as their vibration rates approach the upper or lower limits of the series.

On the whole the curves show a much more orderly progression than those for Experiment I. This would be expected from the more extended practice of the subjects and the change in the order of the vowel progression. In Experiment II the diphthong ai (fine) is placed after instead of before the vowel ī (see) as in Experiment I. While phonetically *ai* (fine) is a combination of ā and ī, yet since it ranks third in the order of frequency it seems probable that it is perceptually a unit for most subjects. However, the difficulty in placing this diphthong seems to indicate that there is a partial analysis.

III. Experiment with Teachers of Deaf Children

Class Experiment

To determine how consistently the vocality of fork tones could be discriminated by subjects trained in the analysis of vowel sounds, a class experiment similar to that given in Experiment I was performed before thirty teachers of the State School for the Deaf[4] at Columbus, Ohio. These teachers taught oral speech to children of all degrees of deafness. To detect and correct speech errors in those who cannot hear their own voices requires special ability in the analysis of vowel character and this ability should manifest itself even though the vowel character of the fork tones varies so considerably from the vowels of actual speech that the teachers are unable to use their special training to its limit.

The experimental method was practically the same as in Experiment I. Ten series were given instead of only one. The forks were not seen by the subjects. The tones were presented at random, each fork being sounded twice,—the first tone for orientation, the second for final discrimination. The subjects were instructed to listen to the tone and then record a word, the vowel of which resembled the fork tone. If no vowel could be assigned nothing was to be recorded. Data sheets, ruled in squares with each square numbered were provided. At the top of the sheet were given the vowels:

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[4] The writer takes this opportunity to thank Superintendent B. H. Jones for his interest and suggestions, and the teachers for their patience and conscientious effort during a trying hour.

um as in hum, *a* as in father, *o* as in tone, *a* as in cat
a as in sale, *i* as in fine, *u* as in cute.

The subjects were to regard this list as merely illustrative. Vowels not in the list were to be used whenever necessary.

TABLE III

Thirty Teachers at the State School for the Deaf.—Ten Series—Total of 300 Discriminations for Each Fork—The Values of the Table Represent Percentages—Thus the First Value 28 Means That for the Fork 128 the Vowel um (hum) was Reported in 28 Per Cent of the 300 Trials

Forks	128	256	320	384	512	640	768	896	1024	1152
um (hum)	28	18	17	11	5	7	3	2	3	2
iu (cure)	16	15	18	11	9	21	14	13	10	7
au (now)	0	3	3	0	1	0	0	0	0	0
ō (fold)	10	10	11	8	10	16	5	4	2	0
o (not)	1	2	1	2	2	0	2	1	0	0
ā (far)	29	22	21	15	11	6	11	5	5	4
a (fat)	1	1	4	6	10	3	3	3	3	3
e (set)	0	1	1	5	7	6	4	5	6	4
ē (they)	2	4	0	10	4	8	5	7	5	6
i (sit)	2	2	1	4	5	8	20	20	17	13
ī (see)	1	2	6	3	7	7	11	20	30	32
ai (fine)	0	2	2	1	8	4	7	8	4	11
failed	10	18	15	24	21	14	15	12	15	18

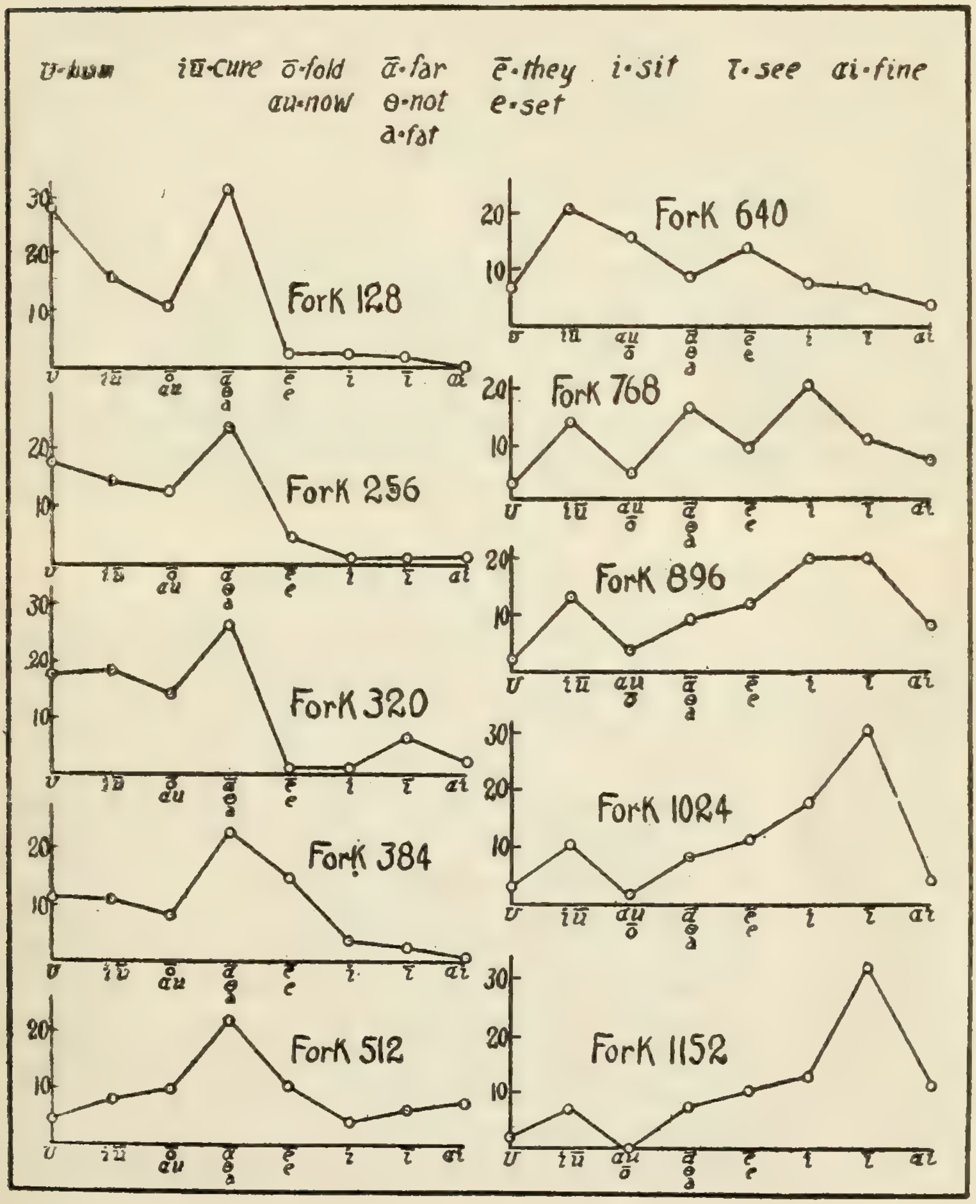
Average failures = 16 per cent.

Table III. Perhaps the characteristic thing in the data of the teachers was the greater variety of vowels used, twelve as compared with eight for the Series A of the elementary students. The short vowels o (not), a (fat), e (set), i (sit), were used much more frequently than in the preceding experiments. The frequency of these short vowels is considerably less than of the long vowels, but there is clear evidence that the influence of the duration of the sound on the character of the vowel has been discriminated. This would be expected since the quantity of vowels is one of the important factors in teaching a non-hearing student to speak.

The relatively large percentage of failures was due partly to the instructions not to record the instances in which it was impossible to decide upon the vowels, and partly to the novelty of being subjects in an experiment. The university students were more familiar with class demonstrations and laboratory work and the impressiveness of the occasion did not sit so

CURVES III

TEACHERS OF DEAF CHILDREN—CLASS EXPERIMENT—THIRTY SUBJECTS—VALUES TAKEN FROM TABLE III—EACH FORK PLOTTED SEPARATELY—VOWELS WHICH HAVE BEEN COMBINED ARE SHOWN UNDER EACH OTHER



heavily on their shoulders. Furthermore the attitude of 'trying to make a high score' was not so successfully eliminated with the teachers and this always has an inhibiting effect on the reactions of more mature individuals.

Curves III. To facilitate comparison with Experiments I and II the short vowels have been grouped with the corresponding long ones. The shapes of the curves correspond in general with the curves of Experiment I. The high frequency for ā (far) for the first five forks, seemed anomalous at first, but some of the teachers pointed out that this was the most ambiguous vowel of the series and where the discrimination was difficult this vowel served the purpose of a general utility vowel better than any of the others.

Considered as a whole the data may be regarded as complementary to the other experiments. It must be remembered that these teachers had not been trained for this particular type of discrimination. Their theories of vocalization, methods of teaching oral speech and past speech habits influenced their reactions to the auditory percept of the fork tones. Considering these factors it is rather remarkable that the results are as uniform as we find them. If these teachers had been asked to discriminate the series of fork tones into twelve degrees of quality, brightness or volume, with as little preliminary instruction as they received in discriminating the vowel character, the experiment probably would have failed.

IV. The Distribution of Vowels

To determine the differences between the vowel groups, the results of all three experiments were combined so as to include all the vowels given in Tables I, II, III. Such a combination will approach the average ability in vocality discrimination that would be expected from those subjects who are likely either as students or as teachers to exhibit a detailed interest in audition because of its practical or esthetic significance and to whose needs auditory terminology should be adapted if there are no scientific limitations. Thus if the term vocality is adopted it should not be difficult to explain to a musician what is meant by the ū-character of the lower end of the musical scale or the ī-character of the upper end.

No attempt is made to allow for differences in training, age, sex, experimental method and the variable number of series. Weighting separate experiments no matter how cleverly done, can never be a substitute for a uniform method in a single experimental plan. Yet since the aim is qualitative and the work is regarded as preliminary to future intensive work, such a combination will give a conservative sketch that will help in developing a fruitful methodology.

TABLE IV

Percentage of Vowels Assigned to Each Fork When Discriminations in Tables I, II, III are Combined

Forks	128	256	320	384	512	640	768	896	1024	1152	Rel. Fr.
um (hum)	26	14	11	7	2	3	1	1	1	1	7
iu (cure)	44	25	25	17	12	16	14	10	8	5	17
au (now)	0	1	1	0	0	0	0	0	0	0	0
ō (fold)	12	21	19	21	15	16	8	4	3	2	12
oi (boy)	0	1	2	2	3	2	4	2	1	2	2
o (not)	0	0	0	1	0	0	1	0	0	0	0
ā (far)	13	17	21	23	13	8	11	5	5	4	12
a (fat)	0	0	2	3	4	1	2	2	1	1	2
ē (they)	2	14	9	12	16	18	19	20	10	10	13
e (set)	0	0	0	2	3	2	2	2	3	1	2
i (sit)	0	0	0	2	3	4	12	11	10	9	5
ai (fine)	0	2	3	4	14	12	12	18	24	27	12
ī (see)	3	5	7	6	15	18	14	25	34	38	16
Total...	750	726	732	698	705	721	584	730	729	709	

Total includes only the actual judgments. Percentages are derived from actual judgments made, failures not included.

TABLE IVa

Data of Table IV Grouped Into Vowel Classes

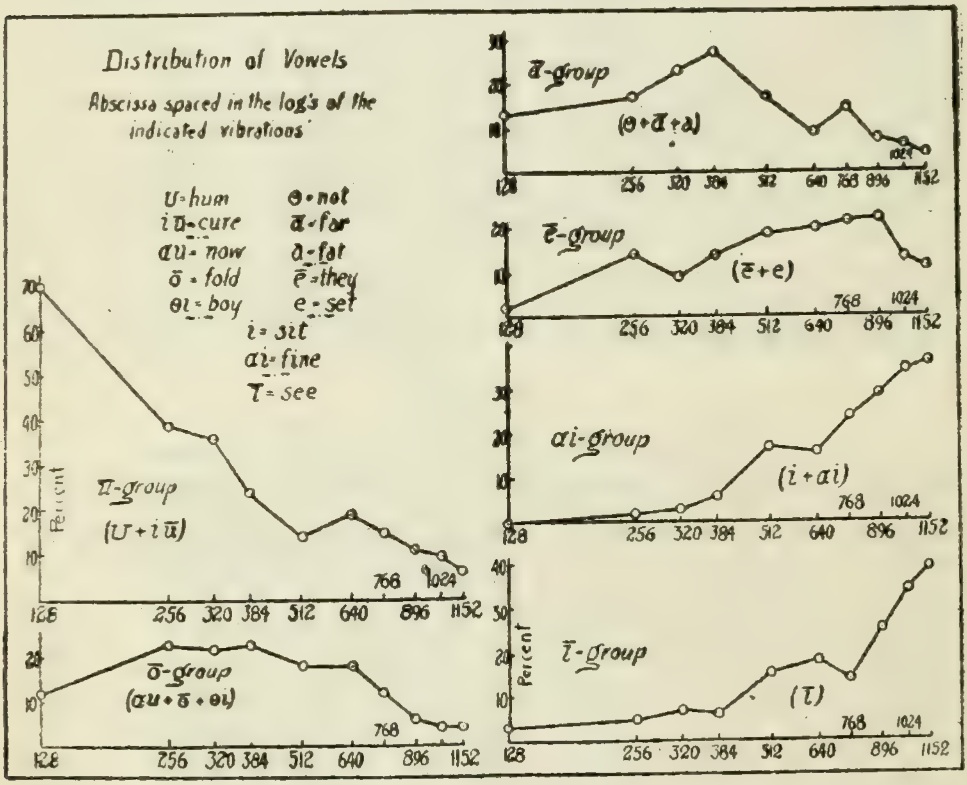
Forks	128	256	320	384	512	640	768	896	1024	1152	Rel. Fr.
ū-group	70	39	36	24	14	19	15	11	9	6	24
ō-group	12	23	22	23	18	18	12	6	4	4	14
ā-group	13	17	23	27	17	9	14	7	6	4	14
ē-group	2	14	9	14	19	20	21	22	13	11	15
ai-group	0	2	3	6	17	16	24	29	34	37	17
ī-group	3	5	7	6	15	18	14	25	34	38	16

Tables IV and IVa. All the vowels that were assigned in any of the experiments are given in Table IV. The last line marked 'Total' indicates the actual number of judgments only. If the failures had been included the total for each fork would have been 800. The low total of 584 for fork 768 is due to the fact that this fork was not available for Series B of

Experiment I. The last column (Rel. Fr.) gives the relative frequency of the vowels in per cent. Only six vowels have a frequency greater than ten per cent and these are selected as the more important classes. In Table IVa those vowels having a frequency of less than ten per cent are grouped together with those of the six important vowels to which they seem to belong. This gives the six *groups* of vowels shown in the first column. Each of these groups has a frequency that is large enough to determine the approximate vibration rate where the maximum frequency for a given vowel group lies.

CURVES IV

Combination of Experiments I, II, III. Showing Frequencies of the Vowel Groups at the Different Vibration Rates as Taken From Table IVa.



Curves IV. The curve of the ū-group shows its maximum very decidedly at the fork 128. For the ō-group no decided maximum appears although its position is probably between 256 and 384 vibrations. The ā-group has a decided maximum at 384 and the drop on either side seems to justify the assumption that its highest frequency is more toward the higher forks than the ō-group curve. The ē-group curve is somewhat ambiguous but in the reverse sense of the ō-group, its maximum seeming to lie between 512 and 896. The ai-group curve is similar to the ī-group; the maximum is at the highest fork used in the series, but shows evidence of having reached its highest frequency. For the ī-group the maximum is also with the highest fork but the slope of the curve at this work indicates that the point of highest frequency has not been reached.

A general consideration of the curves substantiates the statement made by Titchener (8) that the lower tones have a definite ū-character and the higher tones a definite ī-character.

V. The Consistency Between Vowels

The consistency in making the discriminations in these experiments approaches two limits:

1. When the consistency is least a subject will assign any vowel taken at random from his repertory of vowels every time he is asked to report the vowel character of any tone.

2. When the consistency is greatest a subject will assign the same vowel to the same fork every time it is presented.

The consistency may be measured by squaring the number of times a given vowel is assigned to a given fork. Thus if out of ten trials with fork 128 a subject reports 'u' *five* times, 'o' *two* times, and 'um' *three* times, the sum of the squares of the frequencies (5, 2, 3) is $25 + 4 + 9 = 38$. The number 38 measures the consistency.

If the subject had reported only a *single* vowel for all ten times, this would give a frequency of 10 for one vowel and the sum of the squares (since there is only one vowel) is $10^2 = 100$. The value 100 represents the upper limit or maximum degree of consistency.

If the subject assigns ten *different* vowels, each vowel can occur only once and the sum of the squares is $10 \times 1^2 = 10$. This represents the lower limit or minimum degree of consistency.

To make the measure of the consistency independent of the number of trials, the sum of the frequencies of the vowels squared, is divided by the square of the number of trials. By doing this the maximum consistency becomes 1.00 no matter how many trials are given, but the minimum will depend upon the number of trials.[5] Since the vowels available are relatively few and both groups used the same vowels we can regard the minimum consistency as equal for both groups of subjects. For this we have selected $T = 10$ which gives the minimum consistency of .10.

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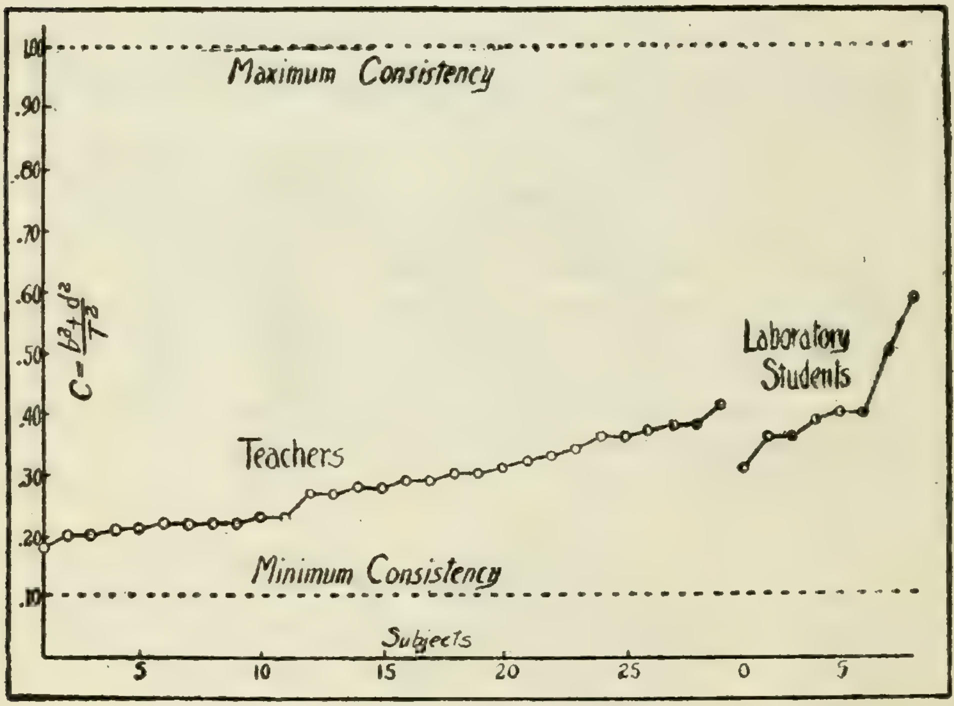
[5] The consistency (C) is algebraically expressed as follows: Let T represent the number of times a given fork is presented, and $b + d$ the number of times each vowel is assigned. Then $C = (b^2 + d^2) / T^2$.

Since there were only two series in Experiment I the consistency was not calculated.

CURVES V

INDIVIDUAL DIFFERENCES BETWEEN SUBJECTS AS MEASURED BY THE CONSISTENCY OF THE VOWEL DISCRIMINATIONS—SUBJECTS ARRANGED IN ORDER OF MERIT



Curves V. The dotted horizontal lines indicate the maximum and minimum limits of consistency. Each small circle on the intermediate curves represents one subject, and these have been arranged according to their degree of consistency. The vertical coordinate represents the average consistency for all the forks.

Considering the curve for the teachers, we find it relatively flat and that it does not indicate great individual variations. The median is at about 28 and this means that on the average

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If the same vowel is assigned each time $(b^2 + d^2) = T^2$ and $C = T^2 / T^2 = 1.00$, the *maximum consistency*.

If a different vowel is assigned each time $(b + d) = T$ or $C = T / T^2$ the *minimum consistency*.

each subject assigned about three different vowels to each fork. For the laboratory students the median is at 40 indicating that on the average two different vowels were assigned to each fork. Out of a possible thirteen vowels actually used this represents a fair degree of reliability in making vocality judgments. If only the vowel *groups* indicated in Curves IV had been used in the measurement of the consistency it would have been more than twice as great and the curves would lie about half-way between the maximum and minimum limits.

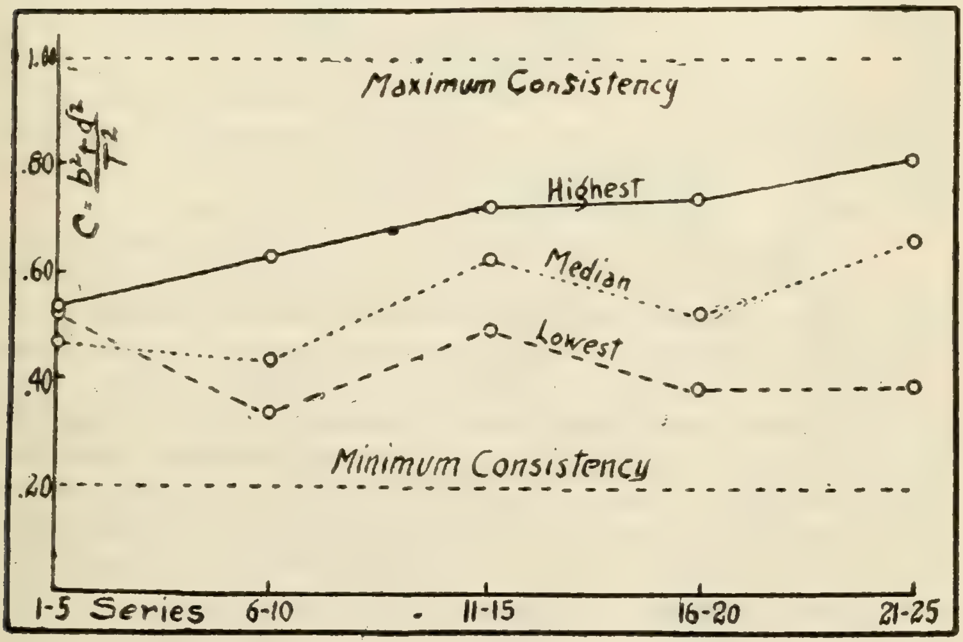
The individual differences between subjects conform to the usual results of those psychological experiments in which there is a gradual transition from one degree into another as for instance in auditory acuity. There is no evidence of a tendency toward a separation into two classes as into musical and nonmusical subjects.

VI. Effect of Practice on the Consistency

In the Series B of Experiment I the consistency is much greater than for the Series A. The question arises what is the effect of practice upon the variability of the discriminations? In Curves VI the consistency for three subjects from

CURVES VI

Practice Effect in Vowel Discrimination—Three Subjects From the Laboratory Class—The Consistency is Calculated After Every Five Series



Experiment II was calculated for successive groups of five series each. The curve marked 'highest' represents the subject whose average consistency for the twenty-five series was greatest. The 'median' curve represents the subject who stood intermediate, and the 'lowest' curve represents the subject whose discriminations varied the most.

The curve for the 'highest' subject shows a gradual increase in the uniformity with each succeeding group of five series. The rise is not marked and signifies that a high degree of consistency had already been reached after the fifth series. For the 'median' subject there is a slight oscillation which probably implies that the consistency is no longer a function of training or practice after the first five series. This is more clearly shown by the 'lowest' subject whose curve shows even a slight drop between the fifth and twenty-fifth series.

The curves seem to indicate that the ability to discriminate vowels in tuning fork tones reaches a constant level after less than five series or after fifty discriminations. This of course does not mean that the maximum acuity has been attained. It merely indicates that the subject has isolated the vocality factor and that he is not confusing it with pitch, intensity, etc.

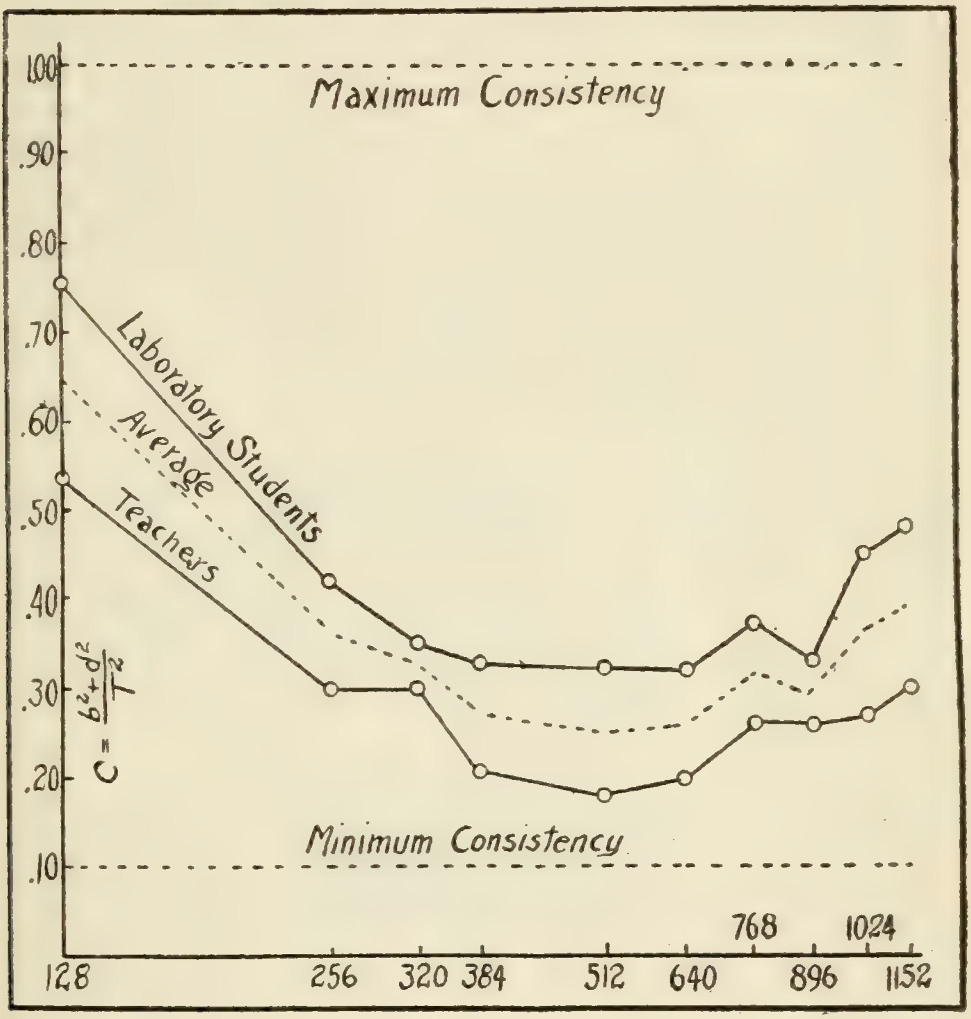
VII. The Consistency Between Vocality and Vibration Rates

The preceding topic indicated the individual differences between subjects. In Curves VII the individual differences of the forks, so to speak, are shown. The nearer a point on the curve approaches the dotted line indicating the minimum consistency the greater is the number of vowels assigned to it. To show the octave intervals as equal, the base line has been *spaced* in the logarithms of the vibration rates, although for easy reading the absolute vibration rates are given.

The curves clearly reveal that the forks in the middle region are more variable than the extremes. This is the opposite of pitch relationship or tonality which shows finer discriminations in the middle regions. The average consistency for the forks is indicated by the dotted curve. In consideration of the fact that these curves are based upon thirteen vowels, the degree of consistency is rather high. If only the vowel *groups* had been taken the curves would have been shifted upward to a median position between the limiting degrees of consistency and this would imply that on the average not more than two vowel groups would be reported for any one fork.

CURVES VII

Relation Between Vibration Rate and Number of Vowels Discriminated—Subjects From Experiments II and III



VIII. Comparison with the Work of Koehler

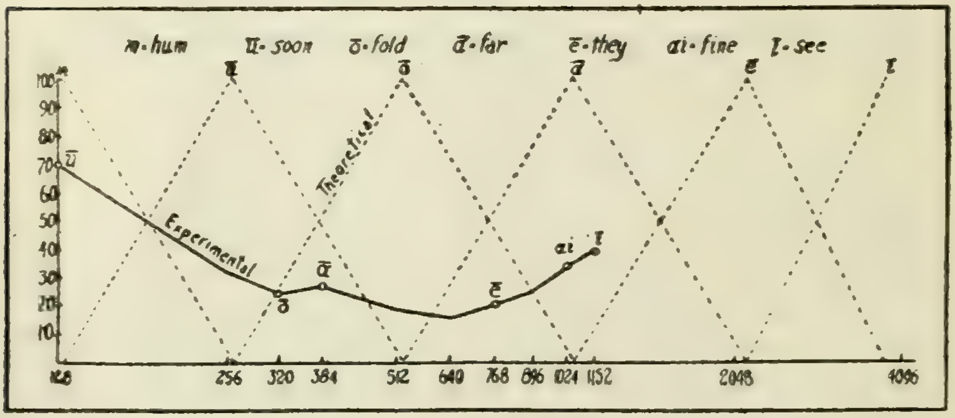
In comparing the results of our experiments with those secured by Koehler it is necessary to allow for the different conditions under which the data was secured. Our comparison curve is a combination of three separate experiments. Koehler's curve is the result of theoretical deductions drawn from his own experiments and various other sources. Along the base of the Curves VIII the vibration rates of the forks are shown, spaced in logarithms.

The dotted oblique lines represent Koehler's theoretical assumptions on the principle that each vowel has a maximum frequency at some absolute vibration rate, with the frequency receding on either side until at the adjacent octaves its frequency is zero. These adjacent octaves however are the maxima for other vowels. Thus Koehler gives the vowel ū (soon) at 265; that is the vowel character of a pure tone of 265 vibrations is ū (soon) and under ideal conditions this vibration rate would be characterized as ū (soon) in 100 per cent of the judgments. As the vibration rate is increased, the subjects would characterize the tone as ū with a trace of ō. A further increase in the vibration rate would make the ō component stronger until a rate is reached at which the vocality resembles ū and ō. If at this vibration rate, which is indicated by the points at which the oblique lines cross, a number of subjects are required to report which of the two vowels ū or ō the tone most resembles, half of the subjects would report ū and half would report ō.

As the vibration rates are increased from this point the ō component becomes stronger until at 526 there would be 100 per cent ō judgments. In the same manner, as the vibration rate is decreased from 265 the vowel character approaches um (hum) until at 132 we have 100 per cent um judgments but none of ū. Whether or not the maxima should be connected with the respective minima by straight lines, Koehler leaves open for the present.

CURVES VIII

Comparison of the Experimental Results With Koehler's Theoretical Assumptions—Base Line is Spaced in the Logarithms of the Vibration Rates



In order to combine the results of our experiments into a single curve upon which all the vowels are represented, rather than drawing a confusing nest of curves showing each of the forks separately, we have retained only the maximal points of the separate vowels. For the intermediate forks we have taken from Table IVa the average of both the percentages involved. For instance, the maximum for the ū-group is at fork 128 and the per cent is 70; the similar fork for the ō-group is 320 and the per cent is 22; at the intermediate vibration 256 the per cent for the ū-group is 39 and for the ō-group 23. The average of 23 and 39 is 31 which is the value we have selected for our experimental curve at 256. For all the other forks, which were not themselves the maximum for a vowel group, the same method was adopted.

Whenever untrained subjects or general class experiments are used, there is usually a masking of those effects that would be revealed by more intensive methods. This masking may be so complete as to be a virtual obliteration. If however there are *relative* maxima and minima shown in the graphical representation of the qualitative results these should coincide more or less closely with the quantitative experiments just as the relative maxima and minima of the quantitative experiments should coincide with the theoretical limits of ideal conditions if the theory represents the facts.

From the experimental curve it will be seen immediately that there is only one decided maximum at ū (soon) and a less distinct one at ī (see). Our ū coincides with Koehler's *m*, but none of the other vowels correspond with Koehler's nor is there any evidence of symmetry in their distribution. By shifting our curve to the right one octave all of our vowels are brought within Koehler's octaves. But whether this rather strenuous stretching is justified by the difference in language habits (English as compared with German) will remain doubtful until this factor is itself made the basis of a special study.

IX. Discussion of Vocality

Relation between Vocality and Vowels. Perhaps the striking thing in making vocality discriminations is the fact that the fork tones do *not* sound like the vowels used in language but yet may be said to resemble them so closely that consistent judgments are soon made upon this basis.

Meyer regards the vocality as a continuous series limited on the one hand by the degree of mellowness of the lowest tones and on the other by the degree of shrillness of the highest tones; following this conception we may regard the vocality of the intermediate vibrations as varying combinations of mellowness and shrillness. However to ask a subject to differentiate *degrees* of mellowness and shrillness directly, does not result in consistent reactions nearly so rapidly as when he is asked to discriminate the vowel character. Obviously this can only mean that the subject is not so familiar with the terms mellowness and shrillness as he is with the vowels of language.

Conventionalized Vocality Combinations. The vowels may be regarded as special names that have been given to those combinations of mellowness and shrillness that have been conventionalized into language elements. The vowel in actual speech is an auditory *complex* and its particular combination of mellowness and shrillness is only one of the components—that component however from which the vowel gets its name. There are of course many more combinations of mellowness and shrillness than manifest themselves in the vowels of any one language, but in speech an infinite number of vowels would be confusing and unnecessary. A dozen vowels meet the practical requirements of any one language. Convention and usage have determined which of the combinations shall be selected and the use of these has developed a degree of facility in their discrimination which gives them a predominance over all other possible combinations of mellowness and shrillness that are not used in language. The relative distinctness between vowels is thus merely an expression of the fact that we have selected and conventionalized those combinations which were most easily discriminated.

If the experimental conditions are such that the subject is asked to discriminate combinations of mellowness and shrillness that have not become conventionalized through their being a component of the language vowels, he describes them by the nearest of the conventionalized combinations (one of the vowels in his particular language) to which he already has learned to react. We might expect further that the vowels are rather closely related to absolute vibration rates because on the whole the range of the *average* human voice is rather restricted.

The Intervals in Vocality. The octave relationship which seems to exist between vowels may be regarded as a manifestation of the Weber-Fechner law. If a continuous physical series such as the frequencies of sound waves is to be divided into definite sensorial steps such as the vowels, the division will occur in equal logarithmic intervals rather than in equal absolute vibration intervals because the logarithmic intervals will give sensorial steps in which the dissimilarity between adjacent steps is equal and greatest. That is, the equal logarithmic steps would become conventionalized rather than would the equal absolute vibration steps. This of course, would give a kind of an octave relationship for the given language. In other languages the apparent octave relationship may be based upon entirely different combinations of mellowness and shrillness. From the vowels and semi-vowels of all languages we might be able to construct a practically continuous series of all possible combinations of mellowness and shrillness.

Definition of Vocality. In the interest of uniformity in terminology the following definition or description of vocality is suggested:

Vocality is that attribute of auditory sensation which is described by the vowel and semi-vowel character of tones and noises. It may be regarded as a continuous series ranging from the degree of *mellowness* of the vibration rates at the lower limit of hearing to the degree of *shrillness* of the vibration rates at the upper limit. Various combinations of mellowness and shrillness manifest themselves as a resemblance to the vowel and semi-vowel sound: *m* (hum), ū (soon), ō (fold), ā (far), ē (they), ī (see), s, f, H, of which *m* is the mellowest and H [pronounced as the *ch* in *loch* (Scotch) or mi*ch* (German)] is the shrillest.

This definition or description is eclectic and is derived from suggestions of Stumpf, Meyer, Koehler and Titchener. If all investigators could agree to call this one property *vocality* and nothing else, and if they would exercise some restraint in the coining of new terms until they had assured themselves that they were describing a form of discrimination that had not been made by others, greater uniformity in terminology would result. If for instance, it is impossible to discriminate between two attributes of sensation then we have only one attribute and there is no advantage in giving this attribute two or more different names. If brightness (*Helligkeit*) cannot be discriminated from shrillness, then there is no difference between brightness and shrillness and one of the terms is sufficient. Which of the two terms will be adopted is of course immaterial. Priority in use is the simplest criterion.

X SUMMARY AND CONCLUSIONS

The problem was to determine,

A. How consistently relatively untrained subjects could discriminate the vocality or vowel character of tuning forks ranging from 128 to 1152 vibrations.

B. Whether the term vocality or vowel character is a suitable one to describe an attributive or cognitive fact of audition.

A. Consistency of Vocality Discrimination

From experiments with the elementary students (I)[6] we find even with only a single series that the ū (soon) character is predominant for the lower forks and the ī (see) character is predominant for the higher forks. The intermediate vowel classes, ō (fold), ā (far), ē (they), do not seem to have dominant positions at any particular vibration rate; but there is no question that they occur as a series without the possibility of interchanging their relative positions.

The results from the experiments with the laboratory students (II) show the same thing but in a more orderly manner. The discriminations of the teachers for the deaf (III) approach the results of the more specifically trained laboratory students (II) as well as might be expected when we consider the larger number of vowels used.

When the findings of all the experiments are combined (IV) and the vowels are reduced to six groups, each group has a relative frequency (Table IVa) which makes it probable that the group of vowels are associated in an orderly manner with corresponding vibration rates, but our experiments do not exhibit an octave or interval relationship (VIII).

There is no evidence of marked individual variation (V) between subjects in their general ability to discriminate vocality such as is supposed to exist between musical and non-musical subjects. A high degree of consistency is reached after relatively few vowel discriminations. For the elementary students (I) a marked improvement is shown after only one series. For the laboratory students (VII) the degree of consistency at the end of the fifth series (a total of fifty single discriminations) has practically reached the limit that is reached after 250 discriminations. The consistency of the vowel discriminations for the different forks (VI) is highest for the extremes and lowest for the intermediate forks.

B. The Term Vocality

From the rapidity with which the discriminations of vowel character become consistent (I, VI) it seems evident that when a subject is asked to discriminate the vowel character of a fork tone, the reaction is one that is already well established by the ordinary (speech) habits of the individual. Practice (I, VII) improves the vocality discriminations so rapidly that no extended demonstrations are necessary to bring out the meaning of the term.

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[6] The roman numerals in parentheses refer to the headings from which the conclusions are derived.

Improvement in the discrimination occurs spontaneously (III) and manifests itself by the addition to the vocality series of those vowels which are formed by lengthening or shortening the vowel sounds. Even when the discriminations become consistent the subjects (II) still report that while two tones of different vibration rates may be classed as resembling the same vowel there is still a difference between them which can be described as an admixture of another vowel sound but the degree of admixture cannot be reported with a high degree of consistency.

As a definition or description of the term vocality (IX) the following is suggested:

Vocality is that attribute of auditory sensation which is described by the vowel and semi-vowel character of tones and noises. It may be regarded as a continuous series ranging from the degree of *mellowness* of the vibration rates at the lower limit of hearing to the degree of *shrillness* of the vibration rates at the upper limit. Various combinations of mellowness and shrillness manifest themslves as a resemblance to the vowel and semi-vowel sounds: *m* (hum), ū (soon), ō (fold), ā (far), ē (they), ī (see), s, f, H, of which *m* is the mellowest and H, pronounced as the *ch* in lo*ch* (Scotch) or mi*ch* (German) is the shrillest.

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THE VOCALITY OF FORK, VIOLIN AND PIANO TONES

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Problem—The immediate purpose of this study has been to investigate the validity of the theory of absolute vocality as it has been set forth by Köhler and others, when applied to tuning forks, piano and violin. If the assumptions as made be true, then we may expect to find a *relative* degree of consistency between the several instruments. A tone of a given pitch should according to the theory have the same vocality, whatever the instrument that produces it, provided the vocality of the over-tones is considered. Köhler[2] assumes first of all that vowels belong to definite frequencies. Willis and other investigators also found that vowel character is dependent on frequency. Köhler further assumes that all vowel characters may be defined in terms of the proportion of ū, ō, and ī quality. In other words, these three are most definitely located with ū at the lower end of the scale, ī at the upper and the pure ō sound midway between. As one leaves either end of the scale, the vowel quality characterizing that end becomes less and less prominent and that of the other end increases. Those vowel sounds which his observers definitely located, m, ū, ō, ā, and ē, were of such frequencies that their pitches represent successive octaves in the order named.

Observers—The observers were children from a first grade intermediate school, i. e., from the 7B to the 9A grades inclusive. The ages of the children ranged from 11 to 19, in numbers as follows:

No. of Children	*Age*
11	11
34	12
52	13
54	14
36	15
7	16
2	17
0	18
1	19
2	?

This made a total of 199 children with an average age of 13.3 and a median age of 13. Of these children, 123 had studied some little music outside of that regularly given in the public schools, and 76 had not. No definite information could be obtained as to the amounts, so it was merely recorded as 'some' or 'none.' As a general rule, it meant a few lessons on the piano. In addition to the 199 children to whom the three series, i. e., forks, piano and violin, were given, 33 others were given the fork series and the violin series and 26 the fork series only. These results however are not included in the data herein given, as it was desired to have results from the same children for all three series.

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[1] This research was done, under the direction of Dr. Weiss, at the Ohio State University.

[2] Köhler, Wolfgang, 'Akustische Untersuchungen, II,' Zeitschrift für Psychologie, 1911, 58, pp. 59-140.

Material—The material used consisted of a series of tuning forks including the frequencies 256, 512, 1024, 1280, a piano, and a violin. The experimenter was a performer on both these instruments and was thus able to handle them advantageously for such an experiment.

Method—The experiments were performed in a room in which there were tables and chairs across either end. Approximately twenty children were given the experiments at one time. They were seated at the tables in such a manner that their backs were to the experimenter who played the instruments. The position of each child was about ten feet from the source of the sound.

After the forks had been shown to the children and the method of obtaining a sound from them had been explained, the following instructions were given:

Listen carefully to each fork as it is played and then when I have finished with it, write down opposite the number (serial number, as 1. 2. 3., etc.) the word or vowel it sounds most like to you. I shall play each tone three times. Listen carefully till I have finished and then write down the word. If you can not think of one that exactly fits, put the best thing you can think of; if you can think of none, leave that space blank.

After it was certain that the children understood what they were to do, the first series was presented, giving 256, 512, 1024, 1280, and then a repetition of the same with the order reversed. The same tones were then given in the piano and violin series with the addition of 384 preceding and 1536 following the series as given above. An interval of two or three seconds was allowed after the one tone rung off before it was repeated. Between tones of different pitch sufficient time was allowed to have each child note down the word or sound that he thought represented the tone just given.

The work was marked by interest and splendid cooperation on the part of the children. From the whole number tested, (more than 200) only a very few records had to be discarded because they showed lack of effort or a misunderstanding of instructions on the part of the observers. These were for the most part papers in which the children had tried to give the tonic-sol-fa equivalent for the tone given, which response is one of pitch rather than of vocality.

Results—Obviously in so large a number of records, a great variety of words will appear. The number of actual vowel sounds represented however is much smaller. For example, the words, moon, too, threw, blue and many others all represent the vowel sound ū. Likewise, the words, eat, me, seat, fiend and many others represent the sound ī. Among children we do not find the variety of vowel sounds that we do among adults. This is probably due to the narrower limit of the vocabulary of the child as compared with the adult, as well as the lack of development in finer discriminations of sound. It is often noted that children do not make the fine distinctions in the pronunciation of words that we find in the average adult. In this instance the sounds which they gave, fall rather naturally into twelve or thirteen groups. In the tables thirteen are given, but the numbers for one group, namely that with the sound short ŏŏ as in took, are very small. These vowel sounds and the number of observers giving such sound for each tone, are recorded in Table I.

TABLE I

NUMBER OF OBSERVERS, VOWEL CLASSES AND VIBRATION RATES FOR FORK, PIANO AND VIOLIN TONES

	ū	ō	ā	au	u	a	ā	ē	i	ai	e	ï	ŏŏ
Fork	who	know	saw	how	hum	fat	far	prey	hit	pine	get	feel	took
256	123	17	15	0	32	2	0	1	1	1	0	1	0
512	88	30	10	7	13	2	0	4	1	1	7	19	3
1024	38	45	4	9	6	1	2	10	10	3	13	45	0
1280	24	16	5	8	9	1	5	3	24	13	13	52	2
1280	17	21	8	8	10	2	4	4	23	18	7	59	0
1024	40	47	13	14	11	0	3	5	9	9	9	19	0
512	74	48	3	10	13	2	2	5	7	2	8	5	1
256	93	26	8	9	23	0	2	4	2	2	7	2	2
Piano													
384	49	56	4	5	13	3	3	4	3	1	3	8	1
256	62	61	5	4	14	2	0	3	0	5	4	4	0
512	15	18	7	4	17	2	6	11	11	11	7	44	0
1024	17	18	4	6	8	2	6	4	12	13	7	53	0
1280	7	18	4	2	8	2	2	7	18	18	10	54	0
1280	6	16	6	1	6	1	2	4	15	14	13	60	0
1024	33	29	8	6	12	4	3	6	8	12	17	11	1
512	29	37	13	4	22	4	4	11	1	4	8	6	1
256	39	59	11	3	17	0	2	2	2	7	7	3	0
1536	8	18	3	3	7	2	2	3	24	6	39	30	0
Violin													
384	23	44	13	6	27	1	3	10	4	5	15	14	7
256	47	57	12	7	21	2	2	11	4	1	2	9	1
512	15	28	3	3	9	3	2	9	7	13	20	61	0
1024	5	7	2	4	6	2	1	8	13	16	14	93	0
1280	10	9	2	5	11	0	3	5	22	20	6	78	0
1280	6	10	4	1	5	3	4	5	17	11	8	100	0
1024	16	15	7	8	11	0	2	13	19	9	14	49	3
512	23	48	15	5	16	1	3	19	8	4	12	14	6
256	33	60	14	11	17	3	6	7	1	12	5	1	4
1536	10	11	3	0	4	1	1	12	20	7	32	63	2

The numbers in the first column indicate the vibration rate of the tone used in each case. The numbers in the remaining columns indicate the number of times the vowel sounds at the top of the columns were given as corresponding with the tones indicated at the left.

For example, when tone 256 was sounded, from the forks, 123 observers reported the vowel sound u, 17 observers the vowel sound o etc. These figures are represented graphically in Figure I.

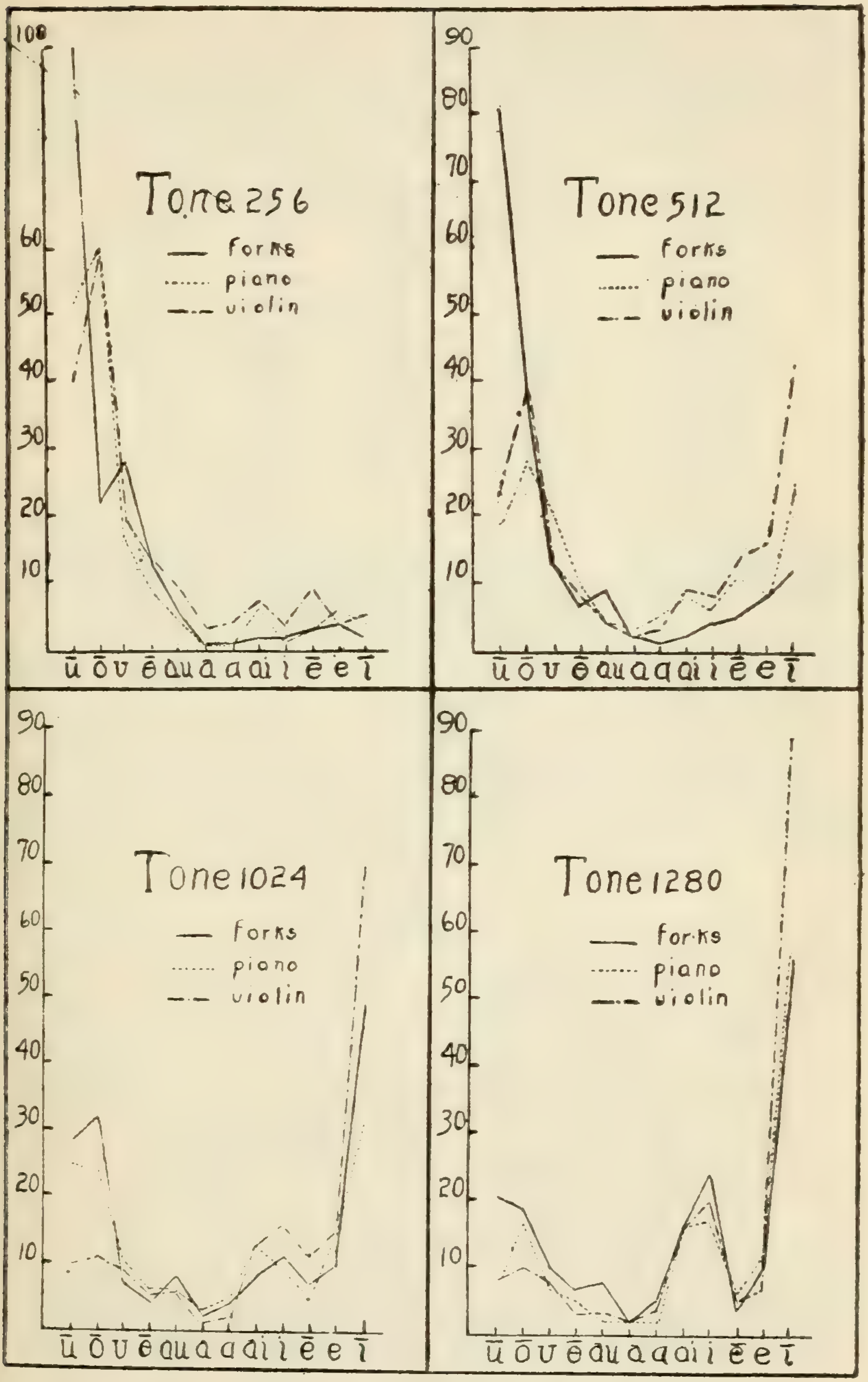

Fig. I—Graphical Representation of Results in Table I. Both trials averaged.

TABLE II

Vibration Rate at Which Vowel was Most Frequently Recorded

Vowel	Forks	Piano	Violin	Identity
ū (who)	256	256	256	3
ō (know)	1024	256	256	2
ā (saw)	256	512	256	2
au (how)	1024	1024	256	2
u (hum)	256	512	256	2
a (fat)	512	512 (1024)	256	2
ā (far)	1280	512	256 (1280)	0
ē (prey)	1024	512	512	2
i (hit)	1280	1280	1280	3
ai (pine)	1280	1280	1280	3
e (get)	1024	1024	512 (1024)	3
ī (feel)	1280	1280	1280	3
ŏŏ (took)	512	512 (1024)	512	3

The table reveals that eight of the vowel sounds were placed at the same tone for forks and piano; for eight the forks and violin agree. In six all three agree. Noting the number of times that any particular vowel sound was found throughout the whole series, we find that the numbers for ā, ā, and ŏŏ (Table I) are very small as compared with the other vowel sounds. These vowels might have been combined with other vowels but it was thought best to give actual results without any arbitrary attempt at grouping.

A very large majority of the observers agreed that the sound ū characterizes the tones of the lower end of the scale, and that the sound ī characterizes the higher. Between these limits there is more variation and diversity of opinion, than at the extremes. This fact is shown in Table III, in which is recorded the vowel sound which was given by the largest number of observers for a given tone.

TABLE III

Vowel Sound Reported Most Frequently for Each Tone

Tone	Forks		Piano		Violin	
	I	II	I	II	I	II
256	ū	ū	ū	ō	ō	ō
512	ū	ū	ī	ō	ī	ō
1024	ō and ī	ō	ī	ū (ō)	ī	ī
1280	ī	ī	ī	ī	ī	ī
384			ō		ō	
1536			ē		ī	

I and II refer to first and second trials. For fork 1024, vowels ō and ī were given by the same number of observers (45 each). For tone 1024 when sounded from the piano, ū was given by 33 observers and ō by 29.

It was thought desirable to calculate the consistency[3] or reliability of the vowels assigned to the various vibration rates. The formula $C = (b^2 + d^2)/T^2$ was used. For example, if tone 256 is sounded ten times, and the vowel *ū* is reported three times, the vowel *ō* four times, and the vowel *àu* three times, an application of the formula gives $C = (3^2 + 4^2 + 3^2)/10^2$ or reduced, $C = .34$. If the same vowel is given each time, we get $10^2/10^2$, a maximum consistency of $C = 1.00$, if a different vowel is recorded each time or by each observer we get 1^2 . . . . . 10 times or $10/10^2$, a minimum of $C = .10$. In Table IV are recorded the consistencies for each tone and for each instrument.

TABLE IV

Consistency for Each Vibration Rate

	256	512	1024	1280
Fork	.34	.23	.14	.13
Piano	.17	.08	.09	.11
Violin	.15	.11	.17	.23

These Figures Represent the Average of the Consistencies as Calculated for the First and Second Trials—Fig. II Shows the Comparisons More Clearly

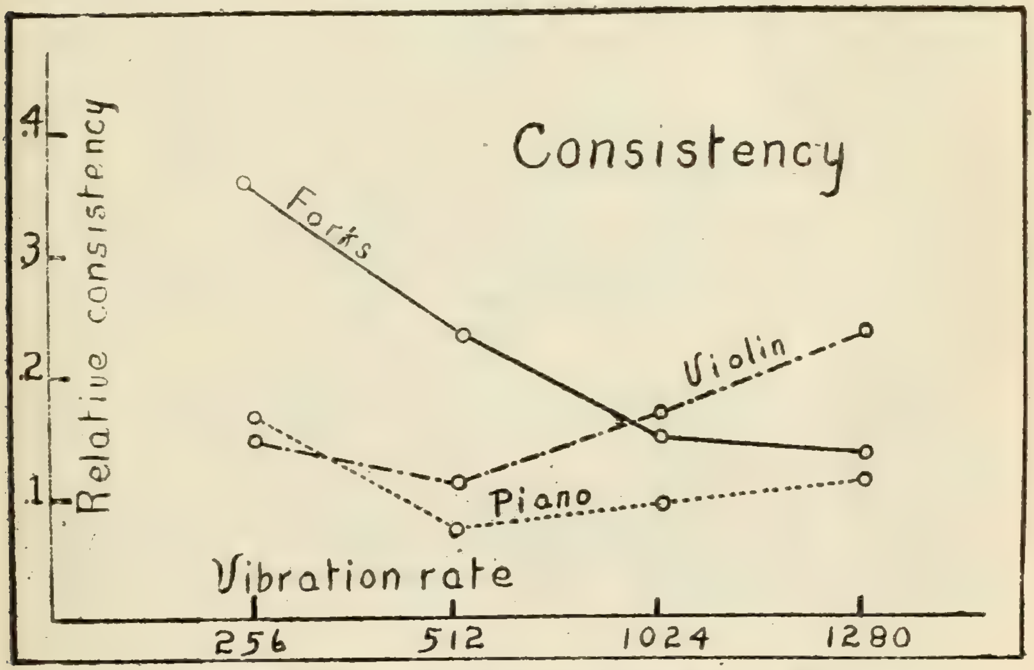

Fig. II—Graphical Representation of Table IV.

Among 200 observers, with all the possibilities of vowel sounds before them, it is not surprising that we do not get a greater consistency. We find that the curves of consistency for the forks and the piano are approximately the same shape, but in the curve representing the tones as given by the violin, there is a rise at the upper end, instead of the lower. Miller[4] has found that the lower tones when sounded from the violin have weak fundamentals. Furthermore, the resonance of the body of the violin is such that it emphasizes the higher tones and their partials. The lowest tones on the violin are lower than the fundamental resonance of the violin body. On the average, the consistency for the forks is greatest, the violin second, and the piano least.[5] The consistency or agreement between the two trials is also greatest for the forks, for the violin second, and for the piano least (Table III).

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[3] In conformity with a similar article by A. P. Weiss it was thought best to adopt this formula.

Another interesting result is that gained by comparing the vowels given for the two lowest tones and those for the two highest tones of the series. Fifty-three per cent of the observers gave either ū or ō as the vowel representing the vocality of the tone 384 as sounded by the piano, whereas 62 per cent gave these vowels for the vocality of tone 256. This fact shows a discrimination of the lower vowel quality of the tone 256 as compared with tone 384. Likewise, 37 per cent of the observers gave the five vowels representing the highest vowel quality,[6] when tone 1024 was sounded, whereas 52 per cent of the observers gave these high vowels when tone 1280 was sounded. This shows a discrimination of the ī vowel quality as the upper limit of the scale is approached.

In the violin series, 59 per cent of the observers gave the vowels of the lower end of the series after tone 384 was sounded, whereas 74 per cent gave these same vowels to the tone 256. This shows a discrimination of the lower vowel quality of the tone 256. Likewise, at the upper end of the scale, 50 per cent of the observers gave the ī, āī, or ī vowel sounds to the tone 1024, whereas 62 per cent gave these vowels to the tone 1280. This shows a discrimination of the increase of the ī quality as the upper limit of the scale is approached.

The addition of the two tones to the piano series and the violin series furnished a test of the children's ability to recognize the change in vowel character, when the change was not a part of the progressive series. A study was made of each individual's response to the change, i. e., whether he changed from a lower vowel sound to a higher, kept the same vowel for both tones, or changed from a higher to a lower vowel sound. These results are given in Table V.

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[4] Miller, Dayton, Clarence—The Science of Musical Sounds, 1916, p. 197.

[5] The question as to whether the consistency of different instruments changes for different vibration rates is an interesting problem, but one for which this data is insufficient.

[6] These figures were obtained by adding the number of au, e, ī, i, and ai vowels given for the tone 1024 (or 1280 as the case may be). These vowels are given the greatest number of times at either 1024 or 1280, when sounded from the piano. (Table II).

TABLE V

VOWEL CHANGES RECORDED WHEN TONE CHANGE WAS NOT A PROGRESSIVE ONE

Change	Lower to Higher	Same for Each	Higher to Lower
Piano			
From 256 to 1536	66%	18%	16%
From 384 to 256	18%	61%	21%
Violin			
From 256 to 1536	72%	21%	7%
From 384 to 256	19%	35%	46%

The results show that when the change in interval is from 256 toward 1536, a large per cent of the observers changed the vowel from lower to higher as would be expected. A small per cent of the observers kept the same vowel or changed from a higher to lower. On the other hand when the change in interval is from 384 to 256, (a small change when compared with the preceding one) a high per cent of the observers recorded the vowel quality of the two tones as the same, a small per cent recorded a change from higher to lower (the true change) or from lower to higher. The number recording a change from higher to lower was of course greater than those recording the opposite change. These comparisons are made on the basis of the piano series. In the violin series, the results are practically the same, except that in the change from 384 to 256, a higher per cent of the observers made a vowel change from higher to lower (the correct change) than those who kept the same vowel for both tones, as was the case in the piano series. This does not alter the conclusions to be drawn from these results however. The results simply mean that the observers discriminated a much greater change in vocality between tone 256 and tone 1536 than between tone 384 and 256.

Conclusions—The results of this study warrant the following conclusions:

1. There is unquestioned evidence that there is some degree of consistency in the vocality assigned to tones, whether produced by forks, piano or violin. This means that the vocality is a factor associated with pitch, but not one of absolute pitch.

2. The vowel quality ranges from the ū sound at the lower end up to the ī quality at the upper end of the scale, with a wide variety of vowel characters between the two, which become more and more like these extremes as the upper or the lower end of the scale is reached.[7] The great majority of the observers agree as to the location of the ū and the ī vowel sounds.

3. The degree of consistency of the vowels assigned to the various vibration rates is greatest when the tones are produced by the tuning forks. This is probably due to the absence of overtones, the tones from the tuning forks being relatively much simpler or purer than the same tones produced by the piano or the violin The vocality of any

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[7] Titchener, E. B. 'Beginner's Psychology,' p. 52.

given tone depends not only upon the fundamental tone, but also upon the several overtones, and the intensity of each.

4. The results of this experiment agree with those of Köhler first, in that the vowel characters ū and ī are much the easiest to locate. Secondly, the location of the ō sound is somewhere near the same, 512 (Köhler located it between 520 and 530). Thirdly there is a gradual decrease of the ū quality and an increase of the ī quality as one proceeds from the lower to the upper end of the range. However these results do not agree with the assumption that the vowel character is dependent on frequency. Were this true the dominant vowel quality should be the same, whatever the instrument used. One of the principal differences obtained by using the several instruments, is the change in the number and intensity of the overtones present. We find that there is a shift in the location of some vowel characters with a change in instrument. Köhler reported that he had trouble with the observers, due to their unfamiliarity with the tuning forks. In the present experiments this was not true. Greater consistency was obtained with the tuning forks than with either of the instruments. Köhler suggests that the vowel character may be found not only at a given frequency, but at those frequencies which are in immediate proximity to it. He makes his theory of vowel character comparable in this wise to the resonance theory of Helmholtz.

No attempt was made in this study to point out a definite location for any given vowel sound, but the results do not point in that direction. The pitch of a vowel is not nearly so absolute as is assumed by Köhler. The order of vowels found as related to pitch range was indeed that obtained by Köhler, but the results would rather warrant the following assumption.

There is at the lower pitches a quality, which corresponds to the vowel ū of the speaking voice, and there is at the upper pitches a quality, which corresponds to the ī sound of the speaking voice. These might be referred to as a mellowness or a shrillness, as is done by Meyer, which we merely represent by means of the vowels. In between these limits there are many other characteristic sounds, which may resemble the ū or the ī or may be of such a character as to be identified with neither. These we indicate by means of other vowels which are formed in such manner as to have a mellowness or shrillness between the designated limits. The fact that, as Miller found, starting with the pure ō sound, one may by slowing down the movement of the graphophone produce a sound that becomes less like the ō and more like the ū, or by speeding up the movement, produce a sound that becomes less like the ō and more like the ī, does not necessarily imply that the vowel character is a function of the vibration rate of the fundamental only. The presence of upper overtones of high intensity will of necessity bring into the compound more of the ī element. This is shown particularly with the experiments on the violin tones. The presence of strong overtones whether high or low, will raise the vowel character. We accordingly find that when the tones are produced by tuning forks where the presence of overtones is comparatively unimportant, that the ū sound or quality predominates and that even with the higher pitches, the amount of the ī quality does not reach that of these same pitches when sounded from the piano or more especially the violin.

If a small mouthed bottle be held under a stream of water and allowed to fill, the tone produced by the water passing through the mouth rises in vowel quality as well as in pitch. This closing up of a larger cavity to a smaller produces the change from the mellow to the shrill quality, which we designate by means of the vowel equivalents. This is in some measure the same as what Watt[7] calls tone-volume, although he later defines vowels as "(partial) sounds of a somewhat indefinite pitch which lies about the point of range of pure tones of definite pitch where the greatest resemblance to these vowels is to be found." It seems, however, that it is not the slight variation in fundamental pitch that is so important, as the effect that the change in the intensity of the overtones has upon the whole, and thus upon the vowel character.

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[7] Watt, Henry J., 'The Psychology of Sound,' p. 233.

RELIGIOUS BELIEF AND THE POPULATION QUESTION

By Wesley Raymond Wells, Colby College

In a previous article in this journal[1] I have spoken of the biological value of religious belief. Such considerations as were presented in that article ought to be supplemented by an account of one further situation, a very specific and concrete situation, in which religious belief manifests its biological value most strikingly. I refer to the correlation between religious belief and a relatively high birth-rate, especially in the case of belief of a somewhat legalistic sort such as is now best exemplified in Catholicism. This correlation is very significant. It shows that religious belief possesses survival-value of a high order.

The connection between religious belief and the birth-rate in ancient history is a well-known fact. Fustel de Coulanges[2] has pointed out how, in the patriarchal families of the ancient Greeks and Romans, religion required the continuity of the family. The fate of the ancestral spirits was believed to depend upon offerings made at their tombs by their descendants. Continuity of the family was required for the sake of the sustenance of the departed ancestors. Celibacy was an impiety, forbidden by religion and also by civil law when law arose out of religious requirements as something distinct from them. The ancient laws of Rome forbade celibacy.[3] The ancient Hindus had similar laws. For the Hindus as for the Greeks and Romans the extinction of a family caused the ruin of the family religion, and this was to be avoided by all means.[4] Among the ancient Hebrews also it was a fundamental religious duty to " be fruitful and multiply."

All this, however, is a matter of ancient history. It is more important to inquire about the relation between religious belief and increase of population in modern society.

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[1] Vol. XXIX (1918), pp. 383-92.

[2] *The Ancient City,* (translation by Willard Small), Boston, 11th edition, 1901.

[3] See Cicero, *De Legibus,* iii, 2.

[4] See Fustel de Coulanges, *op. cit.*, p. 62.

In the first place, it seems a matter of common observation that, in general, the birth-rate is highest in families most influenced by religious belief. The reasons that might be assigned as the cause of this are various. In some cases the religiously inclined may feel a mystic obligation to rear a family. Fear of a concrete hell for those failing to fulfill their parental obligations may be the motivating power in other cases. Mr. Russell seems to incline to this explanation, and to think that the belief in hell-fire is thus biologically justified, when he says, " Men and women who can still believe the Catholic faith will have a biological advantage; gradually a race will grow up which will be impervious to all the assaults of reason, and will believe imperturbably that limitation of families leads to hell-fire "[5] A third possible explanation of the connection between a high birth-rate and religious belief is the partial identity of the parental instinct and the religious sentiment. According to McDougall,[6] the tender emotion, which is a correlate of the parental instinct, is a constituent also of the religious sentiment.

In the second place, statistical studies have supported the conclusions of casual observation by showing it to be a fact that, where religious belief thrives, there a relatively high birth-rate is generally to be found. The Italian premier Nitti [7] has called attention to this fact. Leroy-Beaulieu has made a statistical study of the situation, not only in France, but in various other countries as well. He has found a high birth-rate among the Spanish and Italian Catholics, and in those sections of France where the Catholic church is strong.[8] McDougall [9] accepts the contention of Benjamin Kidd [10] that one great influence of religious belief has been, and continues to be, its instrumentality as a support of the parental instinct against the tendency towards prudential limitation of the birth-rate with developing intelligence. Sidney Webb[11] explains the high birth-rate among Catholics in England by reference to the fact that the Catholic church absolutely forbids any regulation of the marriage state. Of the distribution of this high birth-rate he says,[12] "It is significant that Ireland is the only part of the United Kingdom in which the birth-rate has not declined; that in Ireland itself it has declined a little in semi-Protestant Belfast, and not at all in Roman Catholic Dublin; and that in the towns of Great Britain the decline is least in Liverpool, Salford, Manchester, and Glasgow—towns in which the proportion of Roman Catholics is considerable." Webb finds that among the metropolitan boroughs the highest birth-rate is "in those boroughs in which the Irish Roman Catholics (and the Jews who, in this respect, are in the same position) are most numerous."[13] Many economists have tried to correlate high and low birth-rates with conditions of poverty and wealth, but Webb shows that there is a closer correlation with religious belief than with economic conditions. As a matter of fact, however, poverty and religious belief are frequently found together since many people find compensation in religion for the lack of the material satisfactions of life.

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[5] Bertrand Russell, " Marriage and the Population Question," *International Journal of Ethics,* Vol. XXVI (1915-16), p. 451.

[6] See Wm. McDougall, *Social Psychology,* pp. 66ff and Ch. XIII.

[7] Francesco S. Nitti, *Population and the Social Order* (translation), pp. 118-24.

[8] Paul Leroy-Beaulieu, *La Question de la Population.* See especially pp. 395-402. Leroy-Beaulieu gives the following explanation of the influence of religion, especially Catholicism, on population: "*La religion catholique, plus encore que toutes les autres enseigne la résignation à son sort, condamne l'égoïsme et déconseille l'ambition; c'est-à-dire, q'elle exalte le sentiment qui tend à rende les familles nombreuses et qu'elle reprouve ceux qui tendent à diminuer le nombre des enfants.*" Pp. 397, 98).

[9] *Op. cit.*, p. 272.

[10] See Benjamin Kidd, *Social Evolution,* p. 295; also Ch. V.

For a most complete and convincing array of statistics upon this subject, Mr. Meyrick Booth's article, "Religious Belief as Affecting the Growth of Population,"[14] should be consulted. Booth shows, for example, that, according to the Catholic Year Book for 1914, the birth-rate per 1000 of Roman Catholics in Great Britain was 38.6, while the average rate for the whole population was but 24.0. In the United States, among those states which, according to the religious census of 1906, had a small population of Catholics, the birth-rate was low, averaging 15 or less per 1000. On the other hand, in five states with a large proportion of Catholics, the birth-rate averaged about 23 or 24 per 1000.[15]

There may be a temperamental basis for susceptibility to religious belief, and temperament is hereditary. As McDougall says,[16] basing his conclusions on studies made by Karl

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[11] *The Decline of the Birth-Rate,* Fabian Tract, No. 131, 1907.

[12] *Ibid.,* p. 9.

[13] *Ibid.,* p. 9.

[14] In the *Hibbert Journal,* Vol. XIII (1914-15) pp. 138-54.

[15] Booth's article is a careful study of the situation, and gives many tables of statistics, which should be consulted by anyone who is not convinced of the importance of religious belief as influencing population increase.

[16] *Op. cit.,* p. 273.

Pearson,[17] about one-half of each generation is recruited from one-quarter of the preceding generation. The quarter with highest birth-rate is, as I have shown, the most religious portion of society. So far, then, as there is a temperamental basis for religious belief, and so far as temperament is hereditary, the religious temperament, and consequently religious belief, can not die out while the birth-rate is highest among those possessing the requisite temperament.

The correlation between religious belief and a relatively high birth-rate is most conclusive evidence of the biological value of religious belief. If unbelievers are not so much interested as others in founding families, then their unbelief perishes with them; while religious belief springs up in each new generation from a perennial source of vital human need.

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[17] See Karl Pearson, *Chances of Death.*

A NOTE ON PEN-LAPSES, INITIATED VISUALLY

By June E. Downey

The recent analysis by Dr. Roback[1] of a number of pen lapses, and, in particular, his observations relative to the possibility of a pen lapse being set off by the visual perception of a word already written leads me to record a number of self-observations made on pen-lapses occurring spontaneously during rapid composition.

My records were made immediately on recognition of the lapse. It was usually possible to call back the graphic cue and in other respects to snapshot the process.

As a preliminary to the report I may say that in composition my cue is very distinctly acoustic-vocal-motor. There are present, in addition, hand-motor sensations, since the writing movements involve considerable tension; and, also, a high degree of visual consciousness of what the written product looks like. Only under very great absorption in the content do I lose this consciousness of visual supervision. One consequence of this is that pen lapses rarely get past the visual censor; incipient lapses are apt to be detected in the making. Another consequence is, I suspect, the initiation of an occasional lapse through visual suggestion. I select the following cases for report, quoting directly with a few additions in parentheses, from notes made at the time the lapse occurred.

1. "Started to write 'coats' for 'goats.' After writing 'co,' I stopped and wondered what was wrong. The acoustic-motor cue was unambiguously 'goats,' but, visually, the 'co,' with a vague visual continuation of 'ats' seemed just as meaningful. There was considerable hesitation before correction was made....Definite split between the auditory-vocal-motor cue and the visual report on the process. Was the origin of the lapse grapho-motor in nature? (A more probable cause would seem to be the similarity in sound between the 'c' and the 'g.')......Noticed later that the word 'color' occurred in the line above, three inches to the left. Perhaps the sight of this 'c' suggested the lapse. 'Color-blindness' was the topic of the paragraph, so that 'C' may well have persisted in consciousness."

2. "Wrote 'world(s)' for 'words.' This lapse was corrected before 's' was completed; 's' was on the tip of the pen when writing was halted and the correction made...... This lapse is interesting in that it parallels another lapse recorded the same day, ('coats' for 'goats'). The word 'world' occurred in the preceding line, two inches to the left of the place where the lapse occurred."

3. "The auditory-vocal-motor cue was, clearly, 'give' but I wrote 'keep,' recognizing the lapse as soon as the word was written. Was unable to see any reason for such a lapse, then, recalling my observation of a month before, looked up and to the left—sure enough! there was the word 'Keeps' first word in second line above, one and five-eighths of an inch to the left."

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[1] This Journal, 1919, 30, pp. 274-290.

4. "Was taking notes on book I was reading. **Wrote 'See Galton,** p. 20g,' for 'See Galton, p. 209.' (Both G's were written in capital form but with a long down loop.) Distance from the 'G' in Galton to the interpolated 'G' was one and three-fourths inch. The graphic cue was largely visual, the number 209 as visualized on the page; no lip-motor consciousness. Possibly the lapse was due to grapho-motor perseveration, long down stroke on the 'g' (initial stroke on '9' and on 'g' being similar); possibly there was a visual suggestion operating from the peripherally seen G."

5. "Wrote on the margin of a theme I was correcting, 'Collected' for 'Connected.' Sentence read, 'Should be parallel in form and connected.' It was about two inches from the first 'l' in 'parallel' to 'l' as written in 'collected.'

6. "Was intending to write 'On the other hand;' I wrote, instead. 'On the who(le).' The auditory cue was 'hand' but perhaps the word 'whole' was there in the margin. Lapse was immediately recognized visually. After writing an account of the lapse I examined the writing tablet to see if a 'w' had occurred in the preceding line. The word 'what' was found in the line above, about one-half an inch to the right of where the lapse occurred."

7. "Wrote in a letter 'I've been hoping to see your article in print ere this. Of course it's hop(ing).' The verbal cue was definitely 'Of course it's worthwhile.' The lapse was recognized visually at once. It would seem induced by visual suggestion. In the manuscript the first 'hoping' is about two inches to the left and a half inch above the second 'hoping'."

Other sorts of lapses were of course recorded. Perhaps most interesting of all are those which result in a hybrid word, due to coalescence, of the intended and of the automatic word. Sometimes introspection reveals an inchoate or confused meaning-consciousness, which is highly instructive. One or two examples may be given.

8. "Wrote 'Emotent,' for 'Emotion constitutes the content.' This lapse was recognized as soon as written. (No analysis)."

9. "Wrote, 'Going to the door I saw Dig outstretched on the back porch.' There was a conflict in writing-cue between 'Nig' (name of our cat) and 'Dick' (name of our dog); 'Dick' prevailed as cue. It was not until rereading my notes next day that I perceived the coalescence of the two cues. This lapse escaped the visual censor."

10. "In correcting a student's theme I started to write 'please' for 'believe.' Recognized this error when I started on the below-line stroke of 'p' (a visual and motor recognition). Auditory-vocal-motor cue was a blurred 'b-l-l.' I had just been thinking out instructionss to class. I intended to say. 'Please save this theme.' There was a perseveration of meaning which was influential, together with the similarity in sound between 'b' and 'p,' in causing the lapse."

None of these lapses, except possibly 3 and 7 lend themselves to a Freudian interpretation. That Freudian lapses, motivated by unconscious complexes do occur I am convinced. I have, for example, written 'toxin' when I meant 'tocsin' and the like, but such lapses seem the exception, not the rule.

APPARATUS NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

Protractor for Color-Mixing. The annular brass protractor for color-mixing, which cost over six dollars before the war, costs considerably more now. Although it is very satisfactory in use, it is expensive when purchased in quantity for laboratory courses where several experiments in color-mixing go on simultaneously.

In the Clark Laboratory we have duplicated this protractor in a line engraving that can be printed on cardboard. The cut prints an annular ring, 13 cm. outside diam. and 10 cm. inside diam, with a scale of single degrees marked on both outside and inside circumferences. The center of the print is marked with concentric rings that aid in punching a hole for the disk-cutter. The outer circumference is cut entirely about by the disk-cutter. In cutting the inner circumference a bar (2 cm. wide) is left to stretch across the circle and to support in the center another annular ring that fits over the spindle of the mixer and centers the proctractor. The bar and central ring could be made of metal and the cardboard scale cemented thereto, but it seems to be satisfactory simply to cut the whole protractor out of the cardboard.

When printed on 4-ply board this protractor is stiff enough to stand ordinary usage, and thin enough to permit accurate reading at the edge. It is easy to read half-degrees on both inner and outer scales.

The Clark Laboratory will be glad to supply psychologists with these protractors, printed on cardboard but uncut, at cost which is at present about two cents apiece.

Artificial Daylight. Although the type C-2 Mazda lights do not give an exact artificial daylight, they provide a constant illumination that is more satisfactory when colored papers are used (demonstrations, the drill experiments in color, color preferences, *etc.*) than is uncontrolled daylight.[1] The uneven illumination can be avoided by having the lamps frosted at an electrical store. The frosting reduces the illumination scarcely at all; and it makes the lamp less uncomfortable in direct view.

E. G. B.

Exposure Apparatus for Memory Experiments. An inexpensive form of apparatus for successive exposures, devised by the late Professor J. W. Baird, has been found to be of service for presenting the materials of memory experiments in elementary laboratory classes and in group demonstrations. Nonsense syllables of black gummed letters, 1 in. high by 0.8 in. wide, are pasted on cardboards, 7 in. by 5½ in. These cards are then made into a booklet by the use of gummed cloth tape.[2] The tape is stuck on the bottom edges of the back of one card and of the front of the card behind it. In the same manner, all the cards are successively fastened into a booklet. A blank card is put at the front of each booklet. It is convenient in pasting the letters on the cards to use a cardboard form with a rectangle of the extent to be covered by the letters of the nonsense syllable cut from the center. This form is placed over the card on which the letters are to be pasted, and assures uniformity in both the placement and the spacing of the letters. The cards are then notched or 'indexed' across the upper edge so that the experimenter can readily drop one page at a time. When the last card has been dropped, the booklet lies face downward on the table. The experimenter can then pick it up and give a second exposure of the series by again dropping the pages one at a time.

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[1] Cf. G. J. Rich, this JOURNAL, xxx, 1919, 313-315.

[2] Cf. E. O. Finkenbinder, this JOURNAL, xxiv, 1913, 12.

The booklet can also be made with a blank card at the end and the cards notched or 'indexed' on the side.[3] In this case the last blank page is attached by thumb tacks to an upright blackened board, in which a narrow slot of a size corresponding to the notching on the edge of the booklet is cut. This booklet is held up in position by a small metal hook which is passed through the slot in the board; and the exposures are made by moving the metal hook upward over the successive excisions in the cards. This form of booklet has an advantage over the first in that the observer by its use is less apt to be distracted by the sight of the hands of the experimenter S. W. F.

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[3] Cf. S. C. Fisher, *Psychol. Mono.* xxi., (Whole No. 90), 1916, 35.

NOTES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

By E. B. Titchener

1. Electromagnetic Control of Stop-Watch

This little instrument serves a twofold purpose: it safeguards the stop-watch against damage by careless handling, and it enables *E* or *O* to actuate the watch at a distance. The control was suggested to me by Jastrow's "Electric Attachment for the Stop Watch," which is figured in the catalogue of the Garden City Model Works, 1894, no. 11. It consists essentially of an electromagnet with pivoted armature, reinforced by a coiled spring. Watch and magnet are cased in a house of cast metal, with round window for the watch-face; and the clips that hold watch to stand, the pin that governs the tension of the spring, and the actuating button of the armature are all adjustable, so that the control may be applied to any make of watch. If the spring is set at the right degree of tension, and if the armature button is centred on the stem of the watch and moves in accurate alignment with the push-pin, the arrangement works—with the ordinary cheap stop-watch—on a single dry cell; a refractory watch may need two cells. The click at make, which is sometimes disturbing, may be lessened by papering the face of the armature.

The control is sold by the C. H. Stoelting Co., 3047 Carroll Avenue, Chicago, Ill.

2. Curve Tracer

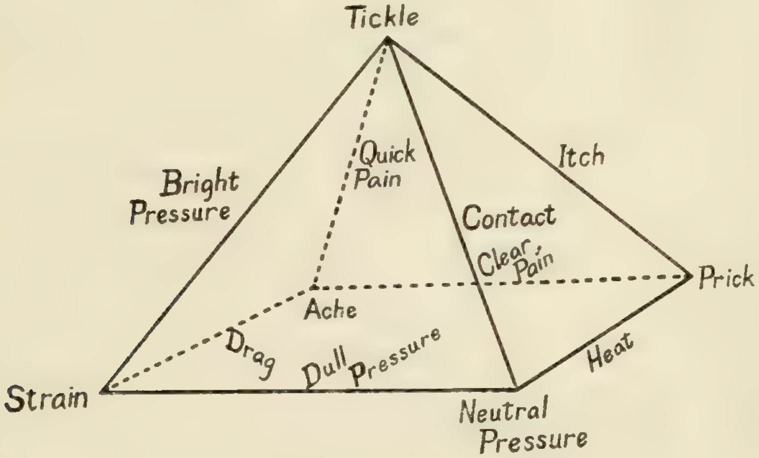
It has always been difficult to draw accurately arcs of circles of large radius. Since these curves are variously useful in laboratory work, I have asked the C. H. Stoelting Co. to manufacture Weitzenböck's device (described by K. Bühler, *Die Gestaltwahrnehmungen,* i., 1913, 72). The model supplied to the Cornell Laboratory has been subjected to preliminary tests, and appears to work as accurately as the Wolz original.

3. Models for the Demonstration of Sensory Qualities

There is nothing like a geometrical model for bringing home to beginners in psychology the nature of the qualitative series and the positional character of the qualitative attributes. Some years ago I described a demonstrational color pyramid (this JOURNAL, xx., 1909, 15 ff.). I wish now to say a word regarding a set of four simpler (frame or outline) models that I have found useful for demonstration in introductory courses. Dimensions do not matter, though on general principles the larger the models are made, the better.

The frames are built of light metal rods, blackened, whose ends are held together by cubical blocks of blackened wood. The blocks are bored to receive the posts of paper flags, which show the initials (or other symbols) of the terminal qualities. The rods further carry one or more sliding blocks for the similar indication of intermediate qualities. The frames thus constructed will stand on any face; the flags may be interchanged for demonstrational purposes within the given model; and wires may be stretched as wanted across surface-planes and interiors.

The four models are the color double pyramid; the taste tetrahedron and the smell prism (H. Henning, *Zeits. f. Psych.*, lxxiii., 1915, 254 f.; lxxiv., 1916, 212); and a touch pyramid, here figured.



I need hardly say that several of my determinations of the touch qualities are tentative, and that I regard their arrangement at best as only a rough approximation to the truth. I publish the figure because I think it marks the direction of recent work; because it may perhaps serve as the starting-point of new experiments; and also for the more personal reason that I have shown the model in my lectures of the past two years, and that the observers in certain investigations shortly to be published from the Cornell Laboratory have therefore been familiar with its terms and series.

For the place assigned to heat, see F. Cutolo, this JOURNAL, xxix., 1918, 445, 447 f.; for what I have called 'quick' pain, see E. Becher, *Arch. f. d. ges. Psych.*, xxxiv., 1915, 205. Warmth and cold for the present baffle me completely; I cannot bring them into qualitative relation with the terms of the figure; but the fact that they are left outstanding—I admit that it is a real difficulty—cannot of itself discredit the figure, and can still less discredit the underlying idea. Finally, auditory sensations may be systematized in various ways; and I have no doubt that some sort of closed figure will presently be found adequate to them. So long, however, as tonality and vocality are in debate, it is hardly safe to propose an arrangement of qualities.

4. SEWING MACHINE MOTOR

I am indebted to Dr. E. G. Boring, now of Clark University, for calling my attention to this motor, which is sold under the name "Sew E-Z" or "Home Motor," by the Hamilton Beach Manufacturing Company, Racine, Wis. The motor is intended for use with sewing machines, and in other household operations. For color mixing in the laboratory it has various merits; it requires practically no attention and stands hard usage. The motor is strongly cased, and its oil-cups are accessible. It will operate on either alternating or direct 110-volt current. It runs in both directions: the direction is reversed, very simply, by loosening a set-screw and shifting the brushes.

The motor is provided with a 6-speed rheostat, operated against a spring by a foot-treadle. The rheostat can be removed from its attachments, and turned by hand on the table. Thus separated, it is convenient and compact: 4 cm. high by 7 cm. diam. The motor itself with its base goes within a 15 cm. cube. The motor is light in weight, and though it is steady enough for ordinary color mixing must be clamped to the table if the discs are unbalanced (as in photometry by the Kirschmann method).

The motor has very little power, but great speed. Boring's tests show that on no-load the six points of the rheostat give about 2000, 4500, 7000, 9500, 12000 and 13000 r. p. m. A load of discs largely reduces this speed; but 20 cm. discs can be rotated on the fourth rheostat point at a speed of 4000 r. p. m., which is ample to prevent flicker.

The Cornell Laboratory has had the sewing machine motor in steady use since 1916, and the Clark Laboratory has recently adopted it. The current price (without discounts) of motor, rheostat, connecting cord and pulley is $18.50.

NOTE ON THE EXPERIMENTAL STUDY OF ATTENTION

By K. M. Dallenbach

It seems necessary to reply briefly to two recent criticisms of my work on the experimental investigation of attention (this Journal, xxiv., 1913, 465 ff.; xxvii., 1916, 443 ff.).

(1) With the remarks of K. Koffka (*Zeits. f. Psych.*, lxxiii., 1915, 281 f.) I am, if I understand them aright, in substantial agreement. But that, of course, is to say that I regard them as irrelevant, in so far as they are intended for a criticism of my method. Koffka, from the standpoint of his own theory of attention, attempts an analysis of the conditions of attention operative in my experiments. The conditions of observable clearness, however, mattered as little to my observers as would the conditions of the appearance of two greys in an experiment on the discrimination of brightness. We, by hypothesis, were observing a sensory attribute of the intensive sort, and were trying to correlate the quantitative changes of this attribute with objectively measurable variations. Clearness was the starting point, not the end-point, of the investigation.[1]

(2) H. Henning (*ibid.*, lxxxii., 1919, 245) adopts a more polemical tone, and is led thereby into several inaccuracies of statement. These are of minor importance: the serious thing is that he has failed to appreciate the significance, in this kind of work, of the 'wrong' judgments, and the interrelation of the Single Task and Double Task methods. He wholly misses the central fact of my results (though I naturally laid special stress upon it), that the curves of the 'right' judgments are skewed toward the higher, those of the 'wrong' judgments toward the lower degrees of observed clearness. A reader who should take Henning's review as adequate to my paper would, therefore, be misled.

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[1] Britz' *Theoretische u. experimentelle Untersuchung ueber d. psychol. Begriff d. Klarheit* (1913) has been discussed by Titchener (*Psych. Rev.*, xxiv., 1917, 43 ff.). I have myself repeated and extended Britz' experiments, and give a critical account of the results in the *Journ. Exper. Psych.*

BOOK REVIEW

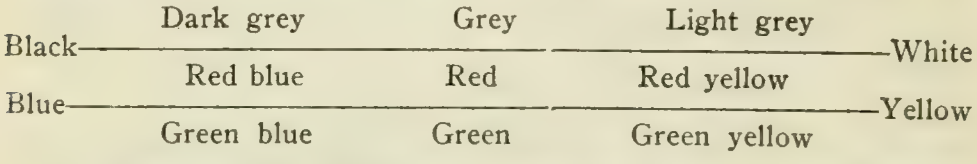
Anpassungstheorie des Empfindungsvorganges. By Julius Pikler, Leipzig, Johann Ambrosius Barth, 1919.

1. *Hypothesenfreie Theorie der Gegenfarben.* 104 p.
2. *Theorie der Konsonanz und Dissonanz.* 34 p.

These two contributions to the study of visual and auditory theory clearly take root in a more comprehensive doctrine of sensory phenomena in general, which the author published in 1917 (*Sinnesphysiologische Untersuchungen,* Leipzig, 516 p.). It would seem that this latter doctrine has indelibly colored the University of Budapest professor's outlook upon data presented by the separate sense departments. The first part of the present monograph (published separately) treats of visual theory. Hering's views on this subject come in for the brunt of adverse criticism.

Bk and *W* are at opposite extremes of an uniform scale of brightnesses which passes through a central or normal point of middle grey. The somatic basis for the sensation of *Bk* is found in the complete absence of that process which conditions *W*, and the basis for greys in the varying degrees of partial absence of this process. The psychological antagonistic positivity of *Bk* derives from the representation in consciousness of a normal middle point in this process to which the organism has become *eingestellt,* and as a result of which deviations in either direction are perceived. The seeing of *Bk, W,* and greys, then, is conditioned by corrective adaptation processes with respect to the maintenance of an autonomous middle grey equilibrium. These adaptive corrections tend to raise or lower the point of equilibrium, from which in turn it follows that these same corrective processes in the direction of increase or decrease become greater or less,—whence the phenomena of simultaneous contrast. Positive after-images are due to the continuation of the adaptation process after cessation of stimulus; negative after-images to the excess of adaptation process after prolonged stimulation which is adequate to a new stimulus in the direction of greatest opposition to the previous sensation.

For the phenomena of color tone, Pikler elaborates and expands the above theory. In rough, his position is somewhat as follows: Colors distribute themselves along a scale in accordance with brightness values similar to the achromatic series. Each hue possesses a constant and "specific" tint. *B* is at the bottom of the brightness scale, *R* and *G* in the middle, and *Y* at the top. The following diagram represents the relation of the achromatic to the chromatic scale:



The phenomena of color vision are explicable only in the light of their brightness components, since the somatic processes underlying the latter are in reality the basis of the former as well. If this be true, then it is clear that *B* and *Y*, e. g., are antagonistic by virtue of their opposite location on the brightness scale. The same explanation given above for contrast, after-images, etc., is applicable to similar hue phenomena.

Part 2 is an extension of the author's general adaptation theory of sensation to the field of audition. Strangely enough, consonance and dissonance is the only subject treated!

The hearing of tonal intervals is fundamentally the perception of geometrical relations. We perceive in the octave the geometrical relation 1:2. The upper tone is the same as the lower, only doubled. This is explained in terms of the adaptation theory by the arousal of a sensation process by the lower tone of the octave, and a corrective adaptation sensation process just double in size by the higher tone. In the case of the fifth, we have the geometrical perception of the higher tone half double the lower, i. e., we perceive the ratio $1 : 1+1/2$, not 2:3. With the major third the upper tone is perceived to be the same as the lower, only one-fourth doubled, i. e., $1:1+1/4$, instead of 4:5. These intervals are the only ones characterized by sameness of components, whence derives their consonance. All other intervals are perceived arithmetically, and the component tones are heard unlike and dissonant.

It is rather difficult to escape the conviction that facts of audition have here been forced into an hypothesis by dint of uncritical analogy with space perception. Remarks Pikler, "Der Gedanke, dass es auf dem Tonhoehengebiete geometrische Verhaeltniswahrnehmungen gibt, ist, soweit ich sehe, bisher niemandem gekommen." How stupid psychology has been!

C. C. PRATT.

BOOK NOTES

An Introduction to Social Psychology. By WILLIAM McDOUGALL. Lond., Methuen and Co., 1920. 459 p.

This is the fourteenth edition of this author's most important work. He has tried to deal with a difficult branch of psychology in a way to make it intelligible to the cultivated reader and without implying any previous familiarity with psychological treatises on his part. There is considerable elaboration in Chapter 3 of a principle formerly enunciated, namely, that all emotion is the affective aspect of instinctive process. He combats the current view that imitation is to be ascribed to an instinct, elaborates his conception of sentiment in a somewhat new way, analyzes the principal complex emotions in the light of the conception of the principle laid down in an earlier chapter on instinct and instinctive process, and finally applies the results of his theories to the description of the organization of the life of emotion and impulse.

The Problem of the Nervous Child. By Mrs. ELIDA EVANS. (With an Introduction by Dr.C. G. JUNG of Zurich.) N. Y., Dodd, Mead, 1920. 289 p.

After years of study of nervous children and parents by the psychoanalytic methods of Dr. Jung, the author here presents an account of the part played in the present life of the adult by mentally bad environment during childhood. The book has many practical illustrations of mistakes, and the cases showing how present nervous trouble in adults is directly traceable to childhood gives the work a certain interest for others than parents and teachers.

Lehrbuch der Logik. By TH. ZIEHEN. Bonn, A. Marcus and E. Weber, 1920. 845 p.

This comprehensive work is connected with the author's psychology and theory of knowledge but independent of them, as the work in pure logic should be. He deals first with the problem, then with the general theory of logic, with its relations to epistemology and psychology, the doctrine of judgment, and finally states his inferences and conclusions.

The Ground and Goal of Human Life. By CHARLES G. SHAW. N. Y., Univ. Press, 1919. 592 p.

The author here attempts a "treaty of peace" between the forces of individualism and those of social thought. In Book I, The Ground of Life in Nature, he deals with the self as thinker, the empirical ego, the surrender to naturalism; Part II of this first division discusses the struggle for the joy, worth, and truth of life. Book II treats of the Goal of Life in Society, including the transvaluation of self and society, the repudiation of sociality, etc. Book III, entitled The Higher Synthesis, takes up the question of the joy, worth, and truth of life in the world-whole.

Life Movements in Plants. By Sir J. C. Bose. Calcutta, Bengal Govt. Press, 1919. 597 p.

This extremely able student of the physiology of plants here sums up his recent experiments, with considerable reference to those that have preceded. It is interesting to note that he has founded an Indian school of physiology, which is well subsidized and of which his country should be very proud. His ingenuity, originality, and independence of thought have enabled him to carry the subject of physiology of plants very distinctly beyond that which any of the many pioneers have attempted before.

Education in War and Peace. By Stewart Paton. N. Y., Paul B. Hoeber, 1920. 106 p.

Peace, no less than war, produces shell-shock, the symptoms of which interfere with individual efficiency, happiness, and social progress. This work calls attention to the urgent necessity of making adequate provision in our universities for training investigators competent to undertake the solution of the vital important educational problems now confronting civilization. The chapters are: Human Behavior in War and Peace, War and Education, and The Psychiatric Clinic and the Community.

A Beginner's History of Philosophy. By Herbert Ernest Cushman. Bost., Houghton Mifflin, 1919. 407 p.

We have here Volume II, entitled "Modern Philosophy" (1453 to the present time). The author's work is intended as a textbook for sketch-courses in the history of philosophy and is written for the student rather than for the teacher. It is based on a background of geography and literary and political history, and its aim is to arrange and organize the material of the history of philosophy for the beginner. This volume begins with the causes of the decay of the civilization of the Middle Ages and ends with the philosophy of the nineteenth century.

Studies in Contemporary Metaphysics. By R. F. Alfred Hoernlé. N. Y., Harcourt, Brace and Howe, 1920. 314 p.

These studies are described as "chips from a metaphysician's workshop" or "blocks hewn out experimentally in the effort after a systematic synthesis." They are like the painter's sketches or the sculptor's modelling in clay, which precede the finished work. The book consists of ten papers with a common animus but more or less independent of one another. He deals with the scientific method in philosophy, the philosophy of Nature, "doubting the reality of the world of sense," mechanism and vitalism, theories of mind, the self in self-consciousness, and religion and its philosophy.

The Intellectuals and the Wage Workers. By Herbert Ellsworth Cory. N. Y., The Sunwise Turn, 1919. 273 p.

The chapters here are: Equality, Proletarianism, Religion, Criticism, History and Freedom, Liberty, The Class Struggle and Fraternity, and Education: A Program for the American University. It is dedicated to Carleton H. Parker, whose name suggests the spirit in which the author writes.

Spiritual Pluralism and Recent Philosophy.. By C. A. RICHARDSON. Cambridge, The Univ. Press, 1919. 335 p.

This work is dedicated to James Ward from the inspiration of whose teaching it derives its main theme. The author's purpose is to present and defend the theory that the texture of the Universe is through-and-through spiritual but he admits that he has been influenced by the Neo-Realists of America and by Bertrand Russell.

Pagan and Christian Creeds. By EDWARD CARPENTER. N. Y., Harcourt, Brace, and Howe, 1920. 319 p.

In this book the author of "Civilization: Its Cause and Cure" propounds a new theory concerning the origin of religious rites and ceremonies to prove that Pagan and Christian cults had the same source. He deals with the astronomical and magical aspects of the subject, the rites of initiation, redemption, sex-taboos, ritual dancing, legends of the Golden Age, the Eleusinian Mysteries, and the progress of Christian thought.

La Meccanica del Cervello. By LEONARDO BIANCHI. Rome, Bocca, 1920. 425 p.

This work sums up the extended investigations of the author on the mechanics of the cerebellum.

Aphasia and Associated Speech Problems. By MICHAEL OSNATO. N. Y., Paul B. Hoeber, 1920. 191 p.

This is a summary of the subject up to date.

COMMUNICATION—PSYCHOLOGICAL TERMINOLOGY

The Committee on Terminology of the American Psychological Association is taking up for examination terms in the fields of Sensation and Cognition. Psychologists interested in the precise use of terms are invited to assist the Committee in its work by calling the chairman's attention to—

(1) Psychological terms used with two or more different meanings (whether distinguished or not), and terms used indefinitely or ambiguously in contemporary writings.

(2) Pairs or groups of terms which lead to confusion when used interchangeably.

(3) Foreign terms needing definition or translation.

(4) Books and articles containing systematic lists of cognate terms, or discussions of ambiguous terms. (Full references desired.)

It is a matter of prime importance in any science to clear up double meanings and imperfect synonyms. The word *feeling* is used in standard psychological works with several different meanings. The words *intellect* and *intelligence* are used by some writers interchangeably, while others draw a sharp distinction between them. There are many instances in the literature of both kinds of confusion.

The Committee wishes to include a large number of such terms in its next report, either defining and distinguishing them or citing discussions in easily accessible sources. This list will not be confined to sensation and cognition, but will cover the entire field of psychology. Will readers of this magazine assist the Committee to make the list fairly complete?

HOWARD C. WARREN, Chairman,
Princeton University.

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PSYCHOANALYSIS OF CHARLOTTE BRONTË, AS A TYPE OF THE WOMAN OF GENIUS

By Lucile Dooley, Ph. D.

(Note.—I wish to make grateful acknowledgment to Dr. G. Stanley Hall, my teacher, for inspiration and encouragement received from him in the preparation of this paper. In its later revision I owe thanks to Dr. Edward J. Kempf for many helpful suggestions.)

Introduction

Few English authors, whether men or women, have been so much written about as has Charlotte Brontë. Biographers, critics, interpreters, have been busy from the day, in 1847, when *Jane Eyre* burst upon an astonished world, till 1912, when Miss May Sinclair, in her book called *The Three Brontë's* (1) gave us an appreciation and an interpretation that fully answered her own question, "Why *another* Brontë book?" Miss Sinclair, with her always wonderful insight, has come very near to the psychoanalytical view of Charlotte Brontë's character, if not of her genius. There is very little to add to her interpretation of the novelist's attitude toward children, and she was the first to comprehend the truth here. Among others who have approached the psychoanalytical viewpoint, without any specific knowledge of Psychoanalysis, are Swinburne (2) and G. K. Chesterton (3). A quotation from the latter will suggest the basis of this new interpretation. Speaking of *Jane Eyre,* in his essay on Charlotte Brontë in *Twelve Types,* Chesterton says:

"Yet despite this vast nightmare of illusion and morbidity and ignorance of the world Jane Eyre is perhaps the truest book that was ever written. Its essential truth to life sometimes makes one catch one's breath. For if it is not true to manners, which are constantly false, or to facts, which are

almost always false, it is true to the only existing thing which is true, emotion; the irreducible minimum, the indestructible germ." (p. 7.)
and again:

"Upon the whole, therefore, I think it may justifiably be said that the dark wild youth of the Brontë's, in their dark wild home, has been somewhat exaggerated as a necessary factor in their work and their conception. The emotions with which they dealt were universal emotions of the *morning of existence* (the italics are mine) the springtide joy and the springtide terror. Everyone of us as a boy or girl has had some midnight dream of nameless obstacle and unutterable menace in which there was, under whatever imbecile forms, all the deadly stress and panic of Wuthering Heights. Everyone of us has had a *daydream* of *our own potential destiny* not one atom more reasonable than Jane Eyre." (Again the italics are mine.)

The power of depicting and analyzing emotion in Charlotte Brontë, and the suggestion that it was derived from the psychical experiences of her early life are here the points that we focus upon. It is my plan (or hope) to show that it *was* the childhood of Charlotte Brontë, though not necessarily the gloomy external features, that were the determining influences giving shape to her literary art in later years, giving her much of the power she had, that compelling power that enchains our sympathies in spite of the flaws and errors picked out by critical intellects. I mean to show that it was the interaction of the family circle that gave her those infantile emotions, preserved untransformed by the processes of normal development and escaping from her pen with the force of dammed waters when, other outlets denied, she sought the long-accustomed outlet of literary composition, after adult life and experience had matured her powers.

Her own testimony that her stories, to the very words, came to her whole and unalterable, out of what some of us now choose to call the Unconscious, is corroborated by the deep appeal, deeper than reason, which her works make to our feelings, an appeal that gains our intuitive assent to her every portrayal of the soul-life of men and women. It is deep calling to deep in a very real sense, and she could not possess this power unless the primordial unconscious soul of her remained unchanged and accessible as it does in the abnormally developed personality—the neurotic or the genius. There are reasons enough pointed to by every biographer, why she should not have, and could not have developed normally. There is evidence enough that she was essentially neurotic—in the sense of nervous instability—that while not morbid she was certainly not normally healthy, and that her own inner life dwarfed for her all external experiences and gave her the materials out of which she created those soul-searching tales in which the inner life of woman is set forth as never before or since by any English writer. The secret of this tremendous power of passion in her fiction is emotional conflict in her own soul. This conflict involves a partial repression of one of its exciting causes into the lower strata of subconsciousness, but the emotional energy attached thereto finds its way out through the channel of novel writing with force unabated.

And I wish to show that the initial impulse of this life-long conflict was that earliest of emotional complexes, the complex centering in family life, with father and mother playing the leading rôles. It is a complex of ambivalent feeling toward a strong, stern, powerful, benevolent, adored father, who is loved and feared, obeyed and rebelled against, worshiped and hated, alternately, and, also, simultaneously. These feelings do not find, necessarily, an obvious expression, but they play their part on the stage of the child-soul, as psychoanalysis has shown. That the subject of this study, Charlotte Brontë herself, understood this phase of child psychology, as I shall try later to show, is demonstrated in her descriptions of childhood.

From her life, from her letters, and most of all from her books, we find evidence that the father-personality was the dominant figure in her emotional life. The one great adult love of her life—the passion for her teacher, Monsieur Heger—was the natural outgrowth of this. The love grounded in an unconscious infantile bond to the parent goes out only to the person who, like the parent, is inaccessible, (4, 5, 6). The passion which she felt for the unchanging Father Imago, enhanced by the bars set up against gratification of her desire, she attributed to her heroines, freeing them from the wall of thorns surrounding her own love, dramatically in Jane Eyre, where the mad wife perishes in a spectacular conflagration, more conventionally in her later books, where rank or creed, or race, constituted more vulnerable defenses. The infantile situation, however, is repeated vividly in each of her few tales, with identical elements. Only the externals are altered. In the author's personality the infantile character was preserved unchanged, a fact to which her many biographers bear witness. The reason for this is easily seen when her emotional life is anaylsed.

The tyranny of a close-knit family, especially of a father, a long unbroken isolation in a gloomy home, and the disparity between her upbringing and that of all her fellows, were things that warped and set her apart. Yet the intellectual stimulus, the literary custom of the household, furnished her with a most fortunate road to achievement. We must briefly survey the setting of this all-important period of life, her childhood.

Chapter I. Her Parentage and Childhood

In every study of individual character we must take account of the hereditary background and the congenital disposition of the subject, because, however great the influence of environment and experience, modern research has demonstrated that inherited character is the substratum on which the development of the individual soul must rest, in the last analysis. The inherited disposition determines the specific nature of the reaction to common stimuli, and so helps to create and to modify the environment that is always at work modifying it. This is clearly evident in every psychoanalytic study yet made. Only a responsive and sensitive nature can be far deflected from its normal course by the emotional stimuli that are found to affect so profoundly the growth of the neurotic personality and the genius. In view of the facts of her life, Charlotte Brontë cannot be held an exception to this rule.

Patrick Brontë, the father, was of an Irish peasant family; he rose by force of intellect and ambition to the place of clergyman in the Established Church of England, having made his way through Cambridge University. He lived beyond his eightieth year but was not, in his children's life time, a healthy man. He suffered from what was then called hypochondria, from cataract, and from nervous dyspepsia. In temperament he was not unlike his brilliant daughter, suffering much from depression, but of indomitable will. The mother died of cancer two years after the birth of her sixth child. The two eldest children died of pulmonary tuberculosis at the ages of twelve and eleven. Charlotte, the third child, suffered all her life from a nervous instability that took generally the neurasthenic form but was often of a pronounced hysteric nature. Emily and Anne, the youngest of the six children died of tuberculosis at the ages of twenty-nine and twenty-eight. The only boy, Branwell, was also of nervous temperament which resulted in moral delinquency before it came to physical failure, and he died finally of tuberculosis aggravated and hastened by dissipation and drugs.

Here is a sufficiently tragic picture, with scarcely a redeeming feature. Farther than the parents the family history is not pushed back in detail, but we can surmise that it could not have been favorable, since in the two generations known there is not a single one whose diathesis could be denominated as "negative" or "good." The bright spot in the sorrowful history is the extraordinary brilliance of most of the children, indeed there is reason to believe that both parents were above the average. Both had written and while their works possess no merit to speak of it shows a temper and ability above the average to have written at all. The mother did not publish anything, but her one extant manuscript and her letters show some slight ability in literary expression and a delicate and genial wit. She was a native of Cornwall and the combination of Cornish and Irish blood may well be supposed to have resulted in unusual offspring.

Of the six children born to these parents at least three possessed genius. These were Maria, who died too early for the flowering but who might have been an infant prodigy and who might have equalled her sister; Charlotte, the best known of the family; and Emily who was indisputably the most powerful mind of them all. It is a matter of dispute as to whether the boy Branwell really possessed the capacities credited to him in boyhood. If he did, they were blighted and destroyed by vice and disease. It seems fair to presume, from the records left of him that he might have been moderately successful in art or literature had he made the most of his talents, though it is unlikely that he would have commanded lasting fame. Of the two remaining children, Elizabeth and Anne, little can be said except that they were far below the others, and yet, perhaps because of the family environment, considerably above the average girl in intellect and originality. That a family of eight should possess three persons of genius and five of more than average ability is somewhat remarkable, at least.

Intellig
& Educa

The isolation, the dreariness, the positive gloom, that characterized the home of the Brontës has been too often dwelt upon, yet it cannot be passed over here. Mrs. Gaskell's remarkable "Biography" (7) has left an indelible impression on the reader's mind of the wild, desolate moor, the grey, cold, blank, stone house, next to the Churchyard choked with graves, the rude village, the inhospitable climate and soil. And one of Charlotte's girlhood friends declared that this picture was "not so gloomy as the truth." The fact that the children had practically no intercourse with others of their degree, or with anyone outside their family, until they were nearing womanhood, must be taken into account since it intensified the influence of heredity, of the family circle, and of the cheerless home. At any rate there was no mitigating

Tragic

and corrective contact with the outside world. The family situation, the family complex, which is the first influence on every life, and is likely to be the most important, is doubly, trebly, significant for Charlotte Brontë because it continued so long paramount. So long was it uninterrupted by normal external factors that when these factors came they could not achieve their proper effect. She remained fixed in her infantile mould, emotionally and temperamentally, although her intellect, well nourished, was capable of a marvellous growth. The influence of the family complex was still further intensified by the circumstance that it was a group of unusually strong personalities and that the father was always the dominant one of these personalities.

Patrick Brontë's character was a contradictory mixture of reserved, sensitive pride and rugged stoicism, of sternness amounting to harshness and half-ashamed tenderness; he was self absorbed but had strong fatherly feeling. I do not know how better to present him in an outline sketch than by saying that his was an intense and emotional nature that was never permitted free expression. Sensitive pride, and self conscious shame kept an uneasy leash upon the natural feelings of his susceptible nature. He had "volcanic fires burning under his cold exterior" (Mrs. Gaskell's Biography). His was just such a character as I have seen in the energetic and ambitious American descendants of Irish Protestant Peasantry but I do not know enough of the racial, religious and personal elements involved here to venture a guess as to the significance of this similarity.

While the stories of his temper related in Mrs. Gaskell's Biography are now considered as the exaggerations and perversions of un-learned servants it is very certain that he was not a comfortable man to live with. Not only was he an ascetic, and a cautious, cold, distrustful, almost suspicious recluse, but he was one in whom confidence could not be reposed because his reaction could never be foretold. His children could never be sure of his sympathy, however carefully he looked after their welfare, they could never freely share their joyous or sad moods, their hopes and fears with him, because they never knew when their frank self-expression might bring a rebuke. His presence imposed constraint and self-repression, even while they loved and honored him, and fully recognized his solicitude for their welfare. In that solicitude he was very *father-like* in that he trusted his own judgment supremely and seldom thought of consulting the objects of his care as to their own views on the matter. Fathers with this character only a little less pronounced generally esteem themselves and are esteemed by others as "good fathers," yet some neurologists know that they are more or less directly responsible for the plight of many a nervous and invalid girl and many a wayward and ineffectual boy.

Some writers on the Brontës have drawn a dark and forbidding picture of this father, while others have come to his defense with recitals of his fatherly concern for his children, his real affection for them, and his pride in them. All this defence only goes to show that he meant well and does not mitigate the unfortunate truth that his influence on their lives (and the most powerful of all influences it was) did not make for their happiness, although it had a great share in developing the genius of at least one of them. The conflict of opposite mental traits, which his nature was not strong enough to sustain, brought about physical conditions of health that reacted again upon his mental life, intensifying the repressions and the asperities of his pride and his over-sensitiveness. How his hypochondria and gloom acted upon Charlotte will appear more clearly in a later chapter, where her own physical and mental sufferings are treated. In her childhood his physical condition undoubtedly acted upon her but it was his personality and his fatherly relation that had the formative influence. He was, quite naturally, the autocrat of the house, and he strengthened his position by setting his face sternly against any indulgence or possible encroachment on his absolute authority. He had an eighteenth century notion of the benefits of the "hardening process" for childish bodies and souls. He wished to make his children indifferent to all the pleasures of the flesh, to food, to dress, to society. He succeeded—only too well. Such a discipline involved many a needless deprivation, of which little children—unquestioning of the wisdom of their elders though they be—must often keenly feel the injustice. Moreover—and this is most significant—he was something of an absentee monarch—keeping himself shut up in his study, seeing the children but seldom. His rule would seem the more arbitrary, and the less reasonable and sympathetic, for this circumstance.

Frau Lou-Andreas Salomé in her remarkable analysis of adolescent and pre-adolescent girls contained in her book *Im Zwischenland* (8) has shown the psychological effect of the absent or otherwise distant parents in the story of Mascha and Dascha. Considering the feasibility of a certain infantine misdemeanor these two, whose father was far away and whose mother was dead, decided that it would not do as there was no one to care or to punish them. Also that it would be pleasant, on the whole, to have some one to punish them when they did wrong! This feeling probably grew in the breast of little Charlotte Brontë in the first five or six years of her life for it is expressed in many ways by the orphan-heroines of her books. Her father was there, and he was ruling, but from a distant height, and he neglected details. He was not one to listen to small troubles and to settle disputes. This should have been the mother's place, and there was no mother after Charlotte was five years old. So it came about that this father inspired in his daughters submission, deference, fear, along with adoration which reached up to him over a cold chasm of neglect and unsympathetic repression.

Mrs. Gaskell records that when, only a year or two before Miss Brontë's death, she visited at Haworth Charlotte's attitude toward her father was still that of a child. "Mr. Brontë never seemed quite to have lost the feeling that Charlotte was a child to be guided and ruled when she was present, and she herself submitted to this with a quiet docility that half amused and half astonished me."

This was when the father and daughter were more together than ever they had been in her childhood. More of tender intimacy in those early days would have softened the blighting effects of his strong hand upon her—effects apparent to the end of her life.

It has been often remarked that the keynote of her character was devotion to duty, and this was always shown as the duty of keeping her father's house and caring for him. Thought for him entered into every step of her life and controlled her choice. To others this devotion may have seemed unnecessary, to Charlotte's friend, Mary Taylor, it seemed wrong that talent of such a high order should be so confined and wasted, but it never seemed wrong or unnecessary to Charlotte herself, however much the other side of her nature rebelled. It is clear that her father's influence penetrated to the deepest springs of her nature. In her pathetically spirited answer to that letter of Southey's in which he advised her that "literature cannot be the chief business of a woman's life and it ought not to be," she wrote, "I try to deny myself, and my father's approbation amply rewarded me for the privation."

She came home from Brussels to devote herself to the care of her father and her father's house, she refused to leave him for London, after her fame had grown, she refused four offers of marriage and when she finally did marry, it was, notwithstanding his opposition, partly for his sake. She loved him with a devotion that had yet its ambivalent side of fear, and not with the love of a woman grown, but with that of a child, just as, in childhood, she loved him with a half womanly adoration. When an emotion is called forth before its time and is allowed to become paramount, it crystallizes, "fixates," to use a Freudian term, and the natural course of its evolution is stopped, it remains in something near its pristine form. So it was with Charlotte and her feeling toward her father.

Mrs. Bronte was an invalid and scarcely saw her children after Charlotte was old enough to remember. The six tiny creatures ranging in age from ten to two, amused themselves and, for the most part, cared for each other. From the time Charlotte was three years old Mother was but a vague and unknown personality to her and she has but two or three dim and misty pictures preserved of the one who could have neutralized and sweetened the bitterness of her childhood. During the years of Mrs. Brontë's illness her husband was much occupied with nursing her and the children were even more neglected then than after her death. Moreover they came so near together that Charlotte, just in the middle, could not have known the indulgences of babyhood long. She was but eighteen months older than Branwell, and then came Emily, one year younger than Branwell, and Anne, a year younger than Emily. Maria, the eldest child, was, at seven, a little mother to the others, and when she died, in her twelfth year, Charlotte took her place.

From Mrs. Gaskell (7) I quote again,

"Charlotte's deep thoughtful spirit appears to have felt almost painfully the tender responsibility which rested upon her with reference to her remaining sisters. She was only eighteen months older than Emily (*sic*) but Emily and Anne were simply comrades and playmates, while Charlotte was motherly friend and guardian to both."

The servants of Haworth Parsonage testify that there was "never such a bright, clever little child as Charlotte, and one had to be very careful what was said before her." Her after life, however, would be proof enough, to one who has studied the neurotic type to which she so fully conforms, that her feeling toward her father and sisters and brothers was precociously developed, that she projected herself into a somewhat grown-up situation albeit in a perfectly childlike way, and that she suffered from strong conflicts ever afterward because this situation was too firmly fixed to melt and flow into the wider forms that should come with growing life. She was little wife and mother, in feeling, and yet she was repulsed by the distant coldness and self-absorption of the chief object of her solicitous love and care. Repression gradually spread its chilling influence, but the fire burned all the stronger underneath.

Then, in her ninth year, the shock of the deaths of Maria and Elizabeth, her elder sisters, came with devastating effect, and part of the horror of this was the physical and mental suffering that they, with herself, had undergone at Cowan Bridge School (the Lowood of *Jane Eyre*). Whether she ever blamed her father for placing them in that school we do not know but that this suffering left its mark no one who has read Jane Eyre doubts. The pathetic deaths of the two older, from tuberculosis, left the third little girl with a narrower circle, and with a sad blank, and threw her back yet more surely upon the attachments yet remaining. Maria had been a substitute for the lost mother, in her childish fashion, and after she was gone there never was another for Charlotte. She lavished her love upon Emily, during her girlhood years, but Emily in turn lavished hers upon Anne, and Charlotte never knew what it was to have her passionate tenderness returned in full measure by any member of her family—and her family was her world. No wonder that in her girlhood, and to the last days of her life she was slow to trust the affection of friends and never dared presume on it. Never was a more loving and closer united family than the Brontë's yet Charlotte was, from the beginning, less loved than loving.

After the mother's death her maiden sister, Miss Elizabeth Branwell, came to Haworth Parsonage to care for the orphaned family. She looked after the house and taught her nieces all seemly female accomplishments but she was never in any sense a mother to them and there is not a jot of evidence that they loved her. It is unanimously conceded by all biographers that she commanded their respect and esteem, and nothing more. The sojourn at gloomy Haworth was a hard penance to her, and moreover, her small and wizened nature could not possibly comprehend the spirits of this nest of young eaglets. Much petty tyranny had the girls to submit to from her—well-meaning soul that she was—and it is not impossible that there was injustice and cruelty also, when they were small. I have seen childless women, who were affable and gentle in all other relations, behave with needless cruelty to little children left in their care, when attempting to discipline them, and I suspect that this is the result of their secret, desperate, sense of their own incompetence for the task, to which must be added their ignorance of the needs and nature of children. Such details of Miss Branwell as have been handed down suggest the possibility of this sort of a situation though I cannot state it as a fact. Certain it is that her rule was irksome and that there was little sympathy between her and her nieces.

We can imagine Charlotte smarting from her injustice and saying to herself that "if Father really knew" he would change all this. Father was still a powerful and somewhat unfamiliar being, benevolent in spite of his stringencies, whose potentialities were not all known. She would fancy herself in a closer, more intimate relation with Father, in such a relation as she saw pictured in the Waverly novels, with no stranger-Aunt imposing unwelcome tasks and delivering unmerited rebukes, a relation in which she should be his housekeeper (no need of Aunt!) and he would be her tender guardian and guide, such a guardian and guide as she made every female character in her books desire, in the later years when she poured her inmost soul into her writing. Miss Branwell, far from introducing an element that could break up the Father-fixation of the little girl, only strengthened it, as did every terrible calamity and shock that overtook her in her life.

Father-fixation

I have not once used the word "sexual" in working out, perhaps with too much elaboration, the relation of the child Charlotte Brontë to her father, because in the generally accepted sense of the word there was nothing sexual in it. But the feeling is so closely allied to that which afterward becomes sexual, it shares so many elements in common with the adult development of sexual love that Freud was logically justified in calling it sexual in his own carefully defined sense. That Charlotte was jealous of the sick mother who took so much of her father's time and thought may have been. That she developed a semi-wifely feeling toward Father (as little orphan girls do) is certain. At any rate *he* was very jealous of any man who approached the grown-up Charlotte, as her own letters to her intimate friends testify. "He was accustomed to disapprove strongly of all marriages" says Mrs. Gaskell (7), and "he could not bear the idea of anyone wanting to marry Charlotte." We have not discussed his side of the complex but this will suffice to show that it existed and had its reaction upon her own complex.

This infantile fixation upon him, strengthened and stiffened by the narrow circumstances of her life and by each succeeding shock, grounded in a quick and sensitive nature, proved an effectual barrier, ever afterward, to normal adjustment to an external world, and normal growth of the deepest emotion of the soul up to new and more serviceable objects.

Her childlike character and appearance in later years bore witness to the partial arrest that so often occurs in the development of precocious children. The "old-fashioned" child becomes a naïve, immature (in some respects), perennially young, adult—a character that has often been noted as belonging to the genius. Her smallness, her shyness, her quaint dependence and quainter independence, her quick impulses, followed by quick retreats, her piteous lack of self-confidence in company, contrasted with her certainty of the rightness of her conceptions in the world of creative imagination, all these and many more traits clearly revealed the child in her. "Even the curates perceived it," says Miss Sinclair (1). It was no small part of her charm. This arrest did not concern her intellect, for that was a giant of early and constant growth, it was purely an arrest of the emotional life.

Chapter II. Her Adolescence

It is in the adolescent period, indefinite as its limits are, that the girl becomes conscious of the feelings and impulses that were present in the child only in germ. In adolescence some childish traits and tendencies become intensified and matured, while others, because of the extensive changes going on in the body, and consequently in the Psyche, are converted and transformed into quite different shapes. The instability of the organism at this time makes almost any outcome possible. All things are held in solution as it were, and the precipitates that come out, sometimes one by one, far apart in time, are not always predictable. This period is at least of equal importance with the period of infancy, for if the trends of a life are rooted in infancy the direction and extent of their growth is determined in adolescence. New features seem to be added at this stage (see Hall's Educational Problems, Vol. 11, p. 39) (9) and a new personality to be evolved from the undistinguished grub that was the child.

In treating of Charlotte Brontë's adolescence the problem of what to include is especially difficult, for she was always adolescent psychologically, never reaching full emotional maturity. On the other hand, experiences and feelings that normally belong to adolescence came to her in the pre-adolescent period, as we have partly seen. In treating of this time when her personality first begins to emerge distinctly I must frequently, then, reach both forward and back, drawing upon mature experiences for illustration of what was transpiring in her adolescent years, and going back to childhood for the beginnings of some things that attained importance now. And just because of this chronological confusion Charlotte Brontë, is, perhaps, a valuable exponent of some of the deeper phenomena of adolescence. She was not a normal girl, either physically or mentally; in her the adolescent traits were either under or over-developed, but this condition of affairs makes for clearness in studying separate traits.

There was not, in this case, the clear line of demarcation between infancy and puberty, as to knowledge and as to the attitude of the outer world that there is in most girls' lives. The knowledge of sex that so frequently comes to girls in the early teens had long been in Charlotte's possession. In that famous incident of the Mask, when Mr. Brontë had each child in turn hide its face behind a mask and then answer the question he put—this being his original method of getting an insight into the minds of his children—Branwell, aged six, upon being asked how best to distinguish the difference in the intellects of men and of women answered, "By considering the difference between them as to their bodies," and the others made answers showing an equal acquaintance with adult knowledge, from Anne, aged four, to Maria, aged ten. This is not surprising, as they spent their time reading classic eighteenth century literature—with no expurgations—and the newspapers, playing and walking together on the moors, or listening to the talk of the servants. And, as noted in the previous chapter, the servants found it necessary to be very careful of what was said before the sharp and clever little Charlotte.

If any sort of sex trauma, such as Freud is accustomed to pre-suppose in the childhood of hysteric cases ever occurred, however, there is no record of it. Her precocious brother Branwell may have been responsible for some harmless indiscretion, or the acquaintance with a certain Haworth family, which acquaintance was discontinued when that family became involved in scandal, may have affected her sensitive mind. There is no necessity for laying stress on either supposition.

In adolescence, with the dawn of self-consciousness and sex-consciousness, the girl is likely to develop a new shyness with her father (8), a new fear of him, and, especially if he be uncomprehending and tactless, as he too often is, to turn to her mother and transfer to her the romantic devotion that formerly belonged more to her father, while the mother had been hitherto a useful person, taken for granted. At this time she may idealize and glorify her mother, not knowing that she is really glorifying Motherhood that instinct that is now stirring newly in her. Sex-consciousness has given birth to shame and fear and the budding girl turns to her own sex, and preferably to her mother, as a refuge. If she fails to find sympathy there, as often enough happens, for mothers do not always remember their own girlhood, she may attach herself violently to some other woman older than herself.

And this attachment, whether to her mother or to another woman, forms a very important part of her development and education. By a normal course of development, by imitation of this mother ideal, she comes to fuller self-knowledge and

theor

to a natural and merely wholesomely restrained interest which, as self-knowledge grows, is toned down from the extravagances of its expression in the first years of adolescence to the womanly norm. The superfluous sex energy and emotion, unutilized in the late-marriage régime of civilized society, is expended in the "frantic friendships" with other girls and with the mother or her substitute, also, in a measure. It is at this time of transference to the mother that the bisexuality, carried over from childhood, and just beginning to pass preponderantly in one direction may become dominatingly either homo- or hetero-sexual. It is now that relations with father and mother, or with brothers and sisters, may tip the scale, already inclined by childhood experience, definitely in favor of homo-sexuality and celibacy, or of normal sexuality. The tendency of childhood may be reversed. If the fear begotten of too close attachment to the father gets the upper hand celibacy may be the result, although the desire for her true birthright burns not less fiercely below the level of full consciousness because it is self-repressed. Both potentialities exist until the end.

After refusing her first proposal of marriage Charlotte Brontë wrote to her most intimate friend, whose brother, it chanced, had made the proposal, "I shall probably be an old maid, but then I made up my mind to that fate when I was twelve years old." By which she meant that at twelve she had begun to fear that she would be an old maid while she hoped that she would not be. The conflict had already begun prematurely between two sexual forces, the one leading forward to normal wedded life the other pushing back to infantile fixation. From this came the changed character of the girl after her twelfth year. The bright, merry, independent little girl described by Haworth servants was gone and in her place was a shy, shrinking, self-distrustful, antiquated little woman, a budding girl already blighted, who was never to fulfill her promise or come to full flowering because the right elements of psychological environment were lacking. Quietism, over-rigid self-discipline, and austerity accompany the changes of adolescence sometimes instead of the exaggerated attention to dress, the giggles, and excitability, that are more often observed, and the former, like the latter, may be a passing phase, the obverse side of the same phenomenon. Charlotte did not grow beyond this phase, she remained adolescent, with the subdued and cramped outer shell of her confining a flame within that burned always with the intensity of adolescence.

It was noted that "she had a desire almost amounting to illness of expressing herself in some way—writing or drawing, (7) (Gaskell, p. 246). It is strange what a yearning the whole family had toward drawing. They lacked the power of execution, not of conception." Charlotte worked much at drawing in her early teens, and we remember that she made *Jane Eyre* proficient with the pencil and brush. Like Leonardo, Thackeray, and her own brother Branwell, she was undecided as to whether the pen or the pencil should be her medium of expression, but some expression there had to be, since the conflict and repression of her inner life prevented the outlets of a normal girlhood. Thus only could she assert herself. Her schoolmates say that she could not say "No," or oppose any one without gathering up all her strength and resolution for the ordeal, yet she never allowed her real thoughts and opinions to be influenced by any one. The line between the outer and the inner personality, showing the duality of her nature, is clearly marked.

There was no mother for Charlotte to turn to in this time of need, and no truly motherly person, not even an elder sister. Her father could not be the recipient of girlish puzzles and questions. He became at once more than ever a stranger, and better known, as girlish reticence and sensitiveness grew, and as understanding of the adult character, with consequent disillusion, developed. Charlotte was turned back upon herself and now for the first time she seeks an outlet in writing. By this means she placed herself on a level with father and mother. The year that she was thirteen saw an enormous output of the little books, still extant in their microscopic script, that she so conscientiously catalogued in 1830 "making in the whole twenty-two volumes," she says in conclusion. These were tales, sketches and poems, with some introductory bits that give a photographic view of life in Haworth Parsonage, and which have been quoted so often in Brontë literature that I shall be content with this mere mention of her literary activity in these years of dawn.

A more normal girl would have kept a diary. Charlotte's self-repression was too great for this. She poured her energy and her outraged feelings into objective forms, refusing to use even her own name. These Juvenilia are almost all prepetrated in the name of "Arthur, Lord Wellesley, Duke of Wellington," her hero. Toward her father her feelings were taking the outward and conscious form of a deep sense of duty. If reverence, duty, and obedience to authority are traced by the Freudians to the Father-ideal in boys, how much more is the parent-fixation potent in girls, whose whole life is so much more confined to the family circle and bound up in it? The partial substitution of duty for love is traceable to the ambivalence of feeling that grew up toward her father as her sex-consciousness brought about a withdrawal from him, and as her imperative need of mothering at this critical time increased her sense of frustration and exclusion. In part this inner experience is to be deduced from her self-depreciation, her excessive shyness, her lack of freedom of expression, at this time, but more from the moral consequences later observable, and in the subject matter of her books. Parenthood is a theme little handled in them, the heroines are invariably orphans obliged to make their own way, often misunderstood and unappreciated. The parents that are depicted, are, with the exception of Mrs. Bretton, in "Villette," brought in with some unpleasant complication of feeling or circumstance that shows the subject to be a painful and difficult one. The influence of her father and of her feeling toward him is shown most clearly of all, however, in her attitude toward her few friends, both when she was in her teens and also later. A quotation from Mrs. Gaskell's Biography may serve to point out this:

"I am not sure whether Mr. Brontë did not consider distrust of others as a part of that knowledge of human nature on which he piqued himself. His precepts to this effect combined with Charlotte's lack of hope made her always fearful of loving too much, of wearying the objects of her affection, and thus she was often trying to restrain her warm feelings and was ever chary of that presence so invariably welcome to her true friends. According to this mode of acting, when she was invited for a month she stayed but a fortnight."

I think we are justified in thinking that it was not from her father's precepts alone but also from his example and his attitude toward herself that she derived this deep rooted fear of asking too much, and of presuming too far, of loving where she could hope for no return. She herself expressed in letters to friends the same sentiment that Mrs. Gaskell has described, and in her first letters to Ellen Nussey, after leaving Roe Head School, at which time she was sixteen, she expresses surprise that Ellen has kept her promise to write, and has still an affection for her. The sense of her own inferiority, the habit of self-depreciation, was not altogether due to her sense of inequality in relation with her father, however, it had other roots as well, but these might not have come to full growth had not this complex given it the impetus.

It is here that her only brother, Branwell, plays a rather important part in strengthening the complex that begins to be truly sexual. Branwell was, next to his father, *the* important member of the family, in whom the family hopes and pride centered. Charlotte's clearest recollection of her mother was of seeing her playing with Branwell in the parlor of an evening. Branwell was given special instruction and care by his father while the girls were left more to their Aunt. Miss Branwell, the aunt, was extremely partial to her one nephew, as is only to be expected. Anne, who copied facts more literally in her books than ever Charlotte did, has given in *The Tenant of Wildfell Hall* (10) a little scene between mother, daughters, and son that reproduces many a scene in Haworth Parsonage between aunt, nieces and nephew, in which the girls are made to give way to the spoiled boy. The mother in the story goes on to say:

"You know, Rose, in all household matters, we have only two things to consider, first, what's proper to be done, and, secondly, what's most agreeable to the gentlemen of the house—anything will do for the ladies."

Apparently Charlotte submitted to this doctrine, and idolized her brother as well, during the first years of adolescence, but her idol was soon broken and little of her love survived the spectacle of her brother's self-degradation.

The policy of the household, with its distinction between male and female tended strongly to increase her self-depreciation, while it fostered a subconscious rebellion that never found full expression but sharpened the conflict of her soul and made her in some ways a forerunner of feminism. It was in adolescence that these things were most keenly felt, in adolescence that her feelings toward her brother were strongest and most clearly ambivalent, her loving pride having its reverse side of resentment and anger. When at Roe Head School, in 1832, she writes to Branwell, "As usual I address my weekly letter to you because to you I find the most to say."

Branwell never doubted his superiority to his sisters nor failed to act upon that assumption, so voluntary subjection to her father was supplemented by subjection to a brother—and both subjections had their obverse side. Branwell's moral defection later was a shock not to be minimized though it had not the importance in determining her literary tendencies that some have attributed to it. It was of secondary importance to a more fundamental complex.

With her sisters her relation grew constantly more dear and intimate. Emily, next to Charlotte in age, was the best beloved. Charlotte has confided to a friend that she loved the masculine traits in Emily especially. "I think a certain harshness in her character only makes me cling to her more." Emily too has her relation to the father-complex in Charlotte through these stern qualities. Emily was unresponsive and incomprehensible, like her father. "I think Emily seems the nearest thing to my heart in the world," Charlotte wrote, in maturer years, when Emily was dying. Another strong motive in her attachment to Emily was the mother feeling, which, as we have seen, developed very early in Charlotte, and, I believe, came to such ripeness in her adolescent years, that it preceded and arrested the growth of normal sexual instinct, forming that complex of emotions which makes childlessness a bitter grief, not less bitter because its true nature is hid from consciousness, and blocks up the only avenue to motherhood by usurping the place which true adult love should fill.

From the dawn of the Christian Era men have exalted motherhood above all else in woman, making the Virgin Mother the supreme ideal, free from every contamination of mere man. In India and other oriental lands there have been Virgin-Mother cults, as well. But what this ideal means to women is a question that has been little investigated. Psychoanalysis has shown that many neurotic women have what may be called the "Madonna Complex," by which they suffer from an intense longing for children and at the same time from as intense a loathing of the sex relation, and of men. This complex is generally showing itself in adolescence and is traceable in part to an early Father-fixation, to the disgust and horror aroused by first knowledge of the true nature of marriage, and in part to the transference in adolescence to the mother, if this remains fixed.

When Charlotte was fifteen her father made a serious and wise effort to counteract the warping effects of Haworth and seclusion by sending her to the pleasant school of Miss Wooler at Roe Head. And after this he encouraged visits from and to her school friends, but it was too late to alter her character. She was always a shy and reluctant stranger among the hearty and healthy girls of the school. She would join in none of their games, for she did not know how to play, and her physical weakness, her short sight, and habit of self-depreciation, did not give her the courage to try to learn. She made two friends here who were her friends for life. She never made friends who were not life long friends, indeed, for she was slow to be won and hard, but very tenacious of what she had won. As we have seen, she was distrustful of friendly advances but she eagerly clung to whatever she loved, with the force of her pent up affections, when once she dared to love. To Ellen Nussey she gave her whole heart, and Mary Taylor had a warm place in her regard also.

She never gushed as many young girls do, but that her friendship had a true homo-sexual element (in a purely psychological sense) is indicated by many expressions in her letters, only one of which will suffice to quote, since it is pregnant with meaning. After Ellen had returned home from a visit to Charlotte the latter wrote, " I do miss my dear bedfellow. *No more calm sleep!"* She always suffered from insomnia. When she had her dear friend beside her she could sleep however. Freud (11) has a case of a little boy who developed hysterical symptoms in order to be allowed again to sleep with his mother. We can not draw too close a parallel here, however, as Charlotte's experience cannot be traced directly to habits of childhood. The transference is more roundabout, from a remoter source.

It was at Roe Head School that the final weight to her conviction of physical inferiority was given. She was always small and " undeveloped " as she herself said. She was stooping and near sighted, and she was beginning to suspect that she was ugly. Children generally do not know that they are plain unless their elders impress it upon them but the adolescent boy and girl begin to question. We may remember how " Emmy Lou,"* when a girl of fourteen, sat down before her glass and wondered if she were pretty. Charlotte suspected that she was plain but was not sure, and had not thought too much about it, till blunt Mary Taylor, at Roe Head, told her that " She was very ugly." Years afterward Mary apologized and Charlotte said, " You did me a great deal of good, Polly." But " Polly " had done her anything but good. Long afterward Miss Brontë made this strange confession to Mrs. Gaskell : " I notice that after a stranger has once looked at my face he is careful not to let his eyes wander to that part of the room again." Nothing could have been farther from the truth. She was singularly attractive in spite of her irregular and ill-proportioned features, for her wonderful eyes, and the fire and glow of her personality diverted attention from any merely physical imperfections. But the effect of all this upon her already shrinking spirit is incalculable, and it greatly augmented the shyness from which she suffered and the difficulty she had in adjusting herself to new surroundings and personalities. A robust and cheerfully insensitive nature would have thrown off these fears as to personal unattractive-

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* Emmy Lou, Her Book and Heart," by George Madden Martin.

ness, though indeed it can do no good to any girl to be told that she is ugly. If she really is so she will find it out.

Charlotte keenly felt her differences from the others. To Ellen she wrote, in the later adolescent years when she was nearing twenty, " If you knew my thoughts and the dreams that absorb me, and the fiery imagination that at times eats me up, and makes me feel society, as it is, wretchedly insipid . . . you would pity and, I dare say, despise me. I have some qualities that make me very miserable, some feelings that you can have no participation in . . . that few, very few, people in the world can at all understand. I don't pride myself on these peculiarities, I strive to conceal and suppress them as much as I can, but they burst out sometimes and then those who see the explosion despise me and I hate myself for days afterwards."

This is true adolescent sensitiveness, full of the self-consciousness of her own unique personality, aggravated by the Brontë family-sensitiveness and her strong family-consciousness which set her apart from the rest of the world. Within the four walls of Haworth Parsonage were people who understood her—and nowhere else in the world. At another time she told Ellen how little she could bear ridicule and how light banter that no one else would care about entered into her very soul and rankled. She tried not to care, and so drove the iron in deeper. There is a complex of reasons for her supersensitiveness and I hardly know to which can be given the priority. There was heredity, there was the early reaction to her father's neglect, there was the strong feeling of physical inferiority, and the consciousness of her unique background, training, and childhood. Girls in their early teens are prone to such sensitiveness; Charlotte Brontë never outgrew it.

The result of her feeling of isolation and inferiority is readily seen in her schooldays. " She was an indefatigable student; constantly reading and learning; with a strong conviction of the necessity and value of education, very unusual in a girl of fifteen." (7) We may seek a parallel to this in the psychoanalytic study of Leonardo da Vinci, who made all knowledge his province. But study alone did not suffice her, as it did not suffice Leonardo. She spent her leisure time in " making out," i. e., in imagining stories, and with these she used to entertain her schoolmates at night. She had done this with her brothers and sisters from the time she could talk, but, as we have seen, the creative faculty received a great forward impulse the year that she was thirteen. She asserted her own worth in the one possible way when she fascinated her schoolmates with her stories or excelled them in the schoolroom. Like all children whose interests in real life are few she compensated for the lack by living in a brilliant world of imagination. As an outlet for repressed longings, and affections she had her imaginary heroes and heroines. Some of these heroes kept their place in her heart for years, but of all of them the Duke of Wellington (not, of course, a fictitious character, save as she invested him with fictitious adventures and traits) was the chief. She worshipped him for thirty years, from the time that she was five years old, or less. She said she admired him most for his self-collectedness, and for his strong sense of duty. The first was a trait which she could never acquire, the second the trait that she made it her chief aim to possess. The Duke of Wellington was the lay figure on whom she projected her own ideal personality, her ideal of a man and mate, and her ideal father-man. He was truly her idol, embodying all that she thought worthy of worship, a father-surrogate, possessing just those qualities that her father conspicuously lacked.

Yet, none of her writing at this time, vivid and imaginative as it is, gives any distinct promise of the genius that flowered as late as her thirtieth year. This is because the passion that makes her novels so true, so fresh, so fiery, was not yet full, and it proves too that her genius was truly the product of emotional rather than of intellectual factors. Her intellect was precocious, but it was long before the supreme moment came in her emotional life that fused and crystallized the scattered and formless impulses of the girl's heart and caused the insupportable pressure of grief and disappointment to force its way out by the channel of fiction-writing.

The account of her adolescence is not complete without mention of hysterical symptoms already showing themselves. Many of these are common in adolescent girls and pass away with maturity. With Charlotte they persisted. She was given to inner speech such as is described in Hall's "Educational Problems" in the chapter on the "Budding girl." (9) She held dialogues with herself in which she really seemed a dual personality. She has put many of these into the mouths of Jane Eyre, Lucy Snowe, and Caroline Helstone, characters in her books. Caroline Helstone speaks of these "senseless ejaculations" which we utter when alone and when thinking of a painful passage in our lives which fills us with mortification and which we would fain forget. These ejaculations are sometimes senseless to the speaker herself, as they come from a train of associations running outside the focus of consciousness. A young girl of my acquaintance used to exclaim "Mother!" when alone, and then stop, startled, because, so far as she knew her Mother was in no way connected with her train of thought. Psychoanalysis easily revealed a mother-complex. There is no record to tell us the substance of Charlotte's inward speech. She did not keep a diary to reveal her thoughts, so we can only guess that this trait was the expression of conflicting trends of personality.

She was superstitious, believing more or less in ghosts and banshees, in portents and presentiments. The wind foretold disaster, as is set forth in the early part of *Villette,* concerning Miss Marchmont's death, and again in the wonderful last chapter. The terror inspired by the moving light described in *Jane Eyre,* in the "Red room" incident was taken literally from an experience of her own at Roe Head school and it is said that her health failed from the time of that shock, and she was finally compelled to go home. Dreams were very significant to her. She took them as prophetic of disaster, and no wonder, for she suffered from frightful dreams both at this time and later, when sickness and death were always threatening. We may recall the dream that Jane Eyre had on the eve of her wedding, of carrying a little wailing child that would not be stilled. This was a recurrent dream of Charlotte's and she thought it an evil portent. It was partly an expression of the repressed motherhood wish but is capable of a deeper interpretation. The child is representative of the dreamer herself, suffering and not to be comforted. It might truly bode disaster, in a different sense from that in which she took it, in the sense, rather, in which our dreams do forecast the dangers of which our subconscious selves, sensitized to currents of feeling, are dimly prescient, while yet the perils are hid from the light of consciousness.

"Oh, my prophetic soul, my Uncle!" cried Hamlet (12) when the ghost revealed the name of his father's murderer. Hamlet's feeling of guilty fellowship for him who murdered his brother for the sake of possessing his brother's wife was the prophet here. Even so Charlotte Brontë felt that the unbelievable disaster might happen—the disaster of betrayal of her one great love—for her father—and therefore she attributes this recurring dream of her own to Jane Eyre on the eve of Jane's wedding—the wedding that was forbidden because the bridegroom had a living wife.

A striking dream of her early adolescence which she recorded in one of her "little books" was that of finding herself in a cave under the ocean, of feeling the terror of the walls heaving and cracking, of the floods about to overwhelm her. Then the scene changed to a desert and a roaring lion rushed toward her while she remained rooted to the spot. The latter half of the dream is typical of the dreams of girls and women.

There are few who do not dream of the pursuit of a lion, a bull, or similar animal, from which they are unable to escape. (4.)

The first part of the dream is more complex and comprehensive. It symbolizes, first of all, her situation in life, hemmed in on every side, unable to gratify her deepest desires, and cramped and held back from any sort of freedom in life by the force used to repress these desires. According to the studies in dream interpretation of C. F. Jung (The Psychology of the Unconscious, Chap. I.) moreover, the dream of the cave and the water would represent the regression into infantile pre-natal life, the first Paradise. The threat of the rupture of the walls would be the threat of birth into a harder, more perilous world. This dream finds many parallels in the dreams and fancies of praecox patients.

As for normal manifestations of interest in the other sex she had little opportunity for such things until she was sixteen, but it never seems to have touched her. She met her schoolmates' brothers and all the curates of Haworth vicinity and none of them interested her, so far as we know. There were two proposals before her twenty-fifth year, one romantic, since it was love at first sight, and one very unromantic, since the gentleman was looking for a suitable matron for his school, but she refused both with exemplary calm. The coquetries of girlhood were quite foreign to her. When we know that she really had strong sexual feeling, when we see the part that the sex theme plays in her novels, we must ask why it did not give itself preliminary exercise in the ordinary way given to young folk and we point, by way of answer, to the family-complex. Segantini (13) with his mother-complex and his one love may be cited as a somewhat parallel case, and also Leonardo da Vinci (14), who never wooed any woman. Exact parallels in the psychoanalytic literature of genius it is difficult to find, because there has been little study devoted to women. With her self-consciousness and her strong self-repression she found it as impossible to join in this social game as to join in the ball games of her school mates. And as for real love, it was given already to the shadowy ideal shaped by the father of her childhood and so firmly was it driven to cling to this form by the experiences of youth, it could never be transferred to any lover that did not call forth identical feelings.

The hysterical symptoms of headaches and back aches, of insomnia, lapses of memory, food-taboos (when at Roe Head she would eat no animal food) nervous terrors, obsessions of gloom, were present in the period from her thirteenth to her twenty-second year, especially toward the last, but they greatly increased with her maturity.

One more point in this important period of her life remains to be touched, and this is her religious experience. She was very reticent on this point, only confiding in Ellen Nussey, when goaded to it by that young woman, who was an Evangelical fanatic during her adolescence. Charlotte felt her own lack of true religious feeling, judged by Ellen's standard, and suffered from the consciousness. But with maturity she regained her poise and took her religion as she took her daily bread. Had the morbid stimulation come from a more influential person than Ellen it might have done her lasting harm, but so far as I can see, the hysteria of religion passed from her with the close of the adolescent period, and never played an important part in her neurosis. She was never either devotee or sceptic. Her nature found other more congenial outlets, and she kept the faith of her father in much her father's fashion. She found consolation and support, and a goal for aspiration, in her religion, but it played a subordinate rôle.

Chapter III. Facing Life

The dual nature of Charlotte Brontë's character, and the ambivalence of her emotional being show most clearly in her mental experiences when, maturity reached, she had to face life for herself, to choose her place, and to determine the field of her activities. Ambition and the desire for independence ran high, thrusting her out into the world where she might mingle with others (and she cherished hopes of overcoming her shyness and being cheerfully social and practically competent) where she might earn her own living, and make a name. This represented the *masculine* side of her, if one chooses to call it so, meaning by the word less a matter of sex than the class of aggressive characteristics generally attributed to the stronger sex. It was the self assertive side, the egoistic, the side that rebelled against restraint.

But, pulling against this restless ambition, making it easy to yield before obstacles, causing her to feel a secret relief when her plans came to naught and any aggressive proceeding or adventure in the world of affairs was rendered impossible, according to her view, was the passive, feminine, timid, self, that could not tear itself loose from the bonds of family life, from the shelter of the little world of home, and from the strong attachment, above all, to her father which was easily the strongest attachment of her life. She never doubted that it was her *duty* to remain with him, however differently others might see the case. It was this self that conquered in the end, and condemned her to a sad and solitary life, for the struggle did not end, and it was never a complete victory. The other ambitious self neither died nor slept. The success of her books brought some relief, but such success could not fully satisfy her. It was impossible that she should ever be satisfied, for the conflict-shaken soul in her demanded disparate rewards. In these years of facing life, when the two selves seemed balanced—of equal strength—occurred the events that gave the final impetus and the final shape to her creative genius.

Her first attempt to earn her own living was in the obvious path of the governess, first at the school of her old teacher Miss Wooler, later in two private families, successively. The former position she left on account of ill-health, and in the others she was neither happy nor well, but she gave up the work for family reasons, and for plans of setting up a school of her own, with her sisters. This plan of a school of their own was cherished for years, and actually attempted. For Charlotte it was a welcome compromise, which would enable her to win independence and yet keep the family life, severed from which she suffered cruelly. The school keeping plan looks absurd to us now, in view of her splendid talents, but we must bear in mind that her talents did not reach their full development until she was thirty, that she herself was not fully convinced of their worth, and that school-teaching seemed the only enterprise possible to women of her class, at that time. So she looked to it as the goal of her fermenting desire to have a life of her own.

The career of a governess could not be successful because of its dependence on others and because of her physical and mental unfitness for such duties. It has been thought that the chief cause of this mental unfitness was her lack of love and sympathy for children. That this was untrue and that it was rather her great love for them, unable to express itself easily because of the painful complex involved, Miss Sinclair has well shown in her book " The Three Brontës." The proof she brings forward is largely that which psychoanalysis would offer; namely, the ability shown by Miss Brontë to win the affection of children when left untrammeled by the inhibitions suffered in contact with other adults, the passionate sympathy with the sufferings of childhood expressed in her works; the pleasure so great that it became a tender and exquisite pain which she describes as Lucy Snowe's feeling in caring for the ailing little child in " Villette," her shyness, which is really a fear born of pain, in approaching them;

and, with significant emphasis, the dreams recorded in her works and in her life.

A young woman who participated in an experiment on word association (15) gave the following protocol of her association with the word *child:*

"I had a feeling that I cannot quite express when I saw the word *child*. A feeling I always have when I see a little child. A feeling as if I should like to cry." This young woman possessed the physical symptoms of hysteria suffered by Charlotte Brontë; psychoanalysis showed a strong attachment to her father in childhood, followed by a conversion into rebellion and resentment in adolescence, when she identified herself with her mother, and accompanied by an intense repugnance to the physical elements of marriage and motherhood.

Another young woman with similar physical symptoms and a similar infantile and adolescent emotional history always experienced a rush of tears to her eyes upon the sight of children in a group, or under any circumstances that emphasized the idea of childhood. No conscious emotion accompanied her tears; they came as a surprise to her and were inexplicable. This began about the time she was seventeen and continued until, in her middle twenties, she submitted to psychoanalysis. After learning the true state of her feeling toward children, and that the painful experience was due to her repressed desire for motherhood, the peculiar reaction ceased. Perhaps these two examples will be sufficient to show on what grounds I base my belief that Charlotte Brontë suffered from a painful complex of feeling with regard to children. Her mother-instinct found an object, but not a satisfying one in the care of her younger sisters. They were too near her own age. The care of other women's children under circumstances that constantly reminded her that her right in them was small necessarily gave her more pain than pleasure.

It is just here that her experience as a governess is important for her inner history, for in this experience she suffered shocks that served to strengthen the complex and to deepen the repression in which she held this well developed fundamental instinct of her nature. She suffered shocks from the children themselves whose unregenerate behavior damaged the tender ideal of childhood her inmost mind had cherished. When the youngest little Sidgwick threw a Bible at her one such shock was received. When the same little fellow impulsively caught her hand and said, "I love 'oo, Miss Brontë," and his mother exclaimed, horrified, "Love the governess, my dear!" a deeper shock than any was given. For it was not merely the wound to her pride and self respect, it was the driving in of the realization that she could have no real part in the children for whom she cared, and that she was cut off from the realm where she most longed to enter. Small as the incident was, it acted, with other conditions of her unwelcome servitude, to limit her still further in her emotional scope, to cut off one more possible avenue of self-expansion and of wholesome sublimation of the great biological instincts. It drove her back to the more infantile forms of love, denying to her anew the natural and legitimate means by which a woman's ego transforms itself in the service of others.

Just what the resulting complex of feeling was has been so well described by Miss Sinclair that I shall take the liberty of quoting her phrases in part, not only because I cannot better the description but also in order to defend myself in advance, by this frank quotation, from the charge of plagiarism. While I do not agree with her that "the love of children was the key to Charlotte's nature, the heart of the mystery that was Charlotte Brontë—for I take it to be only one of the keys, I cannot do better than let her speak for me in describing the actual content of motherly feeling in Charlotte.

"She was afraid of children, awkward with them. Such passion has shyness . . . even its perversions, when love hardly knows itself from hate. Such love demands before all things possession. It cries out for children of its own blood and flesh. I believe that there were moments when it was pain for Charlotte to see children borne and possessed by other women. She hid her secret even from herself, as women hide these things. But her dreams betrayed her, after the way of dreams. All her life, I think, she suffered because of the perpetual insurgence of this secret, impassioned, maternal longing." (The Three Brontë's, by May Sinclair, pp. 63-66.)

We have seen how in Charlotte this passion was prematurely roused, and how the intertwined sex-complex or father-daughter-complex worked to make its gratification impossible, and it naturally follows that the secret, unrealized knowledge that it must inevitably be so balked imparted to it a content of well-nigh unendurable pain. The result of this accompaniment of the thought of mothers—and children—is seen in the subject matter of her books, in Jane Eyre, where it works out as a gloomy and harsh characterization of the mother, and in Shirley, where she makes the much-suffering Mrs. Pryor fear to claim her daughter, because of the new suffering she might bring to her—and never in literature, I think, has there been a more subtle analysis of maternal feeling in its less obvious phases. It is a dramatization of her own inward struggle between love and hate, desire for, and fear of, the most precious possession of living beings. In the end, according to the story, love conquers, and so, also, had there been time enough, the resistance imposed by fear and deprivation might have conquered for Charlotte. But she died just before she was to become a mother, and the illness that accompanied her whole period of pregnancy had a psychogenic element, as if, indeed, her soul as well as her body were rent by a conflict between life and death.

The next significant event is the going to Brussels for further education. This has not the great significance attached to it by Shorter (16), Reid, (17) and others, who hang her whole literary career upon it, but it has significance as expressing a phase of the complex of feeling with which she faced life. The desire to go to Brussels came as an irresistible longing to get into larger places, to see, to know, to feel life in larger aspects. It was the legitimate expression of the expanding ego. But from another aspect it was a putting off of the actual responsibility entailed in gripping life at close quarters. It meant a prolonging of the novitiate, always welcome to passive characters, who eagerly seize the excuse of the necessity of learning more before they begin to do. Their fear of actual life is such that they dread the taking of an active part even while they long for it and dream of achievement. Such was Charlotte Brontë's unconscious attitude, while her conscious rationalization of her action was the perceived necessity of knowing French and German before she could hope to get pupils for her school. She was twenty-six when she went to Brussels, and she revealed her feelings about her new departure frankly to her old friend Ellen:

"I was twenty-six years old a week or two since, and at this ripe time of life I am a school girl and on the whole very happy in that capacity. It felt very strange at first to submit to authority instead of exercising it, to obey orders instead of giving them; but I like that state of things. I returned to it with the same avidity that a cow that has long been kept on dry hay returns to fresh grass. Don't laugh at my simile. It is natural to me to submit and only unnatural to command."

She reveals more of herself than even she knows, and her simile is a telling one. The pleasant pastures of childhood were far more grateful to her than the roaring crowded streets of adult life. She found it hard to finish *growing up,* in plain words, and welcomed this temporary return to childhood and tutelage. She liked to be ruled better than to rule, although she liked to be forced by circumstances or by the right person into submission; she could not openly choose it, as the more advanced and conscious part of her rebelled. Each of her heroines in turn, Jane Eyre, Shirley, Lucy, and Frances, illustrates this twofold attitude. The ultimate, unhampered, choices we make, in almost any situation, are sure to be those which most satisfy the demands of our buried, our subconscious selves, as Freud has well pointed out in his, *Psychopathology of Every Day Life* (18). Charlotte Brontë exemplified this principle in her going to Brussels and in her subsequent decision to remain at home in order to minister to the needs of her family.

At Brussels she came under the influence of Monsieur Heger, principal of the school at which she studied, and to his influence upon the development of her intellectual powers and upon her emotional life as well, many writers have not hesitated to attribute her success as a writer. She studied French literature and composition under him, and the devoirs prepared by her at that time, as well as more spontaneous writings have been preserved. Reference to them will dispose satisfactorily of the claim that his teaching greatly influenced her style of literary form. The school exercises are school exercises—nothing more. They are brilliant as such but give no more indication of genius than did the numerous stories and poems written before this time. The question of his influence upon her emotional life is not so easily disposed of. Until three or four letters of hers that were given to the world by Monsieur Heger's son and published in the London Times (19) in 1913 the debate as to whether or not Charlotte Brontë was actually in love with Monsieur Heger was warm among Brontë enthusiasts. As it was the only possible grand passion in her life Clement Shorter, Sir Wemyss Reid and others of less note affirmed it and based upon it the emotional outbursts in her novels, which we are now tracing to deeper sources, while not disregarding this one. Others (notably the women), Miss Sinclair, and Mrs. Gaskell, stoutly denied it, maintaining that Charlotte cared for her teacher, a married man, in the innocent way that an admiring and devoted pupil does care for a favorite teacher in such cases. This sort of mingled reverence and adoration, coupled with a mischievous delight in the teacher's shortcomings, which Charlotte certainly evinced, does resemble a real adult love "as the mist resembles the rain." It is a faint adumbration of the real love, founded, like that, on the father-fixation and it fades away and leaves no scar behind when adult life really begins. That this was the true state of affairs between Charlotte and her teacher, whose favorite pupil she was, I believed until those letters, above mentioned, came to my attention. They are of far too private a nature to have been given to the public, which is the reason why they have been held back so long. They show that Charlotte Brontë really loved M. Heger with a truly adult passion. They show also, however, when taken in connection with the other letters written at the time, that she was never full conscious of the nature of her feeling. M. Heger was a married man and this was enough to censor the truth from the page of her consciousness. But she suffered keenly, from the time when in her second year at Brussels she began to feel it most strongly until several years later, when all communication with him was broken off, and when the success of her books brought wider interests.

To any who have read Mrs. Gaskell's Biography, or Brontë's novel *Villette,* the hero of which is a spiritualized and glorified M. Heger, the relation of this love to the previous emotional history of Charlotte Brontë is obvious. M. Heger answered every requirement of the father fixation. Like her father, he was arbitrary, passionate, severe, yet often kind. He gave his pupil that flattering share of his interest that made his comings and goings, his praise and blame, the emotional interchange between them the one excitement, the one experience that lent color to her days, that at the outset supplied an emotional tension sufficient to keep life from going on too tamely and flatly. It was not until the second year, after her sister Emily had gone home, leaving her alone, that the attachment took possession of her as a true passion. Emily had kept open the bridge to the childhood home, had in some sort represented the compromise that she was continually trying to make between the old demands and the new. With Emily gone the new attachment grew stronger and stronger, while all the powerful resistances inherent in the situation created a conflict quite severe enough to account for the depths of misery that are depicted in her letters of this time. It is significant that no attachment whatever was formed to Madame Heger. Between Charlotte and this lady existed a growing dislike, more aggressive on the Madame's side as she suspected, before either of the principals did so, the feeling of the pupil for her husband.

Toward the close of this year of conflict there came news of conditions at home, at Haworth Parsonage that were welcomed as a call of duty, a legitimate excuse for return thither.

Her father was losing his sight, from cataract, and, under the pressure of grief over his son's disgrace, was taking to drink as well. Emily was at home, while Anne, the other sister, was governess in a distant city, the aunt had died the year before. Only Charlotte could rescue her father and, racked by a new conflict, she determined to go. The parting with her teacher held a bitterness that she made no attempt to disguise, even though she did not realize its depth. Yet there was still a hope for her, she felt, in the work she planned to do. The return home was but an interlude, she had no thought at this time that daughterly and sisterly duties were to monopolize her. She was happy to be at home again in this year of 1844, her twenty-ninth year, the crisis had not yet come and she did not realize the inner conflict.

It is just at this point that too much has been made of the attachment to Heger—a hopeless and unacknowledged attachment to her, with her standard of morality—by those who have accepted it from the first. It was not this passion only that she poured out in her romantic fiction, it was not this store of repressed feeling alone that forced her to write.

In all the struggles between opposing desires that had beset her from childhood up to this, her twenty-ninth year, there had always been the hope of successful and satisfactory solution in the future. In all her reluctances and retreats at the threshold of life, through all her tardiness in entering upon full possession of her womanhood she had held the bright indefinite hope of youth that life held something fair and lovely, and the fulfilling of all heart's desire for her. All that had gone over her was preliminary, she had not yet begun to live. The loss of hope did not come upon her return home, but later, when the situation there forbade her carrying out her plans for work. Soon after this another avenue of expression, was closed to her. This was the cessation of her correspondence with M. Heger, in Brussels. The very fact that much of the nature of her feeling for her teacher remained unconscious to herself made its consequences more significant in her life.

So far as we can determine the correspondence with M. Heger, which had been her great comfort and stay, providing as it did a human outlet for her affections most satisfying to her needs, came to an end definitely sometime in 1846. The last published letter (19) was written November 18, 1845. She did not then accept the fact that she must write no more. Mrs. Gaskell has stated that the letters were discontinued because the teacher requested Miss Brontë to send them to a secret address, in order to escape the censure of his wife. The notes and comments that appeared in the London Times in 1913 (20) following the publication of the letters, tend to deny that such a request was made. It is scarcely possible at this date to determine the truth, and for the purpose of analyzing the affective life of Charlotte Brontë it is scarcely necessary. What we do know is that the letters meant much to her, and that their cessation was a blow severe enough to precipitate a conflict which ended in a new and complete repression of her feelings. How much the writing of these letters meant to her is sufficiently indicated in the following passage from the letter written November 18, 1845:

"To forbid me to write to you, to refuse to answer me, would be to tear from me my only joy on earth, to deprive me of my last privilege—a privilege I never shall consent willingly to surrender. So long as I believe you are pleased with me, so long as I have hope of receiving news from you, I can be at rest and not too sad."

In her books she found again a channel for the outpouring of the feeling that had gone out, under some restraint to M. Heger. That the final realization that she was cut off from her dearest friend was the sting that drove her to the passionate expression of spirit found in *Jane Eyre* seems to me a plausible theory. I do not see any way, at present of confirming this by the fixing of the dates of the writing of *The Professor,* of *Jane Eyre* and of her final renunciation of communication with her friend. *The Professor* was offered for publication in 1846, *Jane Eyre* was written that same year, begun while she was in Manchester with her father, who was there to undergo an operation for cataract. I cannot think otherwise than that an emotional change in the author is the only probable way of accounting for the contrast between the fiery romantic atmosphere of the one and the cold and colorless story of the other.

Had she admitted to her own mind the full realization of her deep love for her teacher, the faulty but lovable master whose personality most completely satisfied the demands of her ideal, had she attempted to fight it in the open, probably she could not have depicted her own love story with such moving power and fidelity to the essence of womanly feeling as she did. Facing the problem and dealing with it frankly would have set free the emotional energy involved in conflict in some more common-place and direct way, as the results of Freudian psychoanalysis demonstrate. It is because a large part of her conflict remained sub-conscious that it could find a perfect and uncensored expression in creative, imaginative, writing. The continuity of this love experience with the infantile love-fixation upon her father is shown in the treatment of her child-heroines. The two experiences were of one mould, her adult passion only the result of that close psychic union with her father too firmly knitted in childhood ever to be broken. That the unrequited passion for M. Heger was the necessary agent for bringing her to the necessity of artistic expression as an outlet for otherwise hopelessly repressed feeling there is no need to assume. That such a passion was ncessary to the development of her emotional power is also an untenable assumption. The essential elements were all present, this passion happened to be the precipitating agent, but out of her original Father Complex her need for emotional expression would have grown without the aid of any new love experience. Had she not hit upon a mode of expression which suited her capacities as writing did, she would not improbably have met the mental shipwreck seen in dementia praecox cases of similar antecedents.

Through most of the year 1844 she was trying to start a school there at Haworth Parsonage. She knew she could not leave home at present but she hoped to get pupils to come to her and her sisters there, with the thought, perhaps, that the future might permit removal to a more favorable location. Before there were definite prospects of securing any, however, Branwell's disgrace, through misconduct at the house where he was employed, had brought him home, and between remorse and weakness he had become a drunkard and an opium eater, making life in the parsonage a night-mare, and the reception of pupils impossible. The plan whereby a start in life and independence were to have been won crumbled to ashes. Her father's blindness kept her at his side, or so she felt, and Branwell's degradation made it impossible to carry out her plan at home. At last the choice was forced upon her and in anguish of spirit she made it. It was the only choice possible to her nature, it was the choice that naturally grew out of her spiritual development but it cost a terrible price, the price of seeing the hope of youth go out finally; of seeing her life bounded forever by the grey walls of Haworth Parsonage, with no prospect of independent being and, it seemed, no prospect of the larger life offered by marriage, which she had not really consciously given up, for who would ever seek her at Haworth? Even had new vistas not opened for her at Brussels, it could not have satisfied her to remain at Haworth.

The life in Brussels with the growth of a new passion had almost converted her from an emotional, pent-up, but powerful dreamer of dreams into an active work-a-day woman. Hear what she writes in the interval before her second year there, in 1843. "There are times now when it appears to me as if all my ideas and feelings, except a few friendships and affections, are changed from what they used to me; something in me, which used to be enthusiasm, is tamed down and broken. I have fewer illusions; what I wish for is active exertion—a stake in life. Haworth seems such a lonely, quiet, spot, buried away from the world. I no longer regard myself as young—indeed I shall soon be twenty-eight and it seems as if I ought to be working and having the rough realities of the world, as other people do. It is, however, my duty to restrain this feeling at present, and I will endeavor to do so."

She had not then given up hope. Had she been able to realize the desires that then filled her, however, we should never have had *Jane Eyre,* though we might have had books from her pen, books, which if we may judge by what she wrote before this critical year of 1844 would not have lived for half a century.

By the first of the year of 1845 she had made her decision, for her friend Mary Taylor, who went to New Zealand soon after, wrote of her, to her biographer (7):

"When I last saw Charlotte (in January, 1845), she told me she had quite decided to stay at home. She owned she did not like it. Her health was weak. She said she should like any change at first, as she had liked Brussels at first, and she thought there must be some possibility for some people of having a life of more variety and more communion with human kind, but she saw none for her. I told her very warmly that she ought not to stay at home, that to spend the next five years at home, in solitude and weak health, would ruin her; that she never would recover it. Such a dark shadow came over her face when I said 'Think of what you'll be five years hence!' that I stopped and said:

"'Don't cry, Charlotte!'

"She did not cry, but went on walking up and down the room, and said in a little while: 'But I intend to stay, Polly.'"

To clear sighted Mary Taylor, with a mind fifty years ahead of her time on the "woman question," Charlotte's sacrifice always seemed wrong.

After this time Charlotte herself wrote to a friend, "All the days are alike, heavy and lifeless. Undoubtedly my duty directs me to remain at home. . . . There was a time when Haworth was a pleasant place to me—it is not so now. . . . I long to travel, to work, to live a life of action. . . ."

Yet it must not be forgotten that her disappointment had its reverse side of relief for her shrinking soul, which feared activity as much as it craved it. Many, if not all, of our bitterest disappointments bear with them an unacknowledged relief from some dreaded or ill-carried responsibility.

With the vanishing of the School bubble the three sisters turned more seriously than before to writing as an outlet for their restless minds and a means of supplementing their scanty living. No doubt the work of each was a stimulus to the others, but to Charlotte, most famous of the three, full power had not yet come. The *Poems,* published in 1846, and *The Professor,* written mostly in this same year, while possessing merit, are very far separated from the fire and intensity that characterized *Jane Eyre.* To explain this disparity in works produced so near together in time Miss Sinclair has suggested that the reading of Emily Brontë's *Wuthering Heights* may have awakened her to the necessity of pouring her fiery spirit unrestrained into *Jane Eyre.* Such an explanation is plausible but appears inadequate. *Jane Eyre* gives the impression not of conscious effort, but of being the resistless outpouring of feeling and imagination that could not be restrained.

That she happily chose a more suitable medium of expression in *Jane Eyre,* a more direct channel for liberation of her spirit than the poems or the story of *The Professor* furnished her would account for a small part of the difference. In *The Professor* she attempted to project her personality upon a masculine personality, and was unable to perform this operation successfully because she could not sufficiently detach from herself the qualities and emotions she tried to depict. She was cramped by the artificiality of her creation. In her later successful books she drew only the characters she really knew—chiefly herself, her father, or M. Heger, and Emily—and with these she succeeded.

But the chief cause of the power that developed in her after the writing of the *Professor* and before *Jane Eyre* is to be sought in events transpiring in the Author's inner world, not in externals. The fresh conflict that rent her, demanding a repression that was almost beyond her strength, arose when she gave up finally the friendship with M. Heger, the man whom she truly loved. The pent-up emotional energy which was denied outlet here must necessarily find other outlet, or destroy the unity of the personality to which it belonged. It is this emotion, this energy of love and life, the mating and reproductive urge, brought to its maturity by the experiences the woman had just passed through, that was transmitted to her work, with the result, to quote a contemporary critic, that *Jane Eyre* seems " written in fire."

Thus, after a long period of preparation, a period lasting till her thirty-first year, she found her full power through the blocking up of all normal paths to satisfaction and happiness. That genius flowers only under such conditions no one can yet postulate, but that the genius of Charlotte Brontë depended upon the turning of the energy engendered by emotional conflicts into the one channel of writing seems to me to be demonstrated. She was precocious, brilliant, she practised her art from early childhood, she had the best English and French literature for her models, yet she failed to bring forth anything worth while until the completion of that repressive process just described. And it is not in the externals of style and plot that she wins praise, as has been often pointed out, but in her truth to inner, emotional, or spiritual experience. Such power rose not from the intellect but from the deeper, fundamental, instinctive nature, the portion of the personality that has most of its activity confined to what we call " The Unconscious."

Chapter IV. The End of the Battle

The story of Charlotte Brontë has been told and retold by many writers. From them we know of her unchildlike childhood, of her pinched youth, of her self-sacrificing womanhood. We have the picture of the delicate, small, shrinking creature, free and alive only in the small circle of her home, and on the wide, wind-swept moors; we can see her sitting desolate in the lonely home, after the last of her sisters have gone; we have a clear impression of her suffering in mind and body, and we wonder yet over the mystery of her genius. But one part of her story has been told only briefly and hastily by most writers, as if it held a problem on which they could not speculate. This is the story of her marriage and her death. I do not intend to say much of them here, only to try to show that both were in keeping with all that had gone before.

The pain of unsatisfied longings, of desires, chained by the triumph of that which she called Duty, but not quiescent, kept her in a state of weak health through most of her life. She was never strong but there is no doubt that happy surroundings and a normal development would have removed much of her suffering, which was almost wholly of a nervous sort. The sleeplessness, headaches, aching spots in the spine, pathological fatigue, great depression, debility, nervous fears and tremors, all are characteristic of the neurasthenic or the hysterical constitution. Her special suffering under any circumstances that tended to rouse the buried wishes—as, for example, her sick headaches and nervous excitement when obliged to meet company in London, all tell strongly in favor of the view that she suffered from hysterical conversions due to emotional conflicts. She was sometimes temporarily better, but never was she well. How she and her father reacted upon each other, since he too suffered from nervous disorders and especially from depression may be guessed. It was an unfortunate fate that bound two such characters together. Had she suffered less physically she might have produced more than the four books that bear her name as author, but had she suffered less mentally she might have produced none. If something could have occurred to break her life-long bondage to her father both sufferings might have been mitigated—had the release come in time.

Her unconscious clinging to him in a way that permitted no rival, and her conscious sense of duty caused her to reject every possible avenue of escape, including that of marriage, until, when she was thirty-eight, having refused four offers of marriage, she finally accepted Arthur Nicholls, whom she had once refused. Why did she do so? She would not, so long as her father maintained his violent and selfish opposition. He came first with her always. Did it not do violence to the inner ideal to which she had so long clung to give herself at last to a man whom she assuredly did not love in any such wholehearted fashion as that in which she had required the heroines of her books to love? No, I think not.

The long faithfulness of Mr. Nicholls and the grief which he could not conceal when she rejected him, worked greatly upon her mother-instinct which is probably always pretty largely present when a woman of her age marries for love. She was not putting him in the place of her ideal but in the place of the son she ought to have had—though she did this unconsciously. Then she felt the need always of protection and guidance, and her father was failing her. He was seventy-six years old, and she was facing the time when she would be left alone. She felt justified in seeking some one to whom to cling, for, while she could bear company less and less as she grew older, she could bear complete solitude not at all. She married Mr. Nicholls in the shy, reluctant spirit with which she had faced life, and he was her last great compromise.

She was not unmindful of her father's claims in this matter. Mr. Nicholls was her father's curate, and had been for years. It was difficult for Mr. Brontë to find another with whom he could work and he was becoming too feeble to attend to his parish duties. By marrying this man she saved to her father his parish and his home—and it would have broken his heart to leave either. Her marriage was not a step forward, it was really a regression, a seeking for that safety, that protection, and control, which the spiritual habit of childhood made necessary, and seeking these in another, stronger personality. I do not believe that she could, at this time of life, have made a marriage that would have taken her from Haworth, radically changed her course of life. She was able to go on as before, the housekeeper of the Parsonage, the comfort and stay of her father, who still treated her as a child. Only one great change it made, it put a stop to her writing, for the time at least. Her husband demanded all the time, the thought, and the care that had once gone to that. She felt the new limitation and if her married life had lasted long it would not have been happy, for her soul was too firmly fixed now for new objects to satisfy it. It had never got away from the moorings of childhood, and in contact with the adult world, with the partial and unequal growth of her personality, conflict between old desires and new was inevitable. The conflict had issued in creative work, without, however, giving peace or true satisfaction to the mind that produced this work. That such a marriage as hers could have given these is impossible. It was too late.

Death seemed the fitting solution of the struggle, piteous and pathetic as her death was. It would be but mysticism unsupported by any scientific facts to trace her death to the mental conflict involved in trying to bring forth life—with all the resistances of her most powerful complexes against it—in our present lack of knowledge as to the connection of the neurological and the psychological. She died in the eighth month of pregnancy, having suffered much from nausea, vomiting and prostration. The data recorded in the biographies are of course insufficient for a diagnosis. Pernicious vomiting of the neurotic variety may be surmised to have been the illness from which she suffered during the early months of pregnancy, though nothing can now be stated with certainty. This complaint always has psychogenic features (21, p. 550). The cause of the fatal termination of her condition, has not, so far as I know, been recorded for us. Without under-rating the true organic factors in her illness we may assume with certainty that her condition was aggravated by psychogenic reactions, derived, probably, from the fear and reluctance she felt at this new facing of life. She, whose life was so strongly bound to him who gave her life, could not become a mother without a destroying conflict. She died, and, whatever the natural causes of her death, her state of mind was one of fear and weariness unsurpassed by anything she had ever felt. It would almost seem that there was remorse as an element of the secret unwillingness she felt to become a mother, as well as the resistance engendered by years of repression.

Chapter V. The Revelations in the Novels, especially in "Villette"

If we did not possess the biographical data so carefully gathered by Mrs. Gaskell, Sir Wemyss Reid, Clement Shorter, Augustine Birrell (22), and others who have interested themselves in the history of Charlotte Brontë, and if we had not the hundreds of letters preserved by her friend Ellen Nussey, we should still have sufficient material upon which to construct the leading motives of this psychoanalytic study in the four novels that she has left behind her. While it is true that none of these is strictly autobiographical, while, none, perhaps, portrays the external events of her life so faithfully as *David Copperfield* tells the story of the youth of Charles Dickens, yet everyone of them is constructed out of the inner experience of the writer. Everyone is a spiritual autobiography, representing different phases of the Author's spiritual history.

Jane Eyre was the result of the first real liberation of the repressed love and longing, after the partial satisfaction of the friendship of her beloved teacher was taken away from her. It is exuberant with the unpruned fancies of a girl's mind. *Shirley,* the least subjective of her books, more mature, written under somewhat happier circumstances, presents no character that is distinctly Charlotte herself though her heroes are again attempts at presenting the father-teacher-master; somewhat more subdued and less improbable than Rochester. The book represents, in its first two-thirds, the broadest and most varied aspects of her personality, indicating her capacity to treat many external aspects of life sympathetically, with more of truth and realism than appeared in *Jane Eyre*. The writing of this book was interrupted by the death of her brother and of her two sisters, all three falling within a space of eight months, during which time she could do no writing. When she took it up again the broad touch was gone and once more the one absorbing interest, the relation between a masterful and benevolently despotic man and a spirited, rebellious, but eventually yielding and submissive woman, dominated the story. "The book quivers with the shock," May Sinclair has said in speaking of the effect of the overwhelming grief of her triple loss upon the writer of *Shirley*. The effect is seen not in lessened power—for the emotional power, which is always her chief excellence, is heightened, as various critics have noted—but in the narrowing of the scope of the story. This reflects the psychological experience of Charlotte Brontë, for she was driven once more back upon herself and upon the eternal complex and conflict of her life. Only her father was left for her to live for.

But it is in *Villette* that we see the final expression of her personality. It is not too much to say that an adequate psychoanalysis of Charlotte Brantë's character could be based upon *Villette* alone. It would lack completeness but no essential characteristic would be lacking. True, the book follows actual external events only in the less important details of the story, but that *Lucy Snowe,* the heroine of *Villette,* is the spiritual portrait of Charlotte no one has doubted. *Lucy* is drawn from the life and while she is not the whole of Charlotte she is the core of her in a way that *Jane Eyre* was not. *Caroline* and *Shirley* have each some of her thoughts and feelings, they are complementary characters, but their originals were Emily Brontë and Ellen Nussey. In *Lucy* we find traits of personality, records of personal experience, without which, with all the letters and reminiscences that friends of the Brontë's have preserved, we could not thoroughly know the author.

Each of her three great books, *Shirley, Jane Eyre,* and *Villette,* is claimed by some eminent admirer as her masterpiece, but I think that those who have studied all her work most thoroughly and comprehendingly unite in giving the palm to *Villette*. Swinburne and Lafcadio Hearn stand as exceptions, both declaring for *Jane Eyre*. But Swinburne, despite his glowing appreciation of her genius, and her praise of her that stands almost alone in nineteenth century criticism, has shown his inability fully to comprehend the object of his reverent admiration on some points where the womanly understanding of May Sinclair and Mrs. Humphrey Ward was keener. These two hail *Villette* as the acme of Charlotte Brontë's artistic achievement. And Lafcadio Hearn wavered in his choice as may be seen from a passage in his *Interpretations of Literature*. (23) (Vol. 1, p. 250.)

The reason for the pre-eminence of *Villette* lies at our hand. The working of the subconscious self is at its best and fullest in this book. Alone with her father, stricken with grief, living upon memories too poignant to be sweet, she wrote, and the writing, refuge from life though it was, was fraught with difficulty. She tells her publishers that this book more than the others, was dependent upon the capricious and elusive "Inspiration" upon which she had always to wait. There were days, weeks, months, even, in which, although hand and brain were no longer filled to overflowing with homely tasks, she could not write a word. At such times her physical condition was wretched in the extreme. She was suffering from nervous weakness, depression, headaches, insomnia, fatigue, all the symptoms of her type of hysterical neurosis, which show the virulence of the inner conflict from which these symptoms arise. The ability to write indicated a temporary victory, or a temporary truce.

In both *Villette* and *The Professor* she makes good use of the abnormal characters, which with the keenness for detecting unusual mental states belonging to the neurotic person, she learned to know at the Brussells School. That school evidently harbored an unusually large number of psychopathic pupils, in a day when special schools for such children were rare. The character of Madame Heger, if she is faithfully portrayed as "Madame Beck" in *Villette,* was one suited to attract and to manage such children. Her methods were based on silent surveillance, cunning, urbanity, unfailing good humor and gentleness, firmness, punctilious orderliness; expediency rather than sentiment governed her conduct. The unemotional exterior of this woman fascinated and repelled Charlotte Brontë, leading her to see in the powerful but treacherous character of the older woman the personality for the rival to her favorite heroine, Lucy. It was a character that corresponded to that attributed by infantile jealousy at times to the mother.

The most perfect portrayal of the neuropathic character, however, is, naturally, found in the study of *Lucy Snowe,* the replica of the author herself. English literature scarcely contains another such masterly, detailed, and consistent description of the neurotic character. No observer, who has not known the suffering of the neurotic, could so accurately describe this type of personality. Lucy has the impassive exterior that often deceives the casual observer, while yet her emotional reactions are exaggerated out of all proportion to their apparent causes. She suffered from sheer loneliness of spirit and from frustration of normal wishes, unable, with her unevenly developed personality, to adapt herself to the society about her. She preferred solitude to the society of those upon whom she was dependent, yet when actual solitude came, in that memorable episode of the "Long Vacation," she was unable to stand alone and her nervous disease reached its height. I must send my reader to the book itself for an adequate conception of this powerful description of agony of mind. "The despair of emptiness," "The utter weariness of life," the "Longing for life to end," are some of the eloquent phrases that show the bitterness and helplessness of the soul out of harmony with life and too fearful of death to risk putting an end to the conflict.

A characteristic symptom, which is almost an automatism, expressing the blind instinct to seek and find Life, is the fever for *walking* that came upon Lucy at this time of solitude. "A goad thrust me on, a fever forbade me to rest, a want of companionship maintained in my soul the cravings of a most deadly famine," we read. Fearing illness, as a result of her growing tendency to fantasy, she soon became really ill, and could neither eat nor sleep and lay down neglected in the echoing dormitory. As the description of what passed now is a page of the author's own life I must quote it, for it sounds the depths.

"Sleep came, but in anger. Impatient of my importunity she brought with her an avenging dream. By the clock of St. Jean Baptiste that dream remained scarce fifteen minutes—a brief space, but sufficing to wring my whole frame with unknown anguish; to confer a nameless experience that had the hue, the mien, the terror, the very tone of a visitation from eternity. Between twelve and one that night a cup was forced to my lips, black, strong, strange, drawn from no well, but filled up seething from a bottomless and boundless sea. Suffering, brewed in temporal or calculable measure, and mixed for mortal lips, tastes not as this suffering tasted. Having drunk and woke, I thought all was over; the end come and past by. Trembling fearfully—as consciousness returned—ready to cry out on some fellow creature to help me, only that I knew no fellow creature was near enough to catch the wild summons—I rose on my knees in bed. Some fearful hours went over me; indescribably was I torn, racked, and oppressed in mind. Amidst the horrors of that dream I think the worst lay here. Methought the well-loved dead, who had loved *me* well in life, met me elsewhere, alienated; galled was my inmost spirit with an unutterable sense of despair about the future. Motive there was none why I should try to recover or wish to live; and yet quite unendurable was the pitiless and haughty voice in which Death challenged me to engage his unknown terrors. When I tried to pray I could only utter these words:

"'From my youth up Thy terrors have I suffered with a troubled mind.'"

This was a chapter of her own experience, portraying the very acme of the horrors felt by a soul in bitter conflict with its unknown forces. The symbols of the sea, the cup, with their association of wild terror, the intuition that here was something beyond the bounds of the human or the mortal, joined with the bitterness of separation from her family in that other, lower world, make a dream strongly resembling the terrifying fantasies of patients suffering from dementia praecox. The foundation of such fantasies is the age-old, deeply buried incest conflict. The desire that had its roots in earliest life finds itself opposed by all the upward-striving impulses of the race, it sees itself subversive of biological progress, and the recoil from it, in dream or fantasy, carries with it the deepest of horror, the horror felt for that which destroys Life. In her loneliness, her desolation, the buried, unconscious, primordial desire stirred in *Lucy,* and brought with it the " avenging dream."

The agony of this attack culminated, and ended, in the famous Confession. Lucy—alias Charlotte—rose from her bed, dressed herself, weak and shaking and went out, in one last half conscious struggle to get in touch with life again and to regain her hold upon the world of reality. After a long walk she went into a church to rest, and moved by an irresistible impulse to seek human intercourse and sympathy, went up to the confessional and poured the story of her agony into the ears of the priest. She had no sin to confess—not even the stain of another's sin, like Hawthorne's " Hilda "—it was simply the demand for expression of her pent-up feelings, a demand that almost swept away her Protestant prejudices. She received a measure of help and comfort, but strongly reacted against the hated " Popery " immediately after. The real reaction was probably against her own weakness in breaking her silence and seeking help, a yielding to " Paternalism." This incident resulted in the finding of friends, and release from her solitary confinement, and new scenes and interests relieved the suffering and brought her back to her normal state.

The great contribution of *Villette* to psychology, and the greatest contribution found in any of her books is the revelation of the Father-complex—" Electra " complex, if we use the Freudian name.

It begins with the child Polly, who absorbs the reader's interest for the first three chapters, and who was quite plainly meant to be the heroine of the novel. One of the most significant points in the analysis of this complex is the transfer of the interest to Lucy, and Lucy's complete usurpation of the heroine's rôle. Judging from Miss Brontë's letter to her publishers concerning the book this was not intended and we are not even sure that she ever fully realized what she had done, that she ever knew that she had robbed Polly and given all her charm to Lucy. The significance of this will come out as we proceed with the analysis.

Polly appears as an extraordinarily self-possessed, old-fashioned, but intensely feeling and keenly suffering diminutive creature, whose whole world exists in her father. She has just lost her mother, but the loss is not so great as it would appear, for her mother was a frivolous society woman who neglected her child shamelessly. The author is convinced that Polly is wholly the creature of her imagination and that her treatment is purely objective, and she probably would have resented the suggestion that there is a parallel between Polly's mother and her own, yet the development of the plot and the characters are such as to convince us that this was the case. The cases of Segantini (13), and Wagner (24), might be cited as partly comparable. Why should Polly's mother have neglected her? This was no necessary part of the story; the consequences of this neglect are never referred to or worked out in any obvious way. This passage was written from the suggestion of the unconscious infantile soul of Charlotte, which still harbored resentment (all unconscious) against the mother who had never, within her memory, played a mother's part to her. Little Polly is left to the care of Mrs. Bretton, Lucy's godmother, whom Lucy, a girl of fourteen, is visiting at the time, while Polly's father goes abroad in an attempt to cure his grief for his wife's death. Polly's intense suffering over the separation, so much greater than any child is supposed to feel, indicates to the casual reader a precocious emotional development, and to the psychoanalyst, a situation arising from previously conflicting emotions of the nature of love and hate in the parent-complex.

An indication of the resistance to the old memories that worked to conceal Polly's true identity from her author is found in the unsympathetic attitude of the girl Lucy, who was intended to embody one aspect of her author's personality, the cold, unemotional, external shell of her. When Polly insisted on waiting upon her father, awkwardly passing his teacup with both her tiny hands, Lucy says, " Candidly speaking, I considered her a little busybody." This was Lucy's ostensible attitude to Polly, yet she gives various unintentional, almost unconscious indications of a secret sympathy and affection for the child which she will not acknowledge.

The most interesting feature of this phase of the story, to the psychoanalyst, is the transference of Polly's affection to the youth, Graham Bretton, after her father has been absent some weeks. Graham treats her with a mock deference that suits her fancy for being grown up, and then makes her his playmate, soon winning her heart completely. The little comedy of the playmates is presented with exquisite tenderness and sympathy, and with an understanding of the mental reactions involved almost uncanny to one who takes a superficial view of Miss Brontë's life and equipment. It is the lilliputian play and interplay of masculine dominance and feminine submission, mixed with coquetry, and it is more. It is the mixture of authority and tenderness, on the boy's side, that is most characteristic of the paternal attitude, and, of imperiousness and gentle docility that belongs to the petted little daughter, and to the ideal wife of the romancers. It is to Graham that Polly ministers now, as she had ministered to her father, over Graham she tyrannized, to Graham's teasing she submitted with a tantalizing conflict of emotions, both pleasurable and painful.

Polly's reaction when the news came that her father, now almost forgotten, was coming back to take her home with him is the most telling part of this study of a child. With the first faint realization that she had transferred her affection from Papa to Graham came irritation, directed against her informant—Lucy, as usual—and incredulity as to his coming. After this she became serious and silent, a struggle was evidently going on. But when importuned by Lucy as to whether, she was not glad to go to her father, "Of course!" she said at last in that trenchant manner she usually employed only in speaking to Lucy.

The masochistic side of her attachment to Graham is shown in the little scene that occurred the night before her departure with her father.

"Little Mousie (Graham's nickname for her) crept to his side (he being absorbed in his books and having scarcely heeded the announcement of her impending departure) and lay down on the carpet at his feet, her face to the floor; mute and motionless she kept that post and position till bedtime. Once I saw Graham—wholly unconscious of her proximity—push her with his restless foot. She receded an inch or two. A minute after one little hand stole out from beneath her face, to which it had been pressed, and softly caressed the heedless foot. When summoned by her nurse she rose and departed very obediently, having bid us all a subdued good night."

But when Lucy went to her room later she found Polly suffering from an inexpressible grief more agonizing even than the painful scene that followed her first separation from her father. Lucy thought fit to warn the six year old child against caring too much for the half grown boy, telling her on this and other occasions that boys did not feel as girls did, that too much affection from girls was felt as troublesome, that she must not expect a return of his affection. It is the dialogue between Reason and Feeling again, which took place often in the mind of Jane Eyre, of the group-up Lucy Snowe, and of Charlotte Brontë herself.

Graham and Paulina (" Polly ") set out to show the combined personalities of father and daughter, lover and mistress, in their interplay, satisfying the infantile demand for a combination of these two rôles. As long as the scene was laid in the remote land of childhood, where Polly's true identity was buried deep as Gradiva (25), under the ashes of Pompeii, they could be kept ideal figures, not consciously taken from the life, differing from their actual prototypes in every external feature, and unrecognized by the mind that conceived them. Paulina was pretty and imperious, Charlotte was not, Graham was fair and debonair, as the misty, illusion-veiled hero of her childhood was not. But the soul of Polly was the soul of Charlotte, and Graham was the ideal hero, glorious, tender, careless, indifferent to the love lavished upon him.

Lucy was intended from the first to be an unflattering portrait of the author, plain, cold, sarcastic, such feelings as she had being carefully kept out of sight, somewhat misanthropic. She was to be the *outside* of Charlotte, and was to play a subordinate part in the story, being the narrator, but letting her story give way to the superior interest of Paulina's. When the novel moved out of the misty regions of childhood however into the near and clear land of Just-yesterday, when the course of the plot carried Lucy, as a teacher, to Brussels, because the material gathered during Miss Brontë's own pupil-and-teacher days there demanded to be used, and when real and well-remembered scenes and incidents began to be woven in, Miss Brontë lost control of her characters. They controlled her. Lucy refused to be the cold, austere, somewhat disagreeable person that the author perceived as one phase of her own character (and a superficial phase it was, as she must have felt) and insisted upon being her full, passionate, self. So it came about that the sensibility, the intensity of feeling, the enormous capacity for suffering, that were shadowed forth in little Polly came to ripeness of development in Lucy, and with them went the charm that had invested Polly, making Lucy a figure to enchain the interest and seize upon the sympathy of the reader in spite of her plain, almost forbidding exterior.

When Polly came back into the story—so late that one is tempted to think the author must have been very near forgetting her—the life is gone out of her. Conscientiously Polly is reconstructed as the external Polly ought to be, but there is nothing in her to make her real because the fire with which she was first conceived has all gone into the vivifying of Lucy, and a fiery, living, tameless creature the pale cool embryonic Lucy of the first three chapters has become.

So far had the character of Lucy usurped the heroine's place that the writer very nearly bestowed upon her the hero, Graham Bretton. She recollected her original purpose in time, as a letter to her publishers showed. It may be well again to call attention to the fact that the book was written slowly, with long intervals of inactivity, and that the writer put into it more of her personal, emotional experience than she had put into any of the preceding books. For this reason it "wrote itself" in a manner of speaking, and she was unable to conceive her plot clearly from beginning to end. It evidently shaped itself as the story grew. Having seen, however, that a match between Lucy and Graham was unsuitable, and having recalled Paulina to the scene, she began developing the character of the Professor, Paul Emanuel, whom she had selected for Lucy. He grows in attractiveness and lovable human qualities, while Graham becomes artificial in just the same way as Lucy waxed in interest while Paulina waned, and for the same reason. The reader's interest obeys the course of the writer's, transferring itself from one pair of characters to the other, as the love of Lucy and Paul is developed. Madame Beck, the sinister mother, appears as Lucy's rival, completing the most ancient of triangle plots. The third volume of the story is most impressive and intense because the situation has come to the basic reality of affective, or emotional, life, at last. A hero founded upon the ideal father-personality and M. Heger, a heroine who was the impersonation of herself, and an older woman to whom she ascribed the cold, deadly implacability of a malevolent pursuing fate, play out in the most realistic and yet most artistic form she had yet been able to give it, the drama that she set forth in each of her other novels. It was more fantastic, more dreamlike in Jane Eyre, more consciously adapted to the conventionalities in Shirley, here it was given with unswerving fidelity to her own inward drama of feeling.

In no other emotional situation of importance was she thoroughly at home. She rose to her highest plane of artistic ability only when she dealt with lovers who combined with the adult relation a large measure of the interplay of feeling that occurs, consciously or unconsciously, between father and daughter.

In *Jane Eyre* the solution of the triangle plot is strikingly like the productions of a child's dream,[2] as Chesterton pointed out. The rival wife, much older than the heroine, of course, is reduced practically to the level of an animal, an inferior creature to whom no real consideration is due. The house is burned, the mad wife perishes in the flames (and we have a right to recall the significance of fire as a symbol of sexual passion) the husband is thus freed, but at the same time he is blinded. He cannot see, therefore, who it is that comes to him, to take the place of wife. His blindness satisfies the "resistance" to the fulfillment of the wish (in the language of the dream interpretation of Freud). The story did not demand that Rochester be made blind, but conflicting forces of the author's unconsciousness required it. The story of Rochester and Jane dramatized her own love for the forbidden man—first for her father, a love wholly unconscious, second for the teacher, a love partly conscious, and the writer had to make some concession to the fundamental obstacle to wish-fulfillment. In childish fashion this was made by shutting the father's eyes to the true situation. I hope the reader is not confused here between the literal sense of the story, and the psychoanalytical meaning of it. Let me repeat—the blindness of Rochester played no important rôle in the story as a literary production. It was introduced to satisfy a demand of the unconscious emotional complex of the writer, out of which complex the story grew.

A fact in the personal history of the author points the relations of the idea of blindness to the father-concept, and of the emotional complex centering about Charlotte's father and M. Heger. She began Jane Eyre in Manchester, whither she had accompanied her father, that he might undergo an operation for cataract. A little while before this she had thought that she herself might be losing her sight, and had carefully abstained from much writing and from all close work, in order to preserve her sight for the use of her father, in reading to him and serving his needs. Her fear appears to have been groundless, probably the eye trouble was a neurosis of the sort to which she was subject. It arose when she was still feeling the effects of the struggle in which she decided to remain at Haworth with her father, and was the crowning act of her self-abnegation for his sake. For him she must even give up her beloved writing. It was at this time that she wrote to M. Heger, that were it not for the failure of her eyesight she would be writing a book, and would dedicate it to him (Gaskell). She soon wrote *The Professor,* as we have seen, and began *Jane Eyre* while in Manchester with her father, after her correspondence with Heger had come to an end in the manner described in Chapter Three. The thought of blindness was thus by an external sequence of events woven in with the emotional elements that constituted the plot of *Jane Eyre,* and so the blindness of the hero was a natural mode of satisfying the moral resistance to the union of two people who, in so far as they impersonated the father and daughter of the universal infantile romance, could not be legally united.

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[2] This resemblance to the dreams of childhood was also pointed out to me by Dr. Edward J. Kempf, to whom I am indebted for other valuable suggestions during the work of revising the manuscript.

In *Villette,* however, the moral resistance could not be so easily satisfied, as Paul and Lucy were so very closely identified with the real characters in the author's own life. She never was conscious of the true nature of the ban upon their union, such truth being always, in all of us, normally, repressed into the realm of the unconscious.

She only knew that she had an unshakable conviction that such happiness could not be, that Paul did not come back, that he was drowned at sea. Her publishers wished her to change this ending, she replied that she *could* not—she had to write the story as it was given to her. Then her father begged for a happy ending. For him she made the compromise that almost spoils the eloquence of that tragic climax. This is the only occasion on which she was ever known to change any important part of her writing at the behest or suggestion of any person, and it is significant that she yielded thus far to her father, who had always been the most influential person for her, who was truly a part of herself. But even for him she could not really change—even he could not abrogate the law that decreed such a marriage impossible.

She veiled the truth in dark and doubtful words, not saying that Paul was drowned, leaving the end ambiguous, yet deceiving no intelligent reader. Strangely, it seems at first glance, the reader is satisfied with this unsatisfactory ending. The inner perception recognizes the truth of the ban upon this marriage, because of the unconscious remainder of the romance of childhood existent in each mind.

The only solution of the conflict grown to its full proportion, as in *Villette,* was Death. No childish ruse of blindness would suffice. Death and Love were close companions in her emotional life. The story of her life is like a long funeral dirge, its measures marked by the death, one after another, of those nearest her. Dream analysis has shown how the fancied experience of death may represent the culmination of love, and fantasies of dying, in the introversional type of psychoses, are the outcome of unsuccessful love affairs. The closing page of *Villette,* with its powerful lyric quality, has well been called by Miss Sinclair "A song of Triumph—the triumph of Death." It was the triumph of the fundamental passion of life, the undying love that transfers itself to one object after another, throughout the development-period, until it rests upon the object that fulfills its ideal, that gave to this celebration of death its exalted and triumphant note. In death lay the mystic fulfillment, as death was the only solution of the problem, a problem so deep in its significance that we can scarcely formulate it, and few people realize its existence in its true form.

The effort to solve the problem of her love-life was the impetus of Charlotte Brontë's literary production, as it has been the impetus of many remarkable men and women. She was successful in her work in proportion to the extent to which her vehicle of expression corresponded to the play of her own inner life. Intensely subjective, plots and characters had to represent her own drama. This is not said in order to detract from her glory, but, if possible, to do justice to it. The greatest genius is he who can succeed in giving his own soul most fully to his creative work. This capacity of pouring out her own self was given to Charlotte Brontë by the suffering she experienced, by the racking conflicts that demanded relief. Without this forcible release of emotional energy her work would have lacked the fire, intensity, and fidelity to nature that makes it the work of genius.

Charlotte Brontë has to-day an assured place in English literature. Her work in spirit belongs to the present more than to her own time, inasmuch as it sets forth psychological truths that are recognized now as they were not in 1850. But one woman in English literature of the nineteenth century has a place very far above her and that one is her own sister, Emily Brontë. Emily Brontë was even far less understood by her contemporaries than was her sister. No one yet has attempted an explanation of her character and genius, though much has been said in her praise. Even if there is sufficient data for an adequate study of the sources of her power the work lies still in the future.

The story of the Brontës shows the paramount influence of family life at its best and at its worst. The strong unbroken impression made by an isolated and contracted home life continuing for many years made all the children alike incapable of successful adaptation to the demands of the larger world. They were shy, reluctant strangers wherever they went. Yet the untrammeled intellectual stimulus of that unusual home, free from many conventional restraints, furnished the means of self-expression and the chance to achieve work of an artistic merit that procured for two of them lasting fame. These same influences, combined with a poor heredity, produced neuroses or other disease in all. We are grappling in this present day with the problem of the family as it affects the individual in new ways. Studies of other families, now living, as complete as that made by the host of Brontë enthusiasts of this family, may shed light upon the question. At least such study may show the need of caution in our eugenic and educative measures for the development of a physically and mentally healthy new generation.

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(NOTE.—In the appended list only those works to which specific reference has been made are included. The many writings on the Brontës, from which I have gathered impressions, and the works on psychoanalysis which are the basis of my work, would make too long a catalogue for the pages of this magazine. If this essay forms later on a part of a larger work on the Psychoanalytic Study of Genius, as I hope it may, a full bibliography will be published.

I wish to acknowledge my indebtedness to Mr. Alfred Ela, of Boston, for numerous references to Bronteana, which might otherwise have escaped my attention.)

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AN EXPERIMENTAL STUDY OF VISUAL FORM[1]

By M. J. Zigler

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INTRODUCTION

The present study has its origin in Schumann's experimental investigation of the visual *Gestaltqualität*[2] or *Form of Combination.*[3] We had three reasons for resuming Schumann's work. In the first place, his analyses seemed to be of so great importance from the methodological point of view as to justify the spending of time upon their repetition. Secondly, Schumann's work was done before the publications of the Würzburg school had brought out the issue of process and meaning; and we might therefore hope to carry his analyses further, and to give them a systematic setting. Thirdly, the recent criticism by Bühler of Titchener's position, which (*e.g.,* in his Textbook) agrees substantially with that of Schumann, shows misunderstandings which can best be removed by the exhibition and discussion of experimental results.

Our experimental work has thus been partly a repetition, and more largely an extension, of certain of Schumann's experiments. It consisted of five series of observations, which were briefly as follows. I. We presented to our observers white cards covered with a pattern of equally spaced black figures, squares, circles, lines, etc. Under observation these figures fell into natural groups. *O*'s task was to report the experience accompanying group-formation. II. We presented cards upon which figures, such as circle, barrel, dumbbell, were partially outlined in black upon the white background. Under observation these white figures tended to complete themselves. *O* was required to describe the figures which resulted from the realization of this tendency. III. A figure, *e.g.*, a square outlined in black on a large white field, was gradually changed during presentation to a new figure, *e.g.*, an oblong. *O* was required to describe the forms observed. IV. A figure, *e.g.*, a square drawn in black upon a white ground, was instantaneously replaced by another figure, *e.g.*, a triangle. *O*'s task was the same as in the preceding experiment. V. Figures were indicated by white dots set on a large black field (thus, 3 dots represented a triangle, 4 a square, etc.) ; these figures could be drawn out or compressed into new figures while observation was in course. *O*'s task was again the same as in III. We take up the experiments in the order given, and describe both the conditions under which the observations were made and the results obtained from them.

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[1] From the Psychological Laboratory of Cornell University.

[2] See F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen, I, *Zeit. f. Psychol.,* 23, 1900, 1ff.; II, *ibid.,* 24, 1900, 1ff.; III*a, ibid.,* 30, 1902, 241ff.; III*b, ibid.,* 30, 1902, 321ff.; IV, *ibid.,* 36, 1904, 161ff. References to these articles will be indicated by I, II, etc.

[3] The English term 'Form of Combination' seems to have been introduced by G. F. Stout, *Analytic Psychology,* I, 1896, 65ff. It is not our purpose here to enter upon the history of the concept, or to discuss in detail the various theories which have been propounded since C. von Ehrenfels published his well-known article "Ueber Gestaltqualitäten," *Vjs. f. wiss. Philos.,* 14, 1890, 249ff.; the reader may be referred to A. Gelb, Theoretisches über "Gestaltqualitäten," *Zeit. f. Psychol.,* 58, 1910-11, 1ff., and to K. Bühler, *Die Gestaltwahrnehmungen,* I, 1913.

SECTION I

In this experiment we used patterned cards, of the kind which Schumann had found favorable to group-formation. Our object was to discover whether a longer and more systematic course of observation would push the analysis of the group-experiences further than Schumann had been able to carry it.

Our stimuli were ten white cards, about 25 cm. square, on which patterns of equidistantly spaced figures, such as lines, squares, diamonds, were drawn in black. The patterns were of four types.[4] Two cards were filled respectively by parallel vertical and horizontal lines, drawn at distances of 1.5 cm. Four cards showed 64 discrete figures, such as squares or diamonds, equidistantly spaced in both directions at 1.5 cm. The maximal dimension of these figures was also 1.5 cm. In the other two types of stimulus-card the figures were not discrete but contiguous. Two cards were marked off into white squares of 1.5 cm. side, formed in the one by vertical and horizontal cross-lines and in the other by diagonals. The remaining two cards were of checkerboard pattern and contained alternate white and black units, in the form respectively of diamonds and triangles.

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[4] Diagrams illustrating these four types of pattern are given by Schumann in Figs. 1, 2, 5 and 6. See I, 7ff.

The cards were presented in an exposure apparatus, before which *O* sat at a distance of some 2m. On the average, the series of 10 was presented to each one of the five *O*'s 26 times. The total number of presentations to all *O*'s was 1,281. With the exception of a single series in which the time of exposure was varied, the duration of presentation was 5 sec.

The observers were Dr. C. L. Friedline (F), graduate student in psychology; Dr. J. M. Gleason (G), instructor in psychology; Dr. L. B. Hoisington (H), instructor in psychology; Mr. B. L. Swartz (S), assistant in psychology; and Mrs. M. J. Zigler. The two last-named were untrained; the others had had varied training in observation.

At first the *O*'s worked under the following instruction: "I shall show you a series of cards on which a number of figures are drawn. You are to observe the figures carefully and, when the screen drops, to describe what you have seen." Later, with a view to fuller description of the group-formations, the instruction was changed to read: "I shall show you a card on which a number of discrete figures are drawn. From the elements form a natural group. When the curtain falls, describe as accurately as you can what you observe when the group has formed." After some hundreds of observations had been made, the reports of F, G and Z were examined, with the idea that characteristic or "natural" groupings might come to light. We found, however, that certain cards lent themselves more naturally to one, and other cards to another kind of grouping. Acting upon this suggestion, we now required the three *O*'s to form groups of a certain number of elements, or of a certain shape, according to their known tendency in presence of the particular stimuli; we hoped in this way to facilitate grouping, and also to secure still more complete descriptions. We further provided a push-button, connected electrically with a stop-watch, and asked the *O*'s to signal the appearance of a group.

H and S continued under the instruction to form natural groups, S until the end, and H until near the end of the experiment. In the last few sittings with H the time of exposure was varied at haphazard in 1, 2, 3, 4 and 5 sec. exposures.

Results.—All *O*'s reported groupings from the first. During the earlier work the reports generally gave successive groupings as the eyes moved about over the card; later we attempted to control conditions so that *O* should obtain only one group-experience during a presentation. The general number of elements constituting the groups, the variability from the general rule, and individual differences were irrelevant to our purpose. Our primary concern lay in the description of group-formations; in whatever it is that differentiates the elements grouped from the similarly drawn ungrouped elements. The whole course of our experimental procedure was aimed to secure this description. The push-button was introduced to allow *O* to signal the appearance of a group, and to halt the exposure for a report on the first group observed.

The results may be reported in two classes: *A*, those characterising groups in which the elements are of the discrete pattern; and *B*, those pertaining to groups in which the elements are contiguous.

A.—The descriptions given by all *O*'s indicate that the marks differentiating a group may belong (1) to the intervening spaces separating the discrete elements, (2) to the elements grouped, and (3) to tied imagery, which sometimes plays a part in group-formation. (1) The white spaces surrounding a group may become (*a*) broader and (*b*) qualitatively lighter or darker than the intra-group spaces.[5] These distinguishing marks were noted by all *O*'s at first, and persisted as the predominant facts for the untrained *O*'s to the end. The more trained observers, however, soon described changes (2) in the elements themselves, and as the work progressed these were reported as the more salient facts.[6] The elements of the group were described by all *O*'s as (*a*) blacker in quality, (*b*) clearer, (*c*) more extended, and (*d*) standing out in perspective. Two *O*'s, G and H, also reported that the elements grouped possessed (*e*) a greater intensity. Four of the five *O*'s also indicate the influence (3) of tied imagery in separating the groups. There were sporadic reports of "thread-like black lines," "gray lines," "gray streaks," "faint gray lines," "shadowy gray lines," which connected the elements of a group or formed its boundary.[7]

B.—Group-formations in the contiguous patterns were characterized (1) by alterations in the black lines bounding the groups, and (2) by changes in the elements themselves. (2) The black contour lines were described as (*a*) qualitatively blacker, (*b*) clearer, (*c*) more extended or broader, and (*d*) more sharply defined at the edges, than the other black lines on the card. G and H again reported (*e*) that the lines were enhanced in intensity. The attributive changes in the elements themselves (2) were noted more generally in the checker-board patterns, although with the trained *O*'s in particular they were also found with the small white square-patterns. The grouped elements were characterized by (*a*) changes in quality, (*b*) greater clearness, (*c*) greater extent, and (*d*) perspective of the group. G and H also reported (*e*) an increase of intensity of the grouped elements.

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[5] Schumann reports that the white intervening spaces *hervortreten* or *auffallen im Bewusstsein,* obtrude in consciousness, while the intra-spaces recede, *zurücktreten;* and that when these spaces obtrude they are broader. The obtrusion, he says, is not necessarily a matter of perspective, although he indicates that perspective plays a minor part. See I, 9ff.

[6] In another place Schumann emphasizes change in extent as the fundamental factor in the experience of group-formation. This applies essentially to the spaces separating the groups from the other elements; it may, however, apply also to the elements of the group themselves. In the latter case the facts are very undependable, afford "ein sehr unsicheres Kriterium." II, 32.

[7] Schumann did not report the influence of tied imagery in these groupings.

Unless specially referred, the facts given above were reported by all *O*'s. The untrained *O*'s tended to find differences in the separating spaces or lines, while the more trained *O*'s soon tended with practice to rely more and more upon the alterations in the elements.[8] We thus arrived at a greater variety of attributive changes in the group-experiences than Schumann attained; and thereby we have rendered still more plausible his contention that form can be adequately accounted for in the phenomena of experience themselves.

The reports gave the further suggestion of stages in the temporal course of the grouping-experiences, stages which indicated a probable correlation of process and meaning. All *O*'s either implied or definitely distinguished three such stages in temporal course. First comes a period of *expectancy,* in which *O* is expectantly set for a group to emerge from the isolated elements. Secondly, a group appears, and is attended by certain attributive changes with which an *abstract* or geometrical meaning of form is correlated. Thirdly, the group takes on the meaning of an *object,* and at the same time the attributive changes become more pronounced. The effects of perspective and tied imagery were essentially confined to the groups which assumed objective reference. These hints of correlation induced us to pursue the experiments farther with more promising materials.

SECTION II

Again the suggestion of appropriate materials comes from Schumann.[9] We used white stimulus-cards on which plane white figures were partially outlined by black characters placed at various intervals along their contours. Our problem here was twofold: first, we desired to get a full description of the perceptual experiences when the figures were imaginally completed; and secondly, we anticipated that experiences wherein the perception of form is more or less gradual in its development would throw into clearer relief the relationship of process and meaning.

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[8] Schumann here emphasizes the obtrusion of the limiting lines in consciousness. These limiting lines are reported by him as "schwärzer und schärfer begrenzt." I, 11ff.

[9] Schumann's Figure 7 gives the principle upon which the construction of these cards is based. See I, 13.

We used a set of 18 stimulus-cards, about 23 by 27.5 cm. A heavy black border framed the cards in the shape of a rectangle, triangle, parallelogram. The 18 figures partially outlined within these frames were: 1 square (Schumann's figure), 2 disc, 3 barrel, 4 oval, 5 hourglass, 6 basket, 7 figure 8, 8 dumbbell, 9 crescent moon, 10 scroll, 11 double axe, 12 horse shoe, 13 four circles, 14 crescent and disc, 15 pointed ellipse, 16 heart, 17 three cherries, and 18 spinning top. Every figure extended over the entire vertical distance within the black frame, and its outline was partially indicated at the sides by black characters of variable shape.

The cards were shown at haphazard in an exposure-apparatus on an average of 9 times for all cards and all *O*'s; the total number of presentations to ten *O*'s was 1,559. The *O*'s were: Mr. P. J. Cavanaugh (Ca), graduate student; Miss C. Comstock (Co), graduate student; Mr. F. L. Dimmick (D), assistant; Dr. L. B. Hoisington (H); Mr. H. S. Liddell (L), fellow; Dr. H. Sheppard (Sh), instructor; Miss R. Stutsman (St), graduate student; Miss A. H. Sullivan (Su), fellow; Mrs. M. J. Zigler (Z), and the writer (Zi). The observations were made in a light optics-room. *O* sat at a distance of 3.5 m. from the apparatus. The time of exposure was kept constant at 3 sec.

The first instructions read: "I am going to present a series of stimulus-cards, one at a time, on each of which a figure is represented. You are to observe it carefully, and when the curtain falls, to describe what you have observed." In order to evoke more explicit and fuller descriptions in attributive terms, the following instructions were subsequently used: "I shall show you a series of cards on each one of which a white figure is represented within a black frame and against a partial black background. Observe the figure carefully, and when the curtain falls describe so far as possible in attributive terms what you have observed. You need not report on the black background. You may, if you wish, draw an outline of the observed figure upon the blank provided. The time of exposure will be three seconds." The blanks provided for sketching were made from pad-sheets 7.5 by 12.5 cm. *E* pencilled in a border in such manner as to leave the area within the frame similar to that of the stimulus-cards.

Results.—The figures were correctly completed in tied imagery by all *O*'s on an average of 57+% of the total number of presentations. The least successful *O* gave 40+%, the most successful gave 84+%. The unsuccessful reports are of two kinds: figures with incomplete outlines, and observations in which *O* failed altogether to detect the more or less concealed figure. The "failures" comprise 15% of the total reports.

In one of the figures, the contours consist wholly of straight lines; in all the rest the outlines are mainly, if not wholly, made up of regular curves.[10] Individual differences appeared in the nature of the imaginal limiting lines, as well as in the aptitude for finding the figures.

We may classify the facts which all *O*'s report, as differentiating the imaginally completed figure from its background (which is of the same uniform quality), under four headings.[11]

(1) The outline is quite commonly given as an imaginary curved line, which may have either a lighter or a darker quality than the white background. Some *O*'s show a slight preference for the one or the other quality, while with others the reports are about equally distributed. The following are examples:

(*a*) *Lighter quality.*—"A very bright tiny line marks the figure out very clearly" (15 H 2).[12] "The figure was bounded by a very narrow white border, which was very bright" (3 L 5). "This figure is defined all around by a little bright white curved line" (4 C 14). "A narrow white line ran all around it. This was very bright white" (6 Ca 5). "There was a real white threadlike line forming the boundary of the figure" (6 Z 5).

(*b*) *Darker quality.*—"It was just as if the outline was painted by a small brush. I really saw the circles bounded by delicate black lines" (8 L 2). "There were narrow dark gray bands around the figures" (13 H 1). "The boundary was given in fine, cloudlike, but yet bright, dark gray" (3 L 7). "A very fine black line completes the figure outline" (18 Co 6). "This dark gray outline ran clear around both balls" (7 Su 9). "The outline was distinctly completed by a dark gray line" (13 D 2). "There was a gray line which ran up to the black border and down around the other side of the figure" (3 St 4).

(2) The figure is distinguished from its background by a difference in quality, which is given by all *O*'s as both lighter and darker than the background. Some *O*'s again show a rather marked preference for the one or the other quality. This difference is frequently reported together with the imaginal contour-lines; sometimes, however, the figure is marked off merely by an abrupt transition from its quality to that of the white background. The following are examples:

(*a*) *Lighter quality.*—"The figure is a much brighter white than the outside white" (10 Co 7). "The whole figure is much whiter than the grayish background" (8 Su 11). "The whole figure grew lighter and was surrounded by gray about the edges" (6 St 3). "It was slightly whiter than the background, which became light gray" (9 Z 3). "As soon as I saw the outline of the figure, it became brighter, and it had a gray outline" (13 Ca 4).

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[10] Schumann was unable to observe 'subjective limiting lines' in figures with curved outlines; he observed them only in figures with straight boundaries. See I, 14.

[11] Schumann gives two modes of differentiation of the figure from its background: an imaginary limiting line; this may be a bright white line, which he calls a true 'subjective line,' or a black line (*reproducirtes Vorstellungsbild*); and qualitative change of the area of the figure. I, 12ff.

[12] The figure before *O*'s initial indicates the stimulus-card, corresponding with the numbers given above. The second number indicates the presentation of the particular card.

(*b*) *Darker quality.*—"A grayish film extends over the surface of the whole figure, and outside of the figure it is very white" (7 Co 4). "Its quality changed to a slightly grayer color, and the regions beyond the figure were brighter" (7 H 8). "The figure was of a dark gray and the background was white" (15 Ca 3). "As I looked at the 8, it became grayer than the outside" (7 Z 2). "That scroll stood out from its background very gray, and the surrounding area was definitely whiter" (10 Zi 5).

Several reports indicate that the figure assumed a lustrous quality. "After an instant there was a central part of the figure which became clearer, and the quality changed to a grayish lustre" (16 H 3). "Again the quality of the figure was a gray lustre" (13 H 4).

One *O* reports a case in which a free image helps to determine the quality of the figure. "I found myself endeavoring to identify that figure. The attempt to name it was very marked. First I said Circle, and then No. Kinaesthesis of shaking the head with this rejection. Then I called it an Indian pot. I do not know that I named it as such, but I identified it as such. I had a free image of a piece of Indian pottery which conformed with this. The figure now looked yellowish gray" (6 D 3).

Another *O* mentions a difference in the texture of the figure. "The figure is distinguished from the background by a difference in texture. The texture of the figure is thinner than that of the background" (18 Co 1). "Just a difference in texture distinguishes it. The figure is thinner" (4 Co 11). "It is a difference in texture that marks it off. The dumbbell is less dense. There is only this qualitative change that marks it off" (8 Co 6).

(3) Another fact reported by all *O*'s is that the figure assumes greater vividness than the rest of the exposed field. The black characters were vague and often unremarked. When the *O*'s undertook to sketch the figure, they were very often unable to place any black on the field; they declared that there was black there, but that they could say nothing about it.

"The figure became very clear and the outlying region became very vague. I could not say anything about the outlying parts at all" (6 H 4). "I did not notice the background as grayer; but I saw the figure as whiter, and knew the background must be grayer. The background was very very obscure" (8 Su 1). "The background was so unclear that I cannot report on it. The figure was the only clear thing" (14 Su 9). "I have no notion at all of the area surrounding the figure" (12 L 2). "The background disappeared when the figure was clearest" (10 Z 3). "The crescent stood forth in attention very vividly and all of the background was indistinct. When you get the figure it just sticks together as a whole and all the rest drops out. The attention-factor is the tremendous thing in the perception" (14 D 1). "There is a sort of spontaneity with which the attention goes to the figure. I absolutely lose all of the background" (15 D 3). "Soon it became a unitary figure, and the rest of the card was ignored. The figure was very very clear and I did not observe anything in the background" (14 Zi 2). "The figure was so clear that I saw nothing else. All of the background was unclear" (8 Zi 2).

(4) All *O*'s report that the figure assumes perspective, in addition

to the attributive changes already mentioned. The perspective, as we shall presently see, comes late in the temporal course and is correlated with objective reference of the figure.

"The outline was hardly so definite as a pencil mark. However, the circularity of the extent had for me the vividness of a natural perception, as the perception of a very white disc of cardboard hung against a slightly darker quality of background. This suggests the vividness of the perception" (3 L 6). "There is no line defining the figure, but it just stands off spatially, just as though a space of half a cm. were between the heart and the background. It is that that marks the figure off for me" (16 Co 1). "I had a distinct perception of separateness. The background seemed a quarter of an inch behind the white figure" (13 Co 1). "The figure became brighter and stood out in perspective, very markedly nearer me, with the darker background surrounding it in the distance" (9 H 5). "When the quality changed to this grayish lustre, the figure seemed an object set back behind the background" (16 H 3). "That figure came out in marked perspective toward me" (8 Z 5).—

Finally, two *O*'s reported changes in intensity of the area of the figure along with changes in quality and clearness. Su gave this report very frequently. Asked just what she meant by intensity, she replied: "The figure becomes more glaring with increased intensity." H insisted that a figure, even if of the same quality as the outlying region, might assume greater or less brightness. The following are examples:

"When the white dumbbell was observed, it was more intensely bright. It was 'glary'" (8 Su 7). "This bowl was very white, but of rather low intensity. It was not glaring and not very bright" (6 Su 7). "The figure now became more clear and more intensely bright and was definitely outlined. I did not note much, if any, change in quality, but it merely became clearer and brighter" (8 H 7).

The *O*'s distinguished three phases in the course of observation which culminated in a fully developed figure. First, under the influence of the instructions, which have set the *O* for perceiving a figure, comes a period of searching or *expectancy*. During this period *O* casts his eyes over the card and along the bits of black in the field, or fixates briefly certain parts of the white area in the expectation that a figure will emerge. Secondly, some portion of the white field between the black characters holds *O*'s eye, and becomes the sensory basis for an abstract or plane-geometrical figure-meaning. The figure is not yet attributively differentiated, and there is no completion of the contours in tied imagery. Thirdly, by steady fixation of this area or by eye-movements around its periphery, the area of the figure becomes differentiated from its background in quality and vividness, assumes perspective, and is labelled by a concrete name, such as ball, sun, egg, pear, lemon, gold-fish bowl, Indian pottery. In the last two phases we have correlation of certain sensory moments with a definite type of meaning. Their distinction is especially significant in that the instructions did not impose, or even suggest, the task of reporting temporal sequence.

All ten *O*'s definitely indicate these three phases, and their characterisations are essentially the same. This does not mean that all phases, and the two last in their dual aspect of process and meaning, were reported in every observation; we have already said that the cards offered a sort of puzzle, which was not always successfully solved. There were various omissions and short cuts. In the 15% of "failures," in which no figure at all was found, phase I only was represented. There was, in fact, a certain percentage of observations in which all three phases were noted; there were cases in which phase I was omitted, while both aspects of the other two phases were fully given; and a few reports by all *O*'s were cut short at the end of phase II, so that processwise the figure was not completed in tied imagery, and was not differentiated in quality, clearness and perspective from the outlying white field, and meaningwise it failed to take on objective reference and was not labelled with a concrete name. The accompanying table shows the percentages of such complete and undeveloped reports for all *O*'s.[13]

O	Total Presentations	Total Complete Reports	%	Total Undeveloped Reports	%	Combined %
Ca	144	9	6.2	11	7.63	13.83
Co	270	9	3.3	29	10.74	14.04
D	90	8	8.88	4	4.44	13.32
H	144	33	22.9	6	4.16	27.06
L	83	7	8.43	12	14.45	20.88
Sh	216	13	6.01	13	6.01	12.02
St	180	15	8.3	11	6.11	14.41
Su	216	22	10.1	15	6.94	17.04
Z	108	10	9.26	5	4.63	13.89
Zi	108	19	17.5	7	6.48	23.98

The following excerpts are descriptive of these phases.

Phase I.—"Just the total white field. That peculiar organic set that makes one look for a figure. The whole card was not equally clear at all times, due to rapid shifts of fixation" (9 H 4). "There was first a tendency to explore, and the whole card was explored, more or less. I was attending to the white parts and they were clearer than the black" (12 H 8). "I keep trying to see something in the center but cannot do it. There must be some figure among the black dots. I feel that they partly outline some figure, but there is no meaning there" (17 Su 7). "Inside of this triangular area were some black figures. Inside of them I was hunting for a white figure"

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[13] The remaining reports are distributed among the following classes: (1) 'condensed' reports, wherein phase II is omitted, and there is an immediate jump from phase I to phase III; (2) 'incomplete' reports, from which one or both aspects of either phase II or phase III are omitted; and (3) the group of 'failures.'

(17 Su 12). "I had a set for a white figure, but when I first saw the card it was only black on white. . . . So I continued to look more actively. . . . I think there was a good deal of eye-movement here. When I do not get the figure at once my eyes move from one black spot to another to get a figure" (9 Co 11). "I just looked all around among the ends of the black lines to get a figure that I could put a meaning to" (14 Sh 8). "Before the circles came in, my eyes were moving around over the card trying to get a white figure. I then noticed the outline of the edges of black and moved along it and the figure appeared" (13 Ca 3). "I did not get the figure until just as the curtain dropped. My fixation and attention jumped around all over the possible parts of the pattern defined to find a figure" (8 D 4).

Phase II. (1) *Process.*—"Then a shift and I saw two circular areas set in between the black lines. At first these had no particular character except they were the clear things" (7 H 5). "Then a bit of the card catches you and stops the exploration. The figure is not completed or marked out in this stage. Just this bit catches the attention" (13 H 7). "I saw a white sort of a figure. It was only moderately intense and clear. There were white surfaces at the sides of the figure that were of no difference in quality from the figure itself, but they did not seem to belong to the figure. I do not know what that figure is, and don't know that it was marked off" (12 Su 12). "I don't believe this figure was marked off at all except by the black lines, but still the white in the center seemed to suggest a figure" (11 Su. 6). "This figure is hard to describe because I did not have time to get it completed. There was continuous eye-movement over it in an attempt to have it take outline and mean something. But the figure seems to have a rather definite extent" (12 Co 12). "The circle is separated from the background only at the ends of the black lines. In between them it is not marked out. It is as if it were about to come out, but it did not before the curtain dropped" (3 Co 7). "The black strips are so broken that a sort of a circle is formed in the center. But I did not get the outline around the circle where there were no black strips" (2 Sh 3). "I just look at that 'shape,' but I cannot see the outline or any changes to mark off the figure. I just did not know how to finish the figure" (15 St 3). "There is just a square shape of white there, but it is not at all different in quality from the rest" (1 St 6). "As I moved my eyes along the black lines I saw a white 'shape' in the center. It suggested circle, but I had to move my eyes around it to get it to come out definitely" (13 Ca 6). "At first that just looked like a figure of white. It was just a flat shape of white, but was not definitely outlined yet" (8 Z 4). "I saw a perfectly white figure suggested by the black lines. But all the white of the whole field was of the same whiteness" (14 Z 2).

(2) *Meaning.*—"Something about this gives a hint of meaning, but the meaning seems to be indefinite, and I continue to sweep over the figure trying to verify the partially-accepted meaning. There is tension or strain in the throat which means suspended judgment, and I move to verify my judgment" (14 H 7). "The figure-meaning came before the thing became a definite or particular figure" (13 H 7). "At first, when there was just a flat surface and it was not outlined, the figure did not mean much. It did not represent any known object" (18 Su 8). "This was a figure, but it did not have a meaning that I could name. But it meant a figure because there were not just black lines there" (12 Su 4). "It seems to have a definite extent, but is not marked out by the usual lines, and does

not mean anything except a white figure-shape" (12 Co 12). "At first this central area was vague and suggested that it might be circular or slightly elliptical in shape" (2 L 1). "At first that figure was merely a 'design' and took on no particular meaning" (11 St 3). "That shape does not remind me of anything, except that it is just a shape. I cannot name it" (15 St 3). "I saw a pattern of white among the black. I call it a pattern because it is not what I mean by a figure. I don't have any tendency to name this figure and a figure has a name. This is just a white pattern of different parts" (7 D 1).

Phase III. *Process and Meaning.*[14]—"The whole figure became clearer and the surroundings obscure. It also assumed convex perspective" (18 H 6). "The figure became of a different quality and was very bright and had perspective. The rest of the field was quite obscure. It was a beautiful *marble* now" (17 H 8). "Very quickly it popped out very white and the background was quite submerged. The black of the background was quite obscure. The figure looked like a real *crescent* now" (9 H 7). "When these changed to little *balls,* they became whiter and more intense and clearer. Little dark-gray narrow line surrounded them. They changed very quickly" (13 Su 6). "This was a *globe to hold goldfish.* It rounded out and had depth. It meant a real globe instead of the flat figure that preceded" (2 Su 5). "That *electric light-bulb* was of a slightly greenish gray and took on marked depth. Where the black lines do not meet at the top, I extend the greenish gray surface so that I make a tip for the bulb. The tip is the same color as the bulb. The contour of the bulb is very distinct now" (6 Co 4). "On a second look it looked like a *hammock* instead of an oval figure. There was a visible gray outline around it. It had a very definite dividing line" (15 St 2). "Then it meant a *vase* to me. The vase was outlined. The disc which was first suggested was not outlined; but when the figure meant *vase,* there was distinct white outlining it. The surface of the *vase* was gray" (6 Ca 7). "Then the figure stood out at me. It was definitely and completely outlined all around. My images were rather a gray strip just outside the contour of the figure. I named it *battle-axe* then" (11 D 1). "As I looked at it more it appeared to be a *barrel.* It came out in perspective and I saw three hoops of grayish quality running across the *barrel.* This was very clear and looked like a real *barrel"* (3 Z 3). "Soon it changed and looked like a *football.* It was gray, especially on the right side, and the gray surface shaded up the edge of the figure prettily" (4 Z 3).

The general results of this section may be summed up as follows:

1. Partially outlined figures may be completed imaginally by curved as well as by straight contours. The figures thus completed are differentiated from their identically colored background (*a*) by imaginal lines of tied imagery completing the contours, (*b*) by a change in the quality of the figure-surface, (*c*) by an enhanced vividness, and (*d*) by perspective.

2. Three stages in the temporal course of these observations could be distinguished: (*a*) a phase of *expectancy,* wherein *O*

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[14] It seems unwise to separate the descriptions of meaning and process in this phase; but the object-meaning is italicised.

is set for the appearance of a figure; (*b*) a phase of *abstract* form-meaning, which has correlated with it a definite sensory datum (this datum gives merely the suggestion of form; it is not marked off from the background by attributive differences other than vividness); and (*c*) a phase of *concrete* or *objective* meaning, in which *O* labels the figure with a concrete name. A wealth of sensory processes is correlated with this object-meaning; they are subsumed under (*a*) tied imagery, (*b*) qualitative changes, (*c*) change in the distribution of vividness of parts, and (*d*) perspective.

SECTION III

In these experiments a plane-geometrical figure, *e.g.*, a square outlined in black on a large white field, was presented to *O;* and, while the observation was in progress, this figure was gradually changed to another, *e.g.*, an oblong. We hoped to determine the ultimate observational moments characteristic of various plane-geometrical figures.[15]

Six stimulus-cards were used. Five operated upon the principle of elongation: 1 square-oblong, 2 circle-flat sided oval, 3 diamond-hexagon, 4 plus sign-cross, and 5 multiplication sign-double arrow. The sixth card allowed the vertical sides of a square to fall together (at the same time that the upper horizontal side was concealed) to form a triangle. The dimensions of the smaller or unelongated figures were 5.5 cm. in each direction; either dimension could be increased by distances up to 10 cm.

These compound cards were presented in a large picture-frame exposure-apparatus. A door at the back held the cards firmly, but not tightly, against the plate glass. A groove mortised in the top of the frame, flush with the inner side of the plate glass, allowed the hinder card to move upward and downward. The movement was effected at a uniform rate by the pull of a running kymograph. The rate, empirically determined as favorable for the observations, was 10 cm. in 8 sec.

The time-orders were roughly equated; *i.e.*, the elongated figure was shown first in an exposure about as often as the unelongated. The square-oblong figure was presented to all *O*'s on an average of 15 times with each direction of movement; the other figures averaged only 9 times in each direction.

The 7 *O*'s were Ca, Co, D, H, Su, Z and Zi. *O* was seated

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[15] Perhaps Schumann also made the first step in this direction. His characterisations, however, are grossly perceptual and associative. See I, 15 ff.

6 m. from the apparatus; at this distance he could not perceive the line of demarcation of the constituent cards. The work was done in a light optics-room. The following instructions were used throughout: "I shall show you a card on which a figure is represented in black. Observe the figure carefully and, when the curtain drops, report what you have observed." The total exposure-time was 12 sec.; the first figure of an exposure was shown stationary for 2 sec. before the movement (which consumed 8 sec.) began, and the second figure was also shown for 2 sec. in the final position before the exposure terminated.

The main series were taken with the frame in the vertical position. In an extra-series with H the apparatus was turned on its side, so that the square, when elongated, became a horizontal oblong. The same technique and instructions were employed.

Results.—A certain period of training was necessary before the *O*'s fell into the strictly "descriptive" attitude. During the earliest sittings *common-sense* reports were the rule: "The oblong is longer than the square;" "The oblong is longer in one direction while the square is the same in both directions." Very soon, however, the *O*'s realised the inadequacy of such reports, and cast about for other criteria of difference between the figures. The next mode of differentiation came by way of an *empathic* tendency: as the figure was pulled upward, there were reports of sensory experiences of "lightness" or of "expansion;" when the movement was downward, pressure on the head and general "compression" were reported. These experiences all reduced to sensory or imaginal kinaesthesis. One *O* (Su) began with a clean-cut *objective* attitude. The long vertical sides of the oblong appeared, with upward movement, to stretch and became narrower, particularly in the middle region; and with downward movement to be compressed, to thicken and to bulge out.

After a short period of practice the seven *O*'s passed by way of these *common sense, empathic,* or *objective* attitudes to the attitude of strict *psychological observation.* The instructions were not altered in order to attain this attitudinal shift; it is probable that the observations of the preceding section exerted an influence which favored the transition. Two *O*'s, who came into the experiment for this series only, and without the previous experience, never passed beyond the *empathic* or *objective* attitude.

There was a tendency with all *O*'s to report an initial exploratory phase, from which the meaning and the attributive marks of the figure emerged. We merely mention these reports; nothing significant is to be gleaned from them except that any part of the figure may first become clear, and may thus be the starting-point of exploration. At the end of this phase the complete figure is in focus. The direction of exploration was usually from left to right; most commonly it began at the lower left corner of the field, and went upward and to the right.

We now take up the stimuli in order, and report the characterizations of the figures.

Square-vertical oblong.—The square was in practically every case described as attributively alike in all its parts.[16] D reported several times that the corners of the square were the clearest parts; he was, however, not very sure of the observation. The oblong, on the other hand, was characterized by differences in distribution of attributive characters; the longer sides were described by all *O*'s as blacker, clearer and broader. Two, H and Su, also report a higher intensity. The short sides of the oblong were described as grayish, vague, and narrower.

H and Co frequently reported an *empathic* difference between these figures. H stated that the square had a "stability" or "compactness," which he found based upon sensory and imaginal processes in the chest or throat. Co reported a "lack of balance" in the oblong, based upon imagery or sensations of pressure in chest or shoulders. The "objective" attitude of Su to the oblong has already been referred to.

Circle-oval.—The circle was described by all *O*'s as attributively uniform in all parts, while the oval was just as universally characterized as blacker, clearer and more extended on the two longer sides.[17] He also reported empathic "compactness" or "stability" in the circle. At times Su and H reported an *objective* attitude toward the circle. When regarded as object, some arc of the circle, usually that upon which it rested, or if it rolled the side toward which it rolled, was qualitatively blacker, clearer, more extended and more intense. These objective characterisations usually followed in time the observation of the figure under the "form" attitude.

Diamond-hexagon.—For the most part the diamond was described as attributively uniform in all parts. Several *O*'s, however, went further in analysis. Co reported the four points clearer and blacker. For D the two vertical angles were the characteristic parts. Ca reported that the figure divided horizontally, and that the upper half was usually attributively enhanced.

In the hexagon, as in the oblong and the oval, the two long vertical sides were attributively enhanced. Co reported an *empathic* "unbalance" in the hexagon. She remarked several times: "I was afraid it would fall over."

Plus sign-cross.—For the most part the plus sign was described as attributively uniform in all parts.[18] Analysis was carried further in later observations, in which the horizontal arm bore enhanced attributive marks, and the vertical arm was grayish, less vivid, and narrower. Co showed the *empathic* tendency by noting "solidity" or "balance" in the plus sign.

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[16] Schumann reports that in the square all four sides are uniformly combined (*gleichwertig verbunden*); in the oblong, the two longer sides obtrude in consciousness (*hervortreten oder auffallen im Bewusstsein*) and are more closely combined (*inniger verbunden*). See I, 15 ff.

[17] Schumann states that in the circle no parts obtrude, while in the oval the two sides which give it its longer dimension obtrude and combine more closely. See I, 22.

In the cross, the long vertical line was universally described as attributively predominant. Co also reported a "feeling of unbalance."

Multiplication sign-double arrow.—The times-sign[19] was at first described as attributively uniform, and the long vertical line in the arrow was enhanced in all attributive respects. Toward the last, Ca and De reported a step in analysis of the times-sign: the upper or lower half of the figure was enhanced. Ca also reported, as did all the *O*'s before the series ended, that one of the diagonals, usually the lower left-upper right, became blacker, clearer and broader as a whole. D sometimes took an objective attitude toward the arrow, and in these instances described one of the arrow points as attributively heightened.

Square-triangle.—As we have already said, the square was observed as attributively identical in all parts. The triangle, however, was characterized by enhancement of the apex and the two sides forming it.[20] The base line was very commonly described as grayish, and sometimes as quite vague.

Square-horizontal oblong.—H reports that the upper horizontal side assumes attributive predominance over the lower. Sometimes the upper side alone, but usually both of the long horizontal sides, are attributively heightened.

These reports show that, even after the "descriptive" attitude had been assumed, the *O*'s were able to take account only of the more obvious descriptive features of the figures. We therefore had recourse to another mode of presentation.

SECTION IV

By the procedure of Section III we were able to present only one figure, *e.g.*, the square, with its corresponding elongated pattern, *e.g.*, the oblong. We now arranged conditions whereby any figure could be presented with any other, *e.g.*, the circle with the square. We hoped by this variation to enable the *O*'s to carry further the analyses previously begun.

The stimulus-cards carried 12 figures, accurately drawn in black on white: 1 square, 2 horizontal oblong, 3 triangle, 4 vertical oblong, 5 tilted square, 6 circle, 7 oval, 8 plus sign, 9 multiplication sign, 10 horizontal rhombus, 11 horizontal diamond, and 12 vertical diamond. The maximal length of any figure was 3 cm. Figures of the same magnitude in both dimensions, *e.g.,* square and circle, measured 2 cm. in each dimension. The figures were shown, as by the method of paired comparisons, in a Dodge tachistoscope.[21] The distance of the stimulus-card from *O*'s eye was 38 cm. The shift from the first exposure field to the second was affected instantaneously by a swinging pendulum; the brightness of the two fields was sensibly the same.

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[18] Schumann reports that the two arms of the plus sign combine equally and that no parts obtrude. He did not use the cross figure. I, 20.

[19] Schumann observes that any two legs of the times-sign lying symmetrically to the vertical or horizontal, preferably to the former, obtrude and combine more closely. He does not note the enhancement of a diagonal, which Ca first reports and which all *O*'s get in the later work. I, 20.

[20] Schumann notes that the base of the triangle is of less observational significance. He states that the two sides forming the apex of the triangle obtrude and combine to a greater degree of unity. I, 20.

The five *O*'s, Co, H, Su, Z, and Zi, had all taken part in the work of Section III. The following instructions were used throughout: "I shall present to you two figures in succession. You are to observe them carefully, and at the end of the exposure to report in strictly psychological terms what you have observed." The time of exposure was regulated by a silent metronome; a complete swing consumed 2 sec., which was the time between the "ready" signal and the first exposure. Each of the paired figures remained under illumination for 2 sec.

A complete comparison-series comprised 66 observations; a complete series for all *O*'s, 330 observations. We performed three series. The experimental conditions remained identical, except as regards a fixation point. In the first series, there was no fixation mark; after the ready-signal *O* was simply staring into darkness, when suddenly the first figure appeared. In the second series we used two fixation points on every card. These were placed horizontally just outside the boundaries of the figures; they were not perceptible during the period of illumination. In the third series the two fixation points were set vertically. Since the results show no significant differences, we do not distinguish the three series in what follows.

Results.—Certain figures brought out stable and uniform descriptions; most definite were the plus-sign, the oval, and the two oblongs. The square and tilted square proved most baffling, and their analyses were reported with less assurance than those of the other figures.

Square.—This figure was most frequently described, under the form-attitude, as uniform in quality, clearness, extent, and intensity in all parts. When the square took on the "object" meaning, Co stated that the base line was increased in all attributive respects, while H described the base line as more vague, grayish, narrower and less intense; the other three sides were uniform. There were occasional reports of a brief initial stage in which two parallel sides were clear and definite, and the other two very indefinite; in this stage, however, the form-meaning was that, not of a square, but of two parallel lines, with two grayish and vague cross lines unrelated to the figure, and usually out of its plane. There were also early reports in which the square figure had the meaning of oblong; *O* then described two parallels as blacker, clearer and a little wider than the others.[22]

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[21] R. Dodge, *An Improved Exposure Apparatus,* Psychological Bulletin, IV, 1907, 10 ff.

Horizontal oblong.—The longer parallel sides of this figure were described as blacker, clearer, broader and more intense than the shorter sides. These marks usually characterised the lines as wholes, although Su sometimes reported that they were blacker, etc., in the center, and graded off attributively toward the ends; the ends were never so vague, however, as the shorter sides of the figure. For the most part the two more pronounced sides were equally enhanced; but toward the end two *O*'s tended to see the one side slightly predominating over the other. For H the upper side was somewhat more pronounced, while with Co the base line was preferred.

In all the work of this section, the differences of quality and clearness were most obvious. All *O*'s reported changes in extent, but less frequently, and with less definiteness of assertion. Differences of intensity were reported only by Su and H.

Triangle.—The two sides forming the apex were attributively enhanced, and the base line was vague and grayish. Usually the sides were enhanced as wholes, but sometimes the parts about the apex were more pronounced. Su and Co reported occasionally that the base line was blacker and broader: in these cases the meaning of the figure had changed. Su stated that she saw just a bold black horizontal line with grayish legs projecting away from it; the lines formed no definite figure, and did not carry the meaning of triangle. Co also stated that the figure as thus seen did not mean "a good triangle." We shall see presently that these *O*'s show a preference for horizontal lines.

Vertical oblong.—The two longer parallel lines were described as clearer, blacker, broader, and sometimes as more intense than the shorter horizontals. Usually both of the longer lines were equally predominant; toward the end, however, Co tended to report the one or the other, usually the left, as the more pronounced. Usually, again, these lines were enhanced throughout their length, but in a few instances only about two-thirds of their extent was changed.

For H, as we have already seen, the horizontal oblong is dominated by its upper side line; in the vertical oblong the two longer sides are equally enhanced. Co reports a predominance of the base line in the horizontal, and of the left vertical side in the vertical oblong.

Tilted square.—Usually two parallel sides assumed attributive prominence in this figure; frequently the upper or lower, and more rarely the right or left half became more marked; a few reports indicate the lateral angles as the characteristic parts; and a few that the figure is attributively uniform in all parts. Thus on the side of process this figure is variously described, even by the same *O*'s. But a different meaning is correlated with the differences of description. When two parallels, usually the lower left and upper right, were most marked, the *O*'s often remarked that the figure seemed slightly longer in the direction of these parallels; it meant a distorted diamond or parallelogram. With the one half outstanding, the figure meant a diamond, two triangles, or two V's put together. The horizontal division, into an upper and a lower half, was largely preferred. When described as uniform in all parts, the figure meant a square. When the lateral angles with a small portion of the lines forming them were the characteristic parts, the meaning was that of a diamond. In a few reports this last interpretation included an "objective reference;" a tied image, whiter or grayer than the background, then formed an observable link between the angles.

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[22] Schumann found that observers could voluntarily favor any two sides, particularly the verticals of a square; and that, when two sides were thus preferred, they united more closely and the figure was no longer a square but an oblong. II, 16 ff.

Circle.—The circle, under the form-attitude, was reported as uniform in all parts. Frequently the figure took on an "object" meaning; this sometimes followed the "form" meaning, sometimes came at once as the figure was exposed. In these cases, as in our previous work, certain parts of the figure were attributively differentiated: if it meant a rolling hoop, an arc of about 30° on the side toward which it rolled was enhanced; if it meant a standing wheel or hoop, it was more marked along the arc on which it appeared to rest.

The attempt to find an attributive basis for the difference between circle and square proved unavailing. There were a number of reports of an empathic difference: the circle was characterised by a "smoothness," which the square lacked, and the square in distinction had a characteristic "abruptness." These empathic data reduced, so far as the *O*'s could tell, to imagery of eye-movements about the outlines of the corresponding figures.

Two of the regular *O*'s, as well as Dr. Titchener, who observed in this series, reported a tendency for the circle to expand laterally; the arcs on either side were then somewhat enhanced attributively.

Oval.—The two long sides of the oval are attributively enhanced, and the short end-arcs are less marked in all attributive respects. For the most part both long sides are equally enhanced; Co and H, however, noted a tendency for the base and upper side respectively to assume a slight predominance.

All *O*'s differentiated the oval from the oblong by the fact that, in the latter figure, the enhancement extended uniformly the whole length of the longer sides, while in the oval there was a gradation from the most marked attributive increase at the center outward toward the extremes of the figure. The *O*'s were unable to specify a precise point at which enhancement ceased or began. Moreover, most *O*'s declared that the round ends of the oval, even though of a very low degree of clearness, were more essentially characteristic of the oval figure than were the short sides of the oblong for that figure. In other words, the short sides of the oblong were less clear than the round ends of the oval. One *O*, Su, frequently reported that the short sides of the oblong were unobserved, while if the oval was to be apprehended as such the round ends must be perceived.[23] An empathic difference was also noted; the oval was characterised by a "smoothness" and the oblong by an "abruptness." These characterisations again appeared to reduce to imagery of eye-movement.

Plus-sign.—In this figure one of the axes was attributively differentiated from the other. All *O*'s except Z described the horizontal axis as blacker, clearer, and wider. Z reported the vertical axis enhanced, just as constantly as the other *O*'s reported the horizontal. Most *O*'s asserted that the parts of this figure are more obviously differentiated than those of any of the other figures. A very stable attitude and a steady and definite type of report were soon acquired. The differentiation of parts was, indeed, so pronounced that one *O* believed the horizontal line to have been drawn broader and heavier. This statement was made a number of times; and at the close of the work the stimulus-card was accordingly handed to *O* for closer scrutiny. Holding it in the usual position, she exclaimed: "I told you the horizontal line was drawn heavier!" Asked to turn the card 90°, she was amazed to discover that the originally vertical line, which in the normal position was very vague, assumed just as great predominance over its rival as this, the originally horizontal line, had held before.

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[23] Bühler finds that two adjacent sides, usually the left and the upper, are essential parts in the oblong; and that the limen of proportion of rectangularity is not essentially heightened by omission of the right vertical and lower horizontal sides. Bühler, K., *Die Gestaltwahrnehmungen,* i., 1913, 182 ff.

Times sign.—The parts of the times-sign which were characterized by more marked attributive moments were somewhat variable. Most commonly one of the legs, usually the lower left-upper right leg, was attributively enhanced. There were also numerous reports in which the figure divided into an upper and a lower half; usually, one half was then attributively increased. There were a few reports in which the figure divided vertically, and the left half was the more characteristic.

Rhombus.—In this figure either the two parallel horizontal sides, or the obtuse angles with portions of the lines forming them, were the attributively marked parts.[24] All *O*'s reported that, when the horizontal parallels were the marked parts, the correlated meaning was that of parallelogram or rhombus. In this situation the oblique sides were very low in clearness. H again showed his general tendency to give the upper horizontal a slight predominance over the lower; both, however, were heightened more or less. When, on the other hand, the obtuse angles were the heightened parts, the figure-meaning was that of diamond. The shift of meaning with definite alteration in descriptive content was extremely constant in this figure, and was brought out by all *O*'s. Several *O*'s reported a tied image connecting the two angles in the diamond; the image was qualitatively either brighter or darker than the background of white, and played a part only when the figure had assumed an object-reference.

Horizontal diamond.—All *O*'s found a good deal of variation in the diamond-figures, and no one part acquired a steady preference or predominance. However, with the shifts in descriptive content we have a fairly definite correlation of shifts in meaning. The portions of the lines forming the acute angles were very commonly enhanced, and the meaning of the figure was then definitely that of diamond. When this particular content assumed an object-meaning, a tied image often connected the angles. Again, two parallel sides of the figure were often enhanced, when it took on the meaning of a sort of parallelogram. The figure was observed as longer in the direction of the concerned parallel sides. At other times the figure divided horizontally, and the upper half was usually described as attributively increased. The meaning in this case was given as diamond, with frequent specification of two triangles with their bases juxtaposed, or two V's put together, the one of which was of a slightly enhanced attributive status.

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[24] Schumann found no outstanding feature in this figure. He says that, when two sides of the figure are in the horizontal direction, all parts are equally combined, just as in the square. Only the differences in the angles differentiate the rhombus from the square. I, 20.

Vertical diamond.—Here again either the portions of the lines forming the acute angles, or two parallel sides, were attributively heightened. In the former case the figure had a definite and positive diamond-meaning, and tied imagery played its usual rôle when the object-meaning supervened. When two parallels were the outstanding parts, the meaning was that of a sort of parallelogram or distorted diamond. There were also a few reports in which the figure divided into halves, as in the case of the horizontal diamond.

The general outcome of the foregoing work may be summarised as follows:

1. Schumann's general statement that certain parts of plane figures obtrude in consciousness and combine more unitedly than the rest is shown to have an analysable sensory basis. The sensory content assumes a definite pattern of distribution appropriate to particular figure-meanings, and may be described in various attributive respects, *viz.*, quality, clearness, extent and intensity.
2. When a figure has an unmistakable meaning, there is a stable pattern of attributive distribution. When the meaning is variable, there is variability of the enhanced attributive pattern corresponding with the various shifts in meaning. We may thus account for the perception of different forms in the same stimulus, and the perception of the same form in different stimuli. The tilted square, *e.g.*, when observed as a diamond, has the attributive characterization of the diamond; and, when observed as a square, is described as the square.
3. If a figure acquires an object-meaning, there is a change in the perceptual content. Tied imagery often plays a rôle in the object-perception.
4. The figures may be distinguished either psychologically, as just indicated, or empathically. Empathic is akin to naïve or common-sense differentiation, and is consequently reverted to when psychological observation is most difficult.
5. Four of our *O*'s showed a preference for the horizontal direction; the other *O* as strongly preferred the vertical. Hence there are individual differences to be reckoned with in observations of this kind.[25]

SECTION V

Our previous experiments have shown, in various contexts, that the same physical stimulus, under repeated presentation, may set off a variety of meanings, and that the psychological experience underlying the observation of the figure differs as these meanings vary. In the instance of the rhombus, *e.g.*, the meaning either of parallelogram or of diamond might emerge, with corresponding descriptive difference. Moreover, we have found a descriptive difference for the same figure taken under the "form" and the "object" attitudes.

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[25] Schumann implies that both the horizontal and the vertical directions are more important than the oblique; IV, 161 ff. He also remarks that lines which lie symmetrically to the vertical axis are more apt to be grouped together as a unity: I, 18 ff.

These results led us to seek conditions which should allow a still greater flexibility of meaning with observation of one and the same stimulus. We relied especially upon the arousal and influence of tied imagery. We therefore prepared figures whose outlines were indicated by widely discrete white pinheads set on a large black background. The instrument employed was Münsterberg's *Augenmassapparat*.[26]

We performed four experimental series. The first two were very short, and served as a continuation of the work of the preceding sections. In the first series a figure, *e.g.*, a square of 6 cm. side, was indicated by four white pins; the two lower pins were fixed in the black broadcloth, and the two upper were carried on a horizontal rider glued to the threads which stretched vertically in front of the broadcloth screen. By operation of the thumb-screws this figure could be changed, laterally into a rhombus, and vertically into an oblong. In like manner the diamond could be reduced to a triangle and conversely, and the triangle into a straight line. The *O*'s were those of Section IV, and their instructions were unchanged. They sat at a distance of 5 m. from the apparatus.

In the second series the two upper dots of a square, outlined as above, were moved obliquely upward; thus the figure expanded as it lengthened upward, and took the shape of a truncated triangle or wedge. This change was accomplished by means of pulleys placed along the top and bottom edge of the broadcloth screen, and belts of narrow black braid running obliquely around the pulleys. The *O*'s and the conditions of experimentation were as in the first series.

In the third series we adopted a procedure which gave greater scope for multiform meanings with the same stimulus-pattern. We used the two stimulus-patterns suggested by Bühler,[27] the square and tilted square, each indicated by four white dots. During the first sitting the one of these patterns (the tilted square for Co, Su and Z, and the square for H and Zi) was presented for a certain period of time, during which *E* called out in succession the names of various figures which *O* should find outlined by the dots. The figures required in the tilted square were: tilted square, right oblique parallels, plus sign, division sign, left oblique parallels, circle, dotted V, and Z; those demanded in the square were: X, square, N, vertical parallels, Z, O, horizontal parallels, and M. These figures were called for at successive intervals of 4 sec. Under observation, the figures were completed in tied imagery. The instructions read: "I shall expose several uniform white dots, which are regularly arranged on a large black background. I shall then ask you, at rather brief intervals, to observe these dots as forming the framework of a series of figures. It will be your task to observe the figures as called for; and after observing a short series of them to describe each one. If in your subsequent description you are unable to recall the order in which they came, I shall be ready to prompt you."

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[26] See E. B. Titchener, *Experimental Psychology, Instr. Quan. Manual*, 1905, 210.

[27] Bühler, K., *Die Gestaltwahrnehmungen*, I., 1913, 26.

At the next sitting the other pattern was shown (the square to Co, Su and Z, and the tilted square to H and Zi), with the instruction that *O* observe as many different figures as possible. The exposure continued until *O* indicated he could find no more figures. The following instructions were used: "I am going to present several uniform white dots which are regularly arranged on a large black background. Your task will be to observe as many different forms as you can, and every time you observe a new form to call out what it is. The exposure will continue until you are no longer able to get a new form. When the exposure has ended, you are to describe the forms observed as carefully as you can. If you cannot recall the order in which you observed them I shall be ready to prompt you."

In the fourth series we attempted to provide stimulus-patterns which should represent different degrees of difficulty in the suggestion of a meaning. We made a set of eighteen stimulus-patterns, which we supposed would give *O* roughly three orders of difficulty. There were six stimuli in every order, *viz.:* (1) *patterns of concrete objects,* light bulb, bell, fork, coat of arms, pennant, hand mirror; (2) *patterns of stellar constellations,* Cepheus, Orion, Pleiades, Cassiopeia, little dipper, sickle; and (3) *patterns of plane geometrical figures,* in which an *extra* pin was placed so as to disturb or break up any tendency to complete the figure,—triangle (in), triangle (out), square (in horizontal), square (out horizontal), square (in vertical), square (out vertical). The extra dot was placed at random by *E* in any one of the spatial directions indicated. These 18 stimuli were arranged in a single series with regular distribution of type. The time of exposure, determined by

O, was measured by *E* with a stop-watch. The three *O*'s, Co, Su and H, worked under the following instructions: "I shall show you a cluster of white dots on a large black background. These dots are so arranged as to outline a figure. You are to search for the figure and to signal its appearance by saying 'Now.' Then you are to describe, so far as possible in attributive terms, what you have observed."

Results.—In the first series the descriptions of the oblong, rhombus, triangle, etc., show the familiar psychological features. The figures tended to complete their outlines in tied imagery, which was clearer, qualitatively more enhanced, and of more definite limits, in those parts which have already been reported as characteristic of the particular figure. Most commonly this imagery was a dark gray, varying in degrees of qualitative difference from the deep black background. Some *O*'s tended to report the total area inclosed by the dots as either blacker or grayer than the outlying background. In such observations the limits of the qualitative differentiation were more cleanly cut on the psychologically characteristic sides of the figure; the limits on the more vague sides (*e.g.*, the short sides of the oblong) were described as "fuzzy," "stringy," "indefinite," "fading away into vagueness." Thus this series with incompletely outlined figures confirms the results obtained with figures whose outlines were continuously drawn.

In the second series the *O*'s tended to hold rather firmly to the "form" attitude. Several stated that it was not necessary to obtain a concrete meaning, although the more pregnant imagery was correlated with such objective references. The oblique sides of the elongated figure were universally described as characteristic; the top and bottom were seldom definitely outlined. These oblique sides were given (1) by a qualitatively lighter or darker tied image, and (2) by a sharply delineated edge of the qualitatively different surface inclosed by the dots. In such observations the figure was usually named a truncated triangular shape. There were occasional reports in which the figure assumed an object-meaning, when it was named vase or pyramid. With the meaning of vase the bottom of the figure was also closed by a definite imaginal line similar to those forming the oblique sides; the top remained open. With the pyramid-meaning the characteristic oblique imaginal lines projected beyond the white dots outward to a point where they intersected. The entire surface within these oblique limiting lines was qualitatively differentiated from the outlying field of black.

In this series, as we have said, the *O*'s declared that it was unnecessary to assume the object-attitude, and tended more and more to remain in the form-attitude. Since we have previously found a greater richness of attributive characterization and a more prominent rôle of tied imagery under the object-attitude, and since our goal here was variability of meaning with identity of stimulus, we thought it advisable to change the procedure, in order to bring the *O*'s, if possible, to look for the object-meaning.

The significant outcome of the first part of the third series is that, on the demand of *E* for the eight different meanings from the same stimulus-pattern, *O* noted corresponding phenomenal changes in every figure. The imagery of a prior meaning broke up and redistributed itself, thus furnishing a new sensory basis for the new meaning demanded. Eye-movement played a part in determining these shifts of imagery. All *O*'s stated that, where the meaning was more " real " and objective, the imagery was more definite and attributively heightened. Roughly, three degrees of definiteness of imagery were indicated. With all *O*'s the parallel lines called for were reported as of the lowest degree of definiteness, and the meaning was uniformly stated as abstract. Perspective was involved in a few reports in which the figure-meaning was concrete.

In the second phase of the same series the figures observed depended more or less upon the ingenuity of *O* in contriving new figure-meanings. Co reported 10, Su 17, Z 12, H 10 and Zi 14 figures. The descriptions forcibly confirm our conclusion that concreteness of meaning is correlated with heightened imaginal processes. The reports of concrete meanings (cornucopiae, hourglass, hoop, earth-on-axis, cross-in-perspective, wheel, end-of-crystal), showing definite objective reference, are always and for all *O*'s attended by description of the best-marked imagery. Most *O*'s again indicated three degrees of definiteness of imagery; Su and Zi gave four. The accompanying summary shows the figures reported, at the different degrees of definiteness of imagery, by the different *O*'s. It is clear that the more concrete meanings fall in the more definite, and the more abstract figure-meanings in the less marked classes of imagery.

In the last series the ease with which *O* obtained the figure, as measured by his reaction-time, turned out roughly as we had anticipated in the construction of the stimulus-series. The " constellation " and " concrete " types of pattern show in the gross no significant differences, and we have accordingly combined these two groups in the averages. The " interference " group in general brings about a marked delay, and in several cases *O* reported that a figure was impossible. The table shows the reaction-times (in seconds) of all *O*'s for every stimulus, and the meaningful nature of the figures observed.

Co

(1) Cornucopiae
Cross in perspective
Hourglass
Square
(2) Diamond
X
(3) H
M
Z
Square with one diagonal

Su

(1) Hoop
(2) Square
Cross
(3) Plus sign
Z
N
V
X
(4) Vert. parallels
Hor. parallels
Oval
Hexagon
Tilted division sign

Z

(1) Square
Earth on axis
M
Hoop
(2) Z
X
N
(3) Hor. parallels
Vert. parallels
Rt. angled triangle
Tilted division sign
Tilted T

H

(1) Square
Cross
Black rectangle in perspective
(2) N
Z
O
Diamond with horizontal bisection
(3) 2 V's
2 pairs of short parallels converging left

Zi

(1) Diamond
Wheel
End of pointed crystal
Cross
Circle
N
(2) Oval
Z
Diamond with horizontal bisection
(3) Dotted V
Plus sign inside tilted square
Division sign
(4) 2 pairs of short parallels converging left

	Co	Su	H	Co	Su	H
1. Cepheus	15.2	24.	6.4	Curved globe	2 △'s, apices together	Crescent
2. Light bulb	9.4	14.4	5.	Tiny Christmas tree with lights	Oval on △ base	Hanging chandelier
3. Triangle (In)	17.	27.2	33.8	Tent	Double V	Kite
4. Orion	14.2	27.	6.2	Dog sitting on hind leg	Light globe on base	? with legs as in funny papers
5. Bell	15.	12.6	3.8	△ with cross line at top	Cross with support behind	Tent or pagoda
6. Square (In H)	22.4	18.2	9.4	Pyramid with apex toward me	Pyramid	Cylinder in perspective
7. Pleiades	11.8	21.8	4.8	Diminutive tennis racket	A sort of dipper	Dipper
8. Fork	7.2	2.8	1.8	Shovel	Banjo	Broom

9. Square (Out V)	18.2	33.4	12.2	An open fan	FAIL (Triangle)[28]	Balloon
10. Cassiopeia	4.8	11.	2.8	Butterfly	Tipped W	W
11. Coat of arms	9.	16.6	33.2	Small fruit tree	Leather bag	Fish bowl
12. Triangle (Out)	90.	16.	14.4	FAIL (geom. figure)[28]	Drawing instrument	Coal scuttle
13. Little dipper	12.2	10.6	5.4	A flower	Dipper for pail	Kite
14. Pennant	43.2	7.6	7.2	A flag or pennant	Flag on pole	Lever & fulcrum
15. Square (Out H)	59.4	65.	7.6	Profile of face	FAIL (Triangle)[28]	Inverted chandelier
16. Sickle (Const.)	46.8	50.4	24.8	Question mark	Standard with hook light bulb	Hay hook
17. Hand mirror	16.	17.4	2.4	Ring with link	Circle with handle	Head calipers
18. Square (In V)	32.6	14.6	17.6	Pyramid with top toward me	Square with dented top	Hollow cup or can
Average Constell. Concrete	17.06	18.01	8.65			
Average Interference	39.93	29.06	15.83			

The *O*'s again reported the "form stage," during which the figure was indefinitely outlined and carried only a suggestive or tentative meaning. By eye-movement about its outline, or by fixation, the figure either completed itself processwise and was accepted with a more concrete meaning, or failed to take on a greater definiteness and was rejected. All *O*'s were set for a fairly concrete figure-meaning, and declared that the object-meaning, which came subsequently, was a confirmation of the suggested or tentatively accepted form-meaning.

The reaction-times throw further light upon the influence of meaning in determining the perception of form. Meaning and process are correlated aspects of the perception, and until both are somehow realized the perception is inadequate. The rôle of meaning is also brought into marked relief by the work with constellation-figures. In presenting these figures as arrangements of white dots upon a deep black background, we offered admirable external conditions for the suggestion of stellar constellations. But not a single *O* observed such a figure, because no *O* was set for such a meaning. One *O* called the "Little Dipper" a dipper, but described it as an ordinary utensil for a water-pail.

CONCLUSIONS

The net outcome of our experimental work is that the perception of visual form presents a dual problem of psychology and of applied logic. Taking the perception psychologically, and attempting to describe the content-processes into which it resolves, we have been able to bring to light a variety of attributive patterns which serve as sensory cues to meaning. Taking the same perception logically, we have found a number of form-meanings, definitely correlated with these sensory cues. We have thus shown how identical stimuli, approached in different sets or attitudes, may arouse the perception of different forms, correlated with differing sensory emphases; and we have shown how different stimuli may arouse the same perception of form correlated with sameness of sensory emphasis.

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[28] The name here was suggested, but rejected because it did not include all of the dots.

For our modes of procedure we claim two advantages. In the first place, the method which we have followed has proved unexpectedly fertile on the side of process, and the resulting descriptions are detailed; and in the second place our discrimination of meanings is itself a small contribution towards a classification of the sort outlined in Weld's recent study.[29]

We conclude from our experiments that the *Gestaltqualität* is a particular meaning established, under a general or particular set, upon particular and definitely correlated sensory data. If this reading is correct, the need of a new ideational content or character of relation or production is done away with; perception appears as an equivocal term, but with the gross reason for its equivocalness fully exposed.

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[29] Weld, H. P., "Meaning and Process as distinguished by the Reaction Method," in *Studies in Psychology* (Titchener Commemorative Volume), 1917, 181 ff.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener, H. P. Weld and K. M. Dallenbach

XLVIII. A Note on the Series of Blacks, Greys, and Whites

By F. L. Dimmick

G. E. Müller's exposition of the four color-series as "series of psychical qualities" has been generally accepted, and the arrangement of the series in a quadrilateral, which forms the base of the color-pyramid, is shown in many text-books. Müller, however, as is well known, regards the black-white series as a fifth qualitative series of the same order; and he holds to this view despite the antagonism of the black and white processes which is apparent in the effects of after-image and contrast. Further, this single series shows two lines of intensities between its terminal qualities, seeing that intensity increases in both directions outward from the middle grey.[1]

It may, however, be doubted whether, in the case of the black and white series, the last word of observation has yet been said. If this series is compared with a double series extending from blue through grey to the antagonistic yellow, or from green through grey to the antagonistic purple, a likeness emerges which, although masked by the achromatic quality of the blacks, greys, and whites, becomes more and more pronounced as the three groups are longer studied. Indeed, it is the purpose of this Note to suggest that the black-white series is, in reality, made up, not of a single qualitative series in Müller's sense, but rather of *two series,* which join each other at an angle of 180°. The one of these series runs from white to grey through the light or whitish greys, the other runs from black to grey through the dark or blackish greys. In a light grey there is a likeness to (or a reminder of) both white and grey; there is no hint of black. Similarly, in a dark grey there is resemblance to (or reminder of) both black and grey; there is not the least hint or trace of white. If once grey has established itself to observation as an independent quality, the terminal quality of qualitative series, then the continuity of the black-white line is immediately resolved, and the two component series, black-grey and white-grey, stand out as clearly as the several chromatic lines which together make up the basal plane of the color-pyramid.

We have sought to illustrate this relation of grey to black and white as follows. In preliminary work, the limits were determined of that range of middle greys which, under our conditions, was never transgressed by the observers as they watched white disappear from the light greys and black disappear from the dark greys. The center of this range was taken as neutral grey; the determination is, of course, only a rough approximation to fact. With this neutral grey the various colors which stand at the angles of the base of the color-pyramid, as well as black and white, were mixed in varying amounts on a Marbe color-mixer. The method of limits was used, and five series each of increasing and decreasing amounts of red, green, blue, yellow, black, and white were presented. The observer was given a sample of the hue or black or white which was added to or subtracted from the mixture, and was asked to say whether he saw any trace of this sample quality in the mixture. The averaged results may be regarded as rough chromatic limens. It is plain that the average values and their mean variations are all, despite the unfamiliarity of the task in the cases of black and white, of the same order of magnitude.

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[1] Zur Psychophysik der Gesichtsempfindungen, *Zeit. f. Pyschol. u. Physiol. d. Sinnes.,* X., 1896, 32 f.

Obs.	H		S		T		W	
Colours	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
Red	13.9	1.18	15.7	1.2	12.	.9	14.4	1.2
Green	14.1	.72	16.6	1.1	12.7	1.04	13.9	1.1
Blue	11.6	1.28	12.9	.94	12.5	1.0	12.8	1.2
Yellow	12.9	.72	13.3	.96	14.3	1.3	15.6	.98
Black	13.6	1.8	17.1	1.32	14.3	1.44	14.6	1.3
White	13.8	1.56	17.9	1.3	14.6	1.2	15.1	1.1

Middle grey=300° Black+60° White.

Black=Hering Velvet Black; White=Hering Baryta White; Colors =Milton Bradley papers: Red, tint 1; Bluegreen; Blue; Yellow. Unit of variation=1.5°; unit of table=1°.

Observers: Dr. L. B. Hoisington, instructor in psychology; Miss A. Sullivan, fellow; Mr. S. Takaki and Mrs. A. K. Whitchurch, graduate students in psychology.

The conception of the blacks, greys, and whites as forming two qualitative series placed end to end, affords a clarification of theory. The color-pyramid stands as before, but its constituent qualitative series receive a new interpretation. The lines that converge upon central or neutral grey, from red, yellow, green, blue, black and white, are now of the same kind, chromatic or saturational series. Heretofore, in order to account for the anomalous relation of black and white, as, at the same time, acting antagonistically in after-image and contrast and mixing to give intermediates, it has been necessary to postulate for them an unique type of qualitative series, composed psychophysically of three factors. No other example of this sort of series occurs, to our knowledge, in psychology.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY[1]

Communicated by EDWIN G. BORING

XXIII. THE SPATIAL CONDITION OF THE FUSION OF WARMTH AND COLD IN HEAT

By J. HENRY ALSTON

Heat, an introspectively unique quality, is conditioned upon the simultaneous stimulation of adjacent warm and cold spots. This theory derives from Alrutz, who obtained heat, a simple and unique quality, from a warm areal stimulus, adequate both to warmth and to paradoxical cold. Cutolo recently verified this conclusion with a more satisfactory technique. He stimulated an isolated cold spot with a cold stimulus, and simultaneously an adjacent warm spot with a warm stimulus of a temperature adequate to warmth but not to heat or paradoxical cold. In this manner he obtained qualitatively unique heats, and demonstrated further that heat actually does depend on the simultaneous excitation of cold and warmth, and not on the intensity of the stimulus. Neither stimulus of his cold-warm pair was alone adequate to heat; but simultaneously, when the cold stimulus was applied to the cold spot and the warm stimulus to the warm spot, they aroused the heat-quality.[2]

The phenomenon of heat bears directly upon physiological theory. If a unique quality, like heat, depends solely upon the coexcitation of two sensory spots that contribute ordinarily to different modalities of quality, then it is plain that the ascription of four modalities—pressure, pain, warmth, and cold—to the skin is psychologically inadequate, and that the concurrent assumption of four specific nerve energies is physiologically unsatisfactory. Whether or not ultimately we find the skin supplied with four separate afferent systems, we now know that we should not be able to regard such systems as acting in mutual independence; the completed psychology of the skin must take account of the interrelation of the excitations arising from the separate sensory spots. In the case of heat we already know that it depends upon the concurrent stimulation of adjacent warm and cold spots; hence we are ready to inquire further concerning the degree of adjacence that is necessary in order that a warm and a cold spot may act concurrently in the arousal of heat. With this question the present paper deals. If we find that excitations of warm and cold spots, remote from each other, may nevertheless fuse physiologically in yielding a simple heat, then we may infer an extensive interrelation of the thermal mechanisms and may perhaps guess even that the fusion is not peripheral.[3]

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[1] In reviving with this paper Professor Sanford's series of Minor Studies from the Psychological Laboratory of Clark University the present director is sensible of the scientific standard that determined the publication of the first twenty-two studies and confesses to an ambition that the revived series may not fall below the ideal of its founder.

[2] F. Cutolo, A Preliminary Study of the Psychology of Heat, this JOURNAL 29, 1918, 442-448. Cutolo's historical orientation of the problem of heat, *q. v.*, is too recent to require repetition.

Our observers in this experiment were Dr. E. G. Boring (B), who was acquainted with the literature of the subject, who was highly trained in cutaneous introspection,[4] and had already served as observer in Cutolo's experiment; Mr. C. C. Pratt (P) and Mr. M. Yokoyama (Y), who were untrained in cutaneous observation but had had considerable general training in introspection.

In preliminary experiments P and Y were trained to recognize the quality of heat by the use of a heat-grill, patterned after Cutolo's.[5] B found that his familiarity with the nature of heat held over from his previous observation in Cutolo's experiment.

In the experiment proper we worked entirely with the punctiform stimulation of a two-point thermaesthesiometer.[6] Our instrument was constructed of two hollow pointed brass cylinders, through which water might circulate and which slid upon a graduated bar in such fashion that they could be set with their points at different distances apart. Each cylinder was 7.5 cm. in length, 1.2 cm. outside diam., and tapered to a point about 0.5 mm. diam. These cylinders were set at such an angle that the surface of their conical points nearest the skin made an angle of about 60° with the skin. One cylinder was connected with the supply of warm water and the other with the cold water faucet, in order that the instrument could be used to stimulate a cold spot with cold and a warm spot with warmth simultaneously. The warm water supply was kept at a temperature of 43-44° C. The supply-tub held about fifteen gallons and, with room temperature 20-22° C, varied less than 1° C in an hour. The cold water from the faucet during our period of experimentation varied between 5° and 10° C. The cold faucet and the tank of warm water were connected with the aesthesiometer through rubber tubing ⅛ in. inside diam. Each stimulus-cylinder of the aesthesiometer was provided with an inlet and an outlet, consisting of smaller tubes let into the side. The inlet was set at an angle of 45° with the side of the cylinder and directed toward its point; in this way we hoped to secure a good circulation at the stimulating point. The outlet emerged from the cylinder near the end remote from the point. The waste water from both outlets was led off through a Y-connection to a large rubber tube. The flow of water through each cylinder ranged from 1,000 to 1,200 cc. per min.

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[3] It is not novel to consider the skin as a single sensory system of organs. Before Blix and Goldscheider, working under the inspiration of the theory of specific energies of nerves, had posited the four separate modalities, we had the *Gemeingefühl,* common sensibility; and after differentiation of tactile sensibility had begun there persisted the theory of pain as common sensation and Hering's theory of a single temperature sense. More recently there is Head and Rivers' conception of the epicritic and protopathic cutaneous mechanisms that cut across the orthodox modalities (*Brain* 28, 1905, 99ff.; 31, 1908, 323 ff.), and Titchener's suggestion of qualitative continua within and between the modalities of pressure and pain. (This JOURNAL, 31, 1920, 213 f.)

[4] *Cf.* E. G. Boring, Cutaneous Sensation After Nerve-Division, *Quart. J. Exper. Physiol.* 10, 1916, 1-95; esp. note, p. 4.

[5] *Op. cit.,* 446.

[6] Cutolo used the Zimmermann thermaesthesiometer, see Zimmermann *Liste 25,* 1912, Nr. 950.

We worked entirely on the volar right forearm. The skin was laid out by a rubber stamp, 4x6 cm., divided into small coördinate squares, each 2 mm. on a side. The small separations of the stimuli fell within a single impression of this stamp; the wider separations required two or three adjacent impressions of the stamp upon the arm. The small separations were applied always in the middle of the volar surface and were longitudinally, transversely or diagonally directed according as the relative positions of the temperature spots determined. The greatest separations involved the greater part of the forearm and were therefore mainly longitudinal.

At every session, after stamping the arm, we mapped first for warm spots, which were the less numerous, and then for cold spots. We used the points of the thermaesthesiometer as stimuli in mapping. Since we did not obtain heat from the stimuli used separately in mapping, we were reasonably sure that heat, when it appeared for simultaneous stimulation, must be due to the concurrence of the two stimuli. Occasionally, in mapping, the warm stimulus did give rise to heat at some single spot; such spots were discarded.

After mapping, which usually required about twenty minutes, the experimenter proceeded to apply the dual stimulus,—a warm point to a warm spot and a cold point to a cold spot. The stimulus was put down with great care and every effort was made to place the two points upon the skin simultaneously. As a matter of fact, however, simultaneity of application has not the importance here that it has in determining the limen of dual impression for pressure. The tactual experience of the points is always felt an appreciable time before any thermal qualities arise. It is felt as "two," as "one," or as some intermediate pattern, depending presumably on the distance between the points. Most frequently the cold sensation arises first after the tactual impression and is followed later by a more complex perception (of warmth, or of heat, or of some concomitance of warmth, cold and heat).

The experimenter continued the application of the stimuli, until the observer reported a thermal quality or pattern, or until he was assured that no such pattern would appear. In most cases an application of ten seconds was sufficient to include the entire thermal experience.

After the removal of the stimulus the observer wrote an introspective account of the thermal perception.

Frequencies of Occurrence of Heat

In determining the relative frequencies of occurrence of the heat sensation and other thermal complexes, we have discarded entirely all trials in which no temperature at all was felt (heat, warmth or cold), in which cold was felt alone without heat or warmth, and in which warmth was felt alone without heat or cold. In other words, the term "total trials" (*cf.* Table I) is to be taken as the total of all trials in which heat occurred, whether alone or with warmth or with cold or with both, and of all trials in which warmth and cold occurred together without heat. All laboratory experience indicates that it is only under usual conditions of technique and for especially responsive spots that the method of stimulating a temperature spot with a metal point invariably gives a thermal response. When we failed, therefore, to get either heat or both cold and warmth as the psychological evidence of simultaneous adequate cold and warm stimulation, we could not tell whether we were dealing with a failure of technique or whether we might actually be concerned with the case of inhibition of cold by warmth, or the converse, or, in the case where no thermal quality was felt, with a mutual inhibition. Our study, therefore, throws no light on the existence or nonexistence of inhibiting effects between warmth and cold. We may note, however, that these apparent failures of technique were not numerous. B failed in this way in 12% of all trials; P in only 6%; and Y in 4%.

TABLE I

Number of cases in which (1) a heat and (2) a simultaneous cold and warmth occurred for different separations of the aesthesiometer points. The percentages are percentages of the total number of trials reported by each observer (B, P and Y) for the given separation. The frequencies for heat include every case where heat was reported, whether alone or with cold or with warmth or with both. Cases occurring at the limit of a class-interval are counted in the upper group; *i.e.* 4 cm. is included in the group "4-6 cm.", and "2-4 cm." means "2 or greater but less than 4."

	Separation	Number of Cases			Per cent of Total Trials		
	cm.	B	P	Y	B	P	Y
1. Heat	0–2	38	32	54	.76	.89	.92
	2–4	34	21	23	.85	.81	.88
	4–6	27	12	20	.80	.86	.77
	6–8	19	21	15	.66	.78	.79
	8–10	18	6	9	.60	1.00	.43
	10–12	12	25	0	.34	.81	0
	12–15	9	12		.23	.63	
	15–18	2	2		.14	.29	
	18–20	0	0		0	0	
	Total heats	159	131	121	.58	.78	.73
2. Simultaneous cold and warmth	0–2	12	4	5	.24	.11	.08
	2–4	6	5	3	.15	.19	.12
	4–6	8	2	6	.20	.14	.23
	6–8	10	6	4	.34	.22	.21
	8–10	12	0	12	.40	0	.57
	10–12	23	6	14	.66	.19	1.00
	12–15	30	7		.77	.37	
	15–18	12	5		.86	.71	
	18–20	5	3		1.00	1.00	
	Total cold and warmth	116	38	44	.42	.22	.27
Total trials		275	169	165	1.00	1.00	1.00

The principal result of our study is shown in Table I. This table shows the occurrence of heat as a function of the separation between

TABLE II

Number of cases in which were reported (1) a heat with a simultaneous cold, (2) a heat with a simultaneous warmth, (3) a heat with cold and warmth simultaneous, (4) a heat with cold or warmth or both cold and warmth simultaneous, and (5) a heat without any thermal concomitants. (4) includes all cases separately given in (1), (2), and (3). The percentages are percentages of the total number of heats reported by each observer for the given separation; see (1) in Table I. The class-intervals of separations are defined in the legend of Table I. Here class-intervals for which no cases occur are omitted from the table.

	Separation	Number of Cases			Per cent of Total Heats		
	cm.	B	P	Y	B	P	Y
1. Heat and cold concomitant	0–2	8		3	.17		.06
	2–4	10		2	.23		.08
	4–6	4		2	.13		.10
	6–8	4		4	.19		.03
	8–10	0		0	0		0
	10-12	2			.14		
	Total	28		11	.18		.09
2. Heat and warmth concomitant	0–2	3			.07		
	2–4	3		1	.08		.04
	4–6	2	1		.07	.08	
	6–8	4			.17		
	8–10	4		1	.18		.10
	10–12	2	1		.16	.04	
	12–15	2			.18		
	Total	20	2	2	.13	.02	.02
3. Heat, cold and warmth concomitant	0–2	1		1	.03		.02
	2–4			1	0		.04
	4–6	2		1	.07		.05
	6–8	2	1	4	.10	.05	.17
	8–10	3		1	.14		.06
	10–12	4			.25		
	Total	12	1	8	.07	.01	.08
4. Heat concomitant with cold or warmth or both	0–2	12		4	.32		.04
	2–4	13		4	.38		.17
	4–6	8	1	3	.30	.08	.15
	6–8	10	1	8	.53	.05	.53
	8–10	7		2	.39		.17
	10–12	8	1		.67		.04
	12–15	2			.22		
	Total	60	3	21	.38	.03	.19
5. Heat without thermal concomitants	Total	99	128	100	.62	.97	.81

the warm and cold spots which unite in the production of heat. A general trend is clear, although the columns of percentages fail to present uniform functions because of the fewness of the cases. When the stimulus-points are close together, heat is usually aroused although cases of simultaneous warmth and cold do occur for spots less than 2 cm. apart. As the separation between points is increased the heats become sooner or later less numerous and finally at a critical distance fall off rapidly in frequency. We find a maximal distance beyond which—in our few cases at least—no heats occurred. Y never felt heat for separations greater than 10 cm., and B and P never felt it for separations greater than 18 cm.[7] Although we did not determine for P and Y the limen of dual impression for pressure, it presumably should be about 3 cm. in the longitudinal direction.[8] B's limen for the corresponding region of the other arm is almost exactly 3 cm.[9] Thus it appears that warmth and cold may fuse into a qualitatively simple heat when the contributing stimuli are separated by a distance five times as great as the limen of dual impression for pressure.

In Table II we have noted the frequencies with which heat was reported as occurring concomitantly with cold or warmth or both. It will be noted that B reports such concomitants more frequently than Y (B 38%; Y 19%) and that P reports almost no concomitants at all (3%). It seems probable that these numerical differences are attributable to differences of attitude and training of the observers. B was highly trained in the observation of cutaneous quality and bore a critical attitude toward the experience of heat. He gave much more complete introspective analyses than P or Y. P on the other hand seems to have taken the attitude of searching for thermal *Merkzeichen;* in other words, he sought for typical and univocal thermal qualities. In the preliminary mapping for temperature spots, it was always more difficult to determine both the cold and the warm spots with P than with the other observers, a difficulty due, at least in part, to P's reluctance to make a positive report when he lacked the full realization of the *Merkzeichen.*

In view of these discrepancies and of the fewness of the cases, it is not possible to state whether the occurrence of concomitants is a function of the separation between the stimuli or not. B's results (*cf.* the fourth section of Table II) suggest that warmth or cold is simultaneously distinguishable from heat more often when the separations are large; *i. e.,* the appearance of warmth or cold, concomitant with the heat, gives promise of the disintegration of heat into warmth and cold that occurs at still greater separations.

Cold seems to appear as a concomitant with heat more often than warmth, and cold and warmth together appear least often as concomitants. The cases are again, however, too few for positive generalization.

It should be noted that we have not excluded the possibility that concomitance means attentive analysis. If heat were sometimes to break up under attention into warmth and cold, it would undoubtedly often be reported as concomitant with the warmth or cold or both.

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[7] Y is 59 in. in height; B is 67 in.; P is 72 in. Czermak, supposing a constant number of cutaneous nerve-endings, held that children and small persons had a finer *Raumsinn* than large persons; *cf.* V. Henri, *Raumwahrnehmungen des Tastsinnes,* 1898, 38 f.

[8] *Cf.* C. L. Friedline, this JOURNAL 29, 1918, 404.

[9] Boring, *op. cit.,* 23.

It is peculiarly difficult for an observer to say, when attention turns from the heat to the warmth or cold, whether the heat lapses or whether it simply drops from the attentive focus to the margin. In the former case there would presumably be an alternation; in the latter there would be a true concomitance.

The fifth section of Table II gives the frequencies of the "pure" heats that occur without any thermal concomitants. The frequency of the "pure" heats must decrease slightly with increasing separation, if we take the fourth section of Table II to mean that the concomitants—the "impure" heats—increase with the separation.

Psychology of Heat

Although we intended the introspections of our observers solely as a qualitative report upon our dual stimuli, we find in them considerable incidental material, which bears on the nature of localization of the heat and its spatial characteristics, upon the existence of fusion in heat or with heat, and upon the qualitative nature of heat itself. The reports make interesting psychological reading but limits of space forbid extended quotation from them. They are given in full in a bound manuscript report of this study which has been placed in the Clark University Library.[10]

Spatial Characteristics of Heat. Psychologically these heats, conditioned upon dual stimuli, are hard to localize although they have definite intrinsic spatial characteristics. B sought definitely to make a localization and ordinarily succeeded. P stated in his early trials that heat was unlocalizable but later indicated a number of spatial references. Y always had difficulty. Both P and Y repeatedly mention heats that are "unlocalizable." Such heats often occur in the presence of two definite pressures and yet can not be placed with respect to the pressures. The observers had, of course, no instruction to localize; nevertheless all three sought at times to do it. B, who was working with knowledge, undoubtedly sought to make the localizations because of their significance for the problem. His success may have been due to his greater effort and to his training in localization of organic processes.[11] Y sometimes, in his effort to localize heat in relation to the pressure-pattern on his arm, ended by locating the heat outside his body, altogether detached from the arm: "The heat was somewhere outside my arm" or "the heat was nowhere or detached from the body, and it was very small."

Heat is referred to a single place on the skin. In 411 heats obtained by our method of dual stimulation there were only three mentions of a "double heat." When heat is localized intentionally (B's reports) it may assume a definite relation to the spatial pattern of pressures.

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[10] The Department of Experimental Psychology of Clark University proposes, in the case of studies where it is impossible to print detailed introspections or to give other data in full, to file a complete report as a bound manuscript in the Clark University Library. This report can be borrowed under the rules of the Library or loaned to other libraries under the usual courtesies of exchange. When a manuscript copy is a thesis and cannot be withdrawn, the Department proposes to deposit also a duplicate copy which may be allowed to circulate. Complete records that would be of value to other investigators are thus made available, while undue pressure upon the limited space in the psychological journals is avoided.

[11] *Cf.* Boring, The Sensations of the Alimentary Canal, this JOURNAL, 26, 1915, 1-57, esp. 38 f.

B found under such conditions that the heat was usually felt between the two pressures or connecting them. He reports: "Sometimes I localize heat as lines connecting the two pressures;" "localization is hard, and when I force it the heat gets itself localized as strands between the pressures;" "the heat stretches between the two points;" "the cold is localized above and the warmth below with the heat connecting them;" [13 such reports]. B also notes that the heat may occur between the two spots without connecting them: "The heat is near the upper pressure but displaced from it in the direction of the lower pressure;" "the heat lies in between the pressures of the two points where the warmth and cold are left respectively;" "the thermal complex lies in between the pressures, stretching toward them but not connecting them;" "by mistake I opened my eyes and could 'see' the heat arising on my arm midway between the two stimuli;" [10 such reports]. Occasionally, when the stimuli are close together, B finds the heat large and including the two pressures.

P and Y do not discover heats connecting the pressures or lying between them. Since localization is difficult, we may be inclined to attribute B's placing of heat in a position symmetrical to the two pressures to his effort to achieve a localization. On the other hand, P and Y occasionally do make localizations with respect to the pressures and always then place heat at one or the other spot. In these cases P always referred the heat to the cold spot [8 cases] and Y to the warm spot [4 cases].

None of the observers had difficulty in localizing warmth or cold with respect to the pressures or with respect to each other; and there are numerous cases in which an observer referred heat to a different spot from a concomitant cold or warmth. The relative localization of concomitant warmth and cold longitudinally upon the arm is nearly always objectively correct.

B and P always referred heat to the surface of the skin. Y uniformly reported it as "tridimensional," "penetrating," and as extending beneath the skin or entirely subcutaneous. The only exceptions to this rule are Y's report of heats detached from the body. Perhaps Y's subcutaneous reference of heat, like his report of "detached" heat, is due to his difficulty in localizing it. To report heat as detached from the body or as subcutaneous is to separate it spatially from the skin and thus from the impressions of pressure, warmth and cold, which are referred to the skin and which Y finds it difficult to bring into local relation to heat.

It appears that heat may be referred to the same region as a simultaneous warmth or to a different region, and to the same region as a simultaneous cold or to a different region. In exceptional cases concurrent warmth, cold and heat may all three be referred to the same region, or, as we have seen, may each be localized in a different place. We have no positive statement to make of the psychological nature of a heat that coincides with a warmth or a cold. One might expect that two qualities, temporally and spatially coincident, would necessarily fuse more or less perfectly after the manner of tones; and we shall see presently that fusions may occur.

Although difficult to localize, heat, nevertheless, appears in a definite spatial pattern. It may be clear-cut or diffuse, small or large, round or oval, even though the observer has difficulty in placing it with respect to other simultaneous sensory impressions or with respect to the habitual points of reference that he uses in localizing upon the arm. It is probable, as we have seen in B's case, that the effort to localize influences the spatial pattern. B, who alone succeeded often in localizing, alone reports heat as large, diffuse or radiating. P and Y nearly always describe heat as small, clear-cut, and sharply defined. B also notes at times the occurrence of clear-cut small heats. We have shown above for B that a heat that connects two pressures or lies between them is diffuse or large, or is visualized as an oval or as strands connecting two pressures. The typical heat, however, is to be described as "very small," "smaller than cold," "1 mm. diam.," "a tiny patch," "a single point about 5 mm. diam.," "pointed," "clear-cut," "sharply defined," "narrowed down," "much smaller than warmth." A few reports mention heat as a tiny spot lying within a larger area of warmth.

Quality of Heat. Although our three observers agree that heat is a unique quality, they have not succeeded well in characterizing it. B, who had described it in Cutolo's experiment as "a quality lying between pressure and pain," continued in this conviction: "Heat is just the pressure-pain thing," "a drawing sensation similar to pressure," "normally heat is a simple pressure-sting," "heat was pressure-like in the way that ache is," "a pressure-aching quality developed into heat." Y laid especial emphasis upon the "brightness" of heat and B in certain reports agrees with him. By Y the appearance of "brightness" is repeatedly identified with the appearance of heat. P's characteristic adjective is "sharp;" he seems to mean by "sharp" something qualitative and not merely the spatial "pointedness" of heat.

Fusion. Physiologically heat is a fusion of the excitations normal to warmth and cold. We may ask, therefore, whether or not we find introspective evidence of this physiological fusion. The answer is that there are plenty of cases in which a simultaneous warmth and cold are observed to give place to ("fuse into") heat or in which a heat breaks up into a warmth and cold. The alternation of heat with warmth-and-cold indicates that in certain critical cases the physiological fusion may be instable.

Such phenomena, however, are not in themselves psychological fusions: they do not demonstrate a warmth and cold coexistent in heat. That heat may break up into warmth and cold may be due to a physiological instability or, on the other hand, it may be the result of an effort of attention to analyze it. These two cases are not necessarily distinct, for if the alternation is attentively conditioned it may constitute a case of analysis that is ordinarily taken as an evidence of psychological fusion. It would be crucial to determine whether the heat lapsed entirely in giving place to warmth and cold or merely became marginal, and whether, conversely, a marginal cold and warmth are to be discovered while heat is still focal. We are unable on the basis of our introspections to reach a conclusion here. P and Y never indicated fusions that could be taken to be psychological. B has but nine reports of a psychological fusion of warmth and cold in heat, and they do not indicate the exact nature of the concomitance: "A faint warmth and a faint cold were well fused, but not perfectly fused, into heat;" "there may be warmth and cold, or warmth or cold, in the heat, a very good fusion but perhaps not perfect;" "heat, warmth and cold are simultaneous, but I can't attend to the heat and to the warm-cold at once;" "a poor heat in which the cold and warmth were not entirely lost though they became less prominent;" "cold-warmth or heat, an imperfect fusion." B has a great deal to say of the difficulty of attending to cold-warmth and to heat simultaneously when the possibility of attention to all is present. He seems to take this situation as indicative of fusion. Y noted warmth and cold as a back-ground to heat [once], heat as a back-ground to warmth and cold [twice], and a simultaneous heat, warmth and cold, which he expressly denied as fusion [once]. P never indicated a fusion.

Cutolo remarks that heat may apparently form an imperfect psychological fusion with cold or with warmth alone, and our results are partially consistent with this conclusion. Most of the observations of such fusions are, however, for B, who was also Cutolo's observer. B has fourteen reports of a fusion of heat with cold and one only of a fusion of heat with warmth. P and Y do not unequivocally indicate these fusions. If such fusions normally occur in the heat-experience, they would indicate that the physiological fusion is partially but not completely represented in the psychological experience.

Heat and Cold. There is some indication that heat is more intimately related to cold than to warmth. We find (1) that heat and cold are both characteristically "bright." (2) B sometimes noted a pressure-like component in both heat and cold. (3) All observers agree that there is a qualitative similarity in some respect between heat and cold, although they also insist that the two sensations as totalities are notably different. (4) Heat and cold are alike in spatial pattern and (5) in temporal course. Both are small, clear-cut, and well defined, and both come in abruptly. Warmth, on the other hand, is typically diffuse, and indefinite in its initiation. (6) Cold is characteristically referred to the surface of the skin and warmth characteristically seems to well up from beneath the skin. B and P insist upon a superficial reference for heat, although Y refers heat beneath the skin. (7) Heat fuses psychologically more often with cold than with warmth [B's reports], and (8) seems more often to be referred to the cold spot [B's and P's reports] than to the warm spot. (9) Heat more often replaces cold in the thermal spatial pattern than warmth. No similar list of relationships of heat to warmth can be made out.

Conclusion

We conclude that heat is a physiological fusion of the excitations normal to warmth and cold. It may be aroused on the volar forearm by the adequate simultaneous stimulation of a warm spot and of an adjacent cold spot. The two spots may, however, yield a simple sensation of heat when separated by relatively great distances (10-15 cm.).

The resultant heat is difficult to localize. Sometimes, when localized, it appears to connect or to lie between the two points stimulated; sometimes it lies at one point or the other. Normally heat appears as small and sharply defined, but, when referred to a concomitant pressure-pattern, it may be large and diffuse. Heat may be localized at the same place as a concomitant warmth or cold or in a different place.

Rarely heat appears as an imperfect fusion of warmth and cold. More often it seems itself to fuse with cold. Frequently it replaces or gives place to a simultaneous warmth and cold.

Both psychologically and psychophysically heat seems to be more intimately related to cold than to warmth.

The quality of heat is unique and may be characterized as "bright." One observer finds heat qualitatively related to pressure and pain.

BOOK REVIEW

The Social Evolution of Religion. By GEORGE WILLIS COOKE. Boston, Stratford Co., 1920. 416 p.

"The Social Evolution of Religion" is a book remarkable for its broad scope and its impartial treatment of the religious life of mankind as one of the phenomena of group activity. The author demonstrates that he is thoroughly at home in the fields of both comparative religion and social theory. He handles the various explanations of primitive religious manifestations such as totemism, animism, the mana concept, animal worship, etc., with ease and intelligent grasp of his subject, and turns with equal penetration to a discussion of the mother goddess cults and nature worship or to Buddhism and Christianity.

When Mr. Cooke turns to topics less definitely related to religious problems, his footing is less secure. Because he follows Durkheim in considering that religion is a product of the social mind, he feels it necessary to devote an introductory chapter to "The Social Transmission of Human Experiences," and another to "The Creative Genius of Social Man." In these sections, he makes some rather startling statements. For instance, he insists that Galton and the other Eugenists are entirely at fault, and that the environment in which the individual mind is situated is even more influential than the hereditary equipment of the mind. Indeed, he goes so far as to say that the eugenic theories and the theory of the superman are in the same class. Had he been a psychoanalyst, he would doubtless have considered both theories a flight from reality.

When Mr. Cooke states his position in less extreme terms, and merely points out that congenital and social heredity are so intimately associated with each other that they are often inseparable, he is, of course, entirely beyond criticism. The work of Watson (*Psychology from the Standpoint of a Behaviorist*) and of Kempf (*The Tonus of the Autonomic Segments as Causes of Abnormal Behavior, J. Nerv. & Ment. Diseases, 1920*) has indicated this same conclusion from studies of the social conditionings of the emotional reactions. Since it has been demonstrated that instinctive and emotional activities can be modified almost infinitely by the social milieu, Mr. Cooke is quite justified in attributing a great deal of significance to the social inheritance in the molding of the biological endowment of the individual.

The most interesting chapters in the book are those already referred to concerning primitive rituals and beliefs, and the concluding chapters on "Universal Religion" and "Religion as a Cosmic and Human Motive." In these last two sections, the modern attitude of scientific inquiry, which does not even shrink from close scrutiny of its gods, is commented upon, and the results of its study summarized. The chief criticism of Christianity is the fact that in our modern economic and social system it may be professed but is seldom practiced. The religion of the future must find some way to bridge this gap between theory and application.

PHYLLIS BLANCHARD.

BOOK NOTES

The Mental Hygiene of Childhood. By WILLIAM A. WHITE. Bost., Little, Brown & Co., 1919. 183 p.

This very timely and much needed book, without attempting to give an exhaustive account of the psychology of childhood and its relations to parenthood, examines the problems of the child from the psychoanalytic point of view. Concerning the child the author holds, contrary to the general opinion, that it is possessed of a developing sexuality, the roots of which seep back into infancy. His other conclusion is that there exists certain disruptive tendencies between the child and his family. Hence in his eleven chapters the author discusses fundamental instincts, the development of the child, function of the parent, historical background, the family situation, etc.

Graphology and the Psychology of Handwriting. By JUNE E. DOWNEY. Balt., Warwick and York, 1919. 142 p.

These studies seek to canvass the possibility of a scientific characterological utilization of handwriting. Their main purpose is one of orientation, preliminary to an attempt to use graphic activity in tests of temperament or character traits, tests which are now in process of standardization. Part I gives a critical comparison of graphological contentions and the outcome of modern scientific investigations of handwriting, while Part II reports a number of experimental studies designed to try out methods of research. Free use is made of the results of other investigations.

Psychoanalysis: Its History, Theory and Practice. By ANDRÉ TRIDON. N. Y., Huebsch, 1919. 272 p.

The author here attempts to sum up concisely the views of the greater American and foreign analysts which are at present scattered in hundreds of books and pamphlets. It is unpartisan but regards Jung's and Adler's theories as of inestimable value. He sees no irreconcilable differences between their points of view. In the twenty-one chapters which constitute this book we have a very good account of all the main lines of development, with a glossary and bibliography at the end. There is an interesting final chapter on The New Ethics.

Introductory Psychology for Teachers. By E. K. STRONG, JR. Balt., Warwick and York, 1920. 233 p.

The first part of this book discusses the learning process; the second, individual differences. The last part takes up some physiological aspects of psychology.

Personal Beauty and Racial Betterment. By KNIGHT DUNLAP. St. Louis, C. V. Mosby Co., 1920. 95 p.

The author makes the procreation of children the predominant ideal in marriage, minimizing companionship and other "spiritual" factors. Another feature of this book is that although he calls attention to various manifestations of sex he has no practical reform program to propose. In the first part he discusses the significance of beauty with negative and positive traits, while the second part is devoted to a discussion of its conservation.

Imagination and Its Place in Education. By EDWIN A. KIRKPATRICK. Bost., Ginn & Co., 1920. 214 p.

Science embodies universal truths; literature expresses truth as seen or exemplified by individuals. Neither alone can adequately deal with imagination. Psychology seeks to present what is true of all minds, but in no field are individual variations so great. In Part I the author treats imagination and related activities; in Part II, the imaginative life of children; III, school subjects and the imagination.

Modern Spiritism. By A. T. SCHOFIELD. Phil., P. Blakiston, 1920. 260 p.

The standpoint of this author is advanced mystical Christianity. He severely critizes spiritualism in the interests of spirituality. He is a firm believer in the immortality of the soul but deprecates all interpretations which even advanced modern spiritism gives to this conception.

Sex Attraction. By VICTOR C. VAUGHAN. St. Louis, C. V. Mosby Co., 1920. 44 p.

Women's Wild Oats. By C. GASQUOINE HARTLEY. N. Y., Frederick A. Stokes Co., 1920. 238 p.

The results of the war have led this author to jot down her reflections upon the following topics: The Prosperity of God, The Covenant of God, That Which is Wanting, "Give, Give!" If a Child Could Choose? and Foreseeing Evil.

The Hysteria of Lady Macbeth. By ISADOR H. CORIAT. N. Y., Moffat, Yard & Co., 1912. 94 p.

The contents of this book are: Repression and the Subconscious, Somnambulism and Hysteria, Psychoanalysis and Literature, The Problem of Lady Macbeth, The Psychoanalysis of Lady Macbeth, and a bibliography.

An Introduction to Social Ethics. By JOHN M. MECKLIN. N. Y., Harcourt, Brace and Howe, 1920. 446 p.

In Part I, which is historical and introductory, the author treats the problem of democracy, the religious background—Calvinism, the triumph of individualism, the great society, and our uncertain morality. Part II, which is psychological, treats the organization of the moral sentiment, the social conscience, public opinion, and moral progress. In Part III, entitled The Social Order, he discusses the rôle of the institution in the moral economy, the individual and the institution, the home, the ecclesiastical ethic, the school and the social conscience, the ethics of private property, mechanism and morals, the worker and the machine process, the ethics of business enterprise, the problem of the city, and political obligation in American democracy.

An Introduction to Philosophy. By HOLLY ESTIL CUNNINGHAM. Bost., Richard G. Badger, 1920. 257 p.

The author seeks (a) to present the student with a method of studying problems of the type of those discussed in philosophy especially and in the social sciences generally, and (b) to acquaint him with the fundamental movements and problems of philosophy so that he will not be "up in the air." So in successive chapters he discusses methods, psychological, physical, and social backgrounds, the nature of primitive thought, origin of the problems of philosophy and the problems themselves; Platonic, subjective, and objective idealism, empiricism, evolution, etc.

The North Riding of Yorkshire. By W. J. WESTON. Cambridge, The Univ. Press, 1919. 161 p.

This is a very interesting and voluminously illustrated study of Yorkshire, somewhat on the basis of Geddes and Branford but including also modern aspects of the country.

Native Villages and Village Sites East of the Mississippi. By DAVID I. BUSHNELL, Jr. (Bul. 69, Bureau of American Ethnology). Wash., Govt. Printing Office, 1919. 111 p.

Thirty-Second Annual Report of the Bureau of American Ethnology. 1910-11. Wash., Govt. Printing Office, 1918. 819 p.

Third-Third Annual Report of the Bureau of American Ethnology, 1911-12. Wash., 1919. 677 p.

Third Annual Report of the Massachusetts Commission on Mental Diseases (1918). Bost., Wright and Potter, 1919. 260 p.

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AN EXPERIMENTAL STUDY OF VISUAL MOVEMENT AND THE PHI PHENOMENON[1]

By F. L. Dimmick

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Introduction

In his "Experimentelle Studien über das Sehen von Bewegung"[2] Wertheimer reports the isolation of a new elementary mental experience, that of movement. To keep it distinct from the perceptual complex in which it usually occurs, he calls it *Phi* or the *Phi phenomenon.* Analysis of it reveals nothing for him but just the movement-experience; it has no visual quality; it has only spatial localization and direction.

The aim of our investigation is to put this elementary movement-experience, this Phi phenomenon, under critically descriptive conditions, and to test its analysability.

The term 'movement sensation' has been in common usage for a long time. Exner[3] first gave to it the meaning, in a pregnant sense, of a primary and elemental experience. He cites experiments to demonstrate its immediacy, and points out its fulfillment of the characteristics of sensation. The peculiar nature of the movement experience was again emphasized by Schumann,[4] and an experiment to demonstrate its simplicity was indicated. Wertheimer followed closely the cue given by Schumann. He chose the simplest conditions under which the impression of movement could be obtained. Two lines, differing in position, were presented successively in the Schumann tachistoscope. The principal stimuli were a pair of parallel horizontal lines, and a pair of lines set at an angle to each other. A number of variations of these two stimuli were made in order to test the effect of different conditions. The *O*'s seem not to have been given specific instructions, but to have been allowed to report spontaneously what they experienced. They were, of course, kept in ignorance of the nature of the stimulus. Only three *O*'s were considered necessary, both because those with whom the work was started agreed well with one another, and because the reports were immediate.

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[1] From the Psychological Laboratory of Cornell University.

[2] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych. u. Phys. d. Sinnes.*, 1 Abt. 61, 1912, 161.

[3] S. Exner, Ueber das Sehen von Bewegungen, *Wiener Sitz. Ber.*, Abt. 3, 72, 1875, 156.

The important determinant for movement, with the above stimuli, Wertheimer finds to be the temporal interval elapsing between the presentations of the two members. There is an optimal time, 60σ, at which the movement of a line all the way from the one position to the other results. With intervals either larger or smaller, the movement experienced is not complete. As a rule two objects are seen, both moving more or less, but not moving over the entire field. Such an experience we shall call bimembral movement. It sometimes happens that only one of the members seems to move, while the other remains stationary. This experience may be designated unimembral movement. A fourth type of movement is that in which there is no change of position of either member, but only a movement within a member itself, an intramembral movement. Under occasional or demonstrational conditions, Wertheimer is able to get the movement-experience alone, independent of any substantive perception and with no visual quality. Because it can thus exist in its own right, it is given the designation Phi or the Phi-phenomenon. It is this Phi, then, which, when added to the perception of a line in two successive positions, gives the impression of movement. It is the basis, too, of that seen movement which is correlated with a physically moving stimulus. The observations which he made led Wertheimer to outline a physiological theory of a short-circuit of nervous excitation in the cortex[5] as the basis of the Phi-phenomenon.

EXPERIMENTAL PROCEDURE

For our experimental work, we used a modification of the Dodge tachistoscope which enabled us to present successively the two members of a bimembral stimulus. In addition it gave us control over the pre- and post-exposure fields.

The particular modification which we made of the Dodge instrument was the addition of a third stimulus-field by insertion of a second transparent mirror. A group of three plain-glass nitrogen-filled lamps (two 200 watt, and one 100 watt) furnished illumination for the stimulus-fields. They were placed at the ends of a set of three parallel tubes of about 5 cm. diameter. The light conducted down the tubes was reflected from mirrors to the respective stimulus-fields. Between the lamps and the open ends of the tubes was placed a falling shutter, so designed that the exposures of the three fields were independently variable. The openings through which the light entered the tubes were horizontal slits, 1 cm. by 4 cm. To give additional control, an auxiliary shutter was set in every tube a little way from its open end, and with it a diaphragm which made possible the equation in sensible brightness of all three fields. The shutter fell from a constant height by its own weight when the current was broken through an electromagnet. The friction, as the falling shutter ran in the guides, was slight and the variation in time of fall proved negligible.

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[4] F. Schumann, *Ber. ü. d. II Kong. f. exp. Psychol.*, 1907, 218.

[5] *Op. cit.*, 248.

This arrangement made it possible for us to start with any pre-exposure field, to present the first member of the stimulus for a given length of time, then after a variable interval (during which the pre-exposure field was repeated) to present the second member for the desired length of time, and then to return to the pre-exposure field. The shutter permitted of total times up to 380σ; $210\,\sigma$ was the longest needed. The eye-piece of the exposure apparatus was inside a dark chamber, and the *O* was allowed 10 min. to become partially dark-adapted before work was begun.

All of the stimuli described by Wertheimer were repeated, and six object-pictures of our own were added. The greater part of our work was done with two arrangements, the one of which gave movement from an oblique to a horizontal position, and the other from an upper horizontal to a lower horizontal position. There were four pre-exposure fields: A, uniform white, identical with the background of the stimulus-cards; B, similar to A, with a black fixation-point at the middle of the field; C, uniform lightless field; D, unlighted field, bounded at the top and bottom by luminous lines.

The exposure-time for each member of the stimulus was kept constant at 30σ. The time-intervals were 30σ, $60\,\sigma$, 90σ, 120σ, 150σ. They cover the range given by Wertheimer from near simultaneity to the region of succession. The principal stimuli, the angle and the horizontals, were presented in series of five exposures with every pre-exposure field, at every time, and to every *O*. Four other stimuli were presented to every *O* in the same sort of series, but with only two of the pre-exposure fields; they were so chosen that every stimulus was studied by one or two *O*'s and with two to four pre-exposure fields. A haphazard order of fields, times, and stimuli was followed in the presentation of the series.

Two different sets of instructions were given to the *O*'s, thus duplicating the above set of series. The one of these, which we shall call the ' process ' instruction, was: " A stimulus will be shown you which will arouse a visual perception. Describe this perception in strictly psychological terms as accurately as you can. Report no process of which you are not sure. The stimulus will be repeated to complete the description." The other set, the 'meaning' instruction, ran: "A stimulus will be shown you which will arouse a visual perception. Characterize this perception as fully as you can. The stimulus will be repeated to complete the characterization." To three *O*'s the process-instructions were given first, and the entire group of series was repeated with the meaning-instructions. Three other *O*'s were required to give meaning-reports first, and were then shifted to the process-attitude. The two most practised *O*'s were instructed in successive series either for process or for meaning by chance, and every series was repeated later with the alternate instructions. Except for the last two, the *O*'s were required to read the instructions at the beginning of every observation hour. Before the regular work was begun, trial series were taken until the reports gave evidence of adequacy and stability.

The eight *O*'s who served were: Miss C. Comstock (C), graduate student in psychology; F. L. Dimmick (D), assistant in psychology; Dr. L. B. Hoisington (H), instructor in psychology; Miss M. F. Martin (M), graduate scholar in psychology; Miss A. H. Sullivan (S), graduate fellow in psychology; S. A. Takaki (Ta), graduate student in psychology; S. A. Tung (Tu), graduate student in psychology; and Mrs. A. K. Whitchurch (W), graduate student in psychology. Observers C, D, H, and S were practised; M, Ta, Tu, and W were relatively unpractised.

O sat with his head in the hood of the instrument. The auxiliary shutter was opened, a ready-signal given, the exposure shutter dropped, and the auxiliary shutter closed.[6] *O* then gave his report according to the instructions. The exposure shutter was raised to the starting position, and the stimulation and report repeated, until the series of five was completed. A rest of 2 or 3 minutes was permitted while the apparatus was reset for a new series. The above procedure was followed for the 160 series with every *O*.

Results

Table I summarizes quantitatively the results obtained with the two principal stimuli. Under our conditions, all *O*'s perceived optimal movement of the horizontal lines in more than 50% of the cases, and only one fell below that percentage with the oblique-to-horizontal movement. Of the perceptions of partial movement, unimembral movement shows itself much more frequently than bimembral for all but two *O*'s. Simultaneity and succession occur but rarely. Closely paralleling the above perceptions are the percentages of reports, under the instructions to 'describe in psychological terms,' in which some visual process was present in the field between the two members of the stimulus. The number of times when the entire spatial interval was involved agrees with the number of optimal movements perceived; the frequency of cases in which the process in the interval accrues to one of the members and not to the other is approximately the same as that of the reports of unimembral movement; processes which involve both members but not the entire field occur as often as does bimembral movement.

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[6] The function of the auxiliary shutter was to shut off all light from passing down the tubes save when a true exposure was made. It eliminated the possibility of a false exposure when the falling shutter was raised to its starting position.

TABLE I

Process Configurations and Movement Perceptions

(Percent. of 100 Reports for Every *O*)

	Horizontal Lines								Oblique Line and Horizontal							
Obs.	Total		Uni-		Bi-		None		Total		Uni-		Bi-		None	
	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M
C	78	77	20	15	3	8	0	0	48	35	43	58	8	6	1	1
D	66	61	22	21	12	16	0	2	77	73	12	14	11	14	0	0
H	66	56	24	24	3	15	7	5	67	66	25	19	5	10	3	5
M	72	65	23	21	1	11	4	3	100	88	0	12	0	0	0	0
S	77	69	6	7	13	23	4	1	90	72	1	23	1	7	8	0
Ta	63	52	25	28	10	17	2	3	60	65	30	33	7	2	3	0
Tu	77	69	22	21	2	9	0	1	80	69	20	29	0	1	0	1
W	60	53	21	22	19	25	0	0	68	77	19	15	13	8	0	0
Average	70	63	20	20	8	15	2	2	74	68	19	25	6	6	2	1
Mean Var.	6	7	4	4	6	5	2	1	13	10	11	11	4	4	2	1

Uni-, Bi-,= experiences of unimembral and of bimembral movement.
P= Process configuration; M = meaning characterization.

TABLE II

Optimal Movement with Different Backgrounds

(Percent. of 25 Reports for Every *O*)

	Horizontal Lines								Oblique Line and Horizontal							
Obs.	A		B		C		D		A		B		C		D	
	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M
C	100	76	53	76	73	80	87	76	53	56	59	52	40	24	40	8
D	60	40	76	48	72	60	60	96	64	68	68	64	80	88	96	72
H	56	56	72	80	72	68	64	52	72	48	72	92	64	68	60	56
M	76	67	56	53	68	67	88	73	100	100	100	100	100	40	100	52
S	84	87	72	93	72	40	88	53	96	60	92	60	80	93	92	73
Ta	67	40	53	48	53	56	80	64	67	60	60	76	67	60	47	64
Tu	73	68	93	68	67	68	73	72	80	68	87	84	73	52	80	72
W	68	60	60	53	52	53	60	47	64	80	60	67	76	67	72	93
Average	73	62	67	65	66	62	75	67	78	68	75	74	73	62	73	62
M. V.	10	11	11	13	7	8	11	13	15	12	14	14	11	17	19	17

TABLE III

Optimal Movement with Different Temporal Intervals
(Percent. of 20 Reports for Every *O*)

	Horizontal Lines										Oblique Line and Horizontal									
Obs.	30		60		90		120		150		30		60		90		120		150	
	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M	P	M
C	67	55	92	95	92	90	83	80	58	65	25	0	62	50	75	70	50	35	25	20
D	50	75	90	65	80	85	80	45	30	35	70	45	95	90	95	95	70	70	45	65
H	55	50	80	65	80	75	65	60	50	30	50	65	90	95	90	90	55	75	50	30
M	50	75	75	92	90	75	75	75	70	8	100	83	100	100	100	100	100	100	100	58
S	60	83	90	92	95	92	95	42	45	33	100	92	85	100	95	92	100	42	70	33
Ta	50	70	83	55	92	65	42	35	50	35	50	50	83	60	92	100	50	75	25	40
Tu	75	70	100	95	100	85	50	55	58	40	75	75	83	90	100	80	90	60	50	40
W	15	25	70	75	90	83	75	42	50	42	60	67	70	92	100	100	80	67	30	58
Av.	53	64	85	79	90	81	71	54	51	36	66	72	84	85	93	91	74	66	49	43
M.V.	12	14	7	14	5	7	14	13	8	10	20	21	9	15	6	8	18	15	18	13

In Tables II and III the results have been arranged to indicate the influence of the pre- and intra-exposure field and of the temporal interval between members. Individual variations appear with the various fields, but they show no uniform tendency, and their nature is indicated in some cases in the introspections. C often objected that the fixation-point on pre-exposure field *B* interfered with her reports of process; S complained of the dark backgrounds. It is significant, however, that the process-reports and the perceptive characterizations parallel each other. This agreement is very evident when the averages for the group are taken, and it is further supported by the fact that the mean variations are not excessive. We may conclude, then, that the kind of pre-exposure field employed had no general and constant effect on the perception or its process-configuration.

Table III, on the other hand, brings out a very definite influence of the temporal interval. The *O*'s vary among themselves, but they agree in that they have curves of percentages which are high for the middle values of the temporal interval and low for the extreme values. Process-reports and perceptive characterizations run parallel. The averages, of course, show the conformity even more prettily. Time-interval 90σ has the highest average percent. of optimal movement and of total configurations; 30σ and 150σ have the lowest. Just as significant is the fact that the mean variations are smallest at the middle value. The larger variations among *O*'s occur with the longer or shorter times.

The evidence of the values, in the above tables, of the various perceptive characterizations indicates unquestionably that we have reproduced the essential factors of Wertheimer's conditions. Table I shows that our temporal intervals lie between those which give succession and simultaneity. The two forms of partial movement appear frequently, but optimal movement predominates. The ineffectiveness of variations of the pre-exposure field does not contradict Wertheimer's observation that the color of the stimulus did not affect the perception. Finally, the behavior of the perception with the different time-intervals is exactly that which Wertheimer suggests, though he makes no precise statement.

On the other hand, in the column in which Wertheimer would write 'nothing,' under the heading 'process,' we have found a closely parallel set of values which represent the configurations of visual process present in the movement-field when the *O*'s were set to describe the experience. The nature of the visual experience which filled the interval can be pointed out most clearly by direct reference to the introspections.

OPTIMAL MOVEMENT; MEANING REPORTS; STIMULUS I

C. A horizontal line appeared in the upper part of the field, moved down to a lower position, and stopped; the movement was slow, smooth, and continuous.

D. I saw a horizontal line move quickly from a position above the fixation point to one below, and disappear. It seemed to come in, move down, and go out, as if that were only a part of some larger movement. The movement was rapid but smooth.

H. A black horizontal line appeared and moved down very rapidly, and then came to rest in a lower position for a short time. The movement was uniform but very fast.

M. A line moved from an upper horizontal position to a lower one; it slowed up just before it reached the final position; the movement was continuous all the way from one to the other.

S. A rectangle moved down smoothly and stopped for an instant. The speed of the line increased as it moved, and was very rapid just before it stopped.

Ta. A horizontal line appeared in the center of the field and moved down smoothly; it became shorter as it moved, and stopped in a position lower down.

Tu. A line dropped downward very smoothly and rapidly, until it reached the lower position, and then disappeared.

W. I saw a line drop smoothly and quickly from one position to another.

STIMULUS II

C. I saw an oblique line which moved down across the field to the horizontal with a smooth and continuous movement.

D. A dark grey rectangle moved smoothly and rapidly from an oblique position to a horizontal.

H. An oblique line fell down into a horizontal position. The movement was continuous, but faster just before it reached the final position.

M. An oblique line moved down through an arc to the horizontal.

S. An object in the oblique position fell to the horizontal. The movement was rapid, and stopped short at the end.

Ta. An oblique line appeared, turned downward, and took the horizontal position. The movement was smooth.

Tu. I saw an oblique line first, and it moved steadily down into a horizontal position.

W. The oblique fell rapidly into the horizontal; I saw movement all the way.

PROCESS REPORTS; STIMULUS I

C. At the upper part was a black horizontal extent which immediately gave place to a grey patch below and joining it. The bottom of the grey was darker and at that place appeared later a black extent similar to the first. The grey changed in quality; it was one continuous extent, but it was different at different places.

D. A black line appeared just above the middle of the field. It disappeared, and extending down from where it had been was a grey band, light at the top and dark at the bottom, and at the lower side was a very dark strip of grey. The grey patch did not change in extent, but the patterning of brightnesses changed.

H. The upper line was not definite at the lower edge, but fused into a filmy grey that was a little darker at the right and left extremes, and terminated in a black process lower down.

M. There was a line of very dark grey at the top, and immediately below a darkening of the field in the general form of a rhombus, the edges of which were fuzzy but fairly definite. At the bottom of this figure was a definite line, and in the middle a darker streak which could hardly be called a line.

S. At the top was a black rectangle, clear and intense. Below was a grey patch, longer in the horizontal dimension, but not clear. At the bottom was a definite line.

Ta. At first an irregular rectangle, black and sharp at the top, appeared. Its lower side was grey and blurred. Below was a wide grey line, sharp at the bottom but indefinite above. The lower and upper sides of the upper and lower lines respectively shaded off and joined each other.

Tu. First, there was a line above the middle of the field; below it, a dark grey which faded out farther down until it reached a lower horizontal line, the upper part of which was grey and indefinite, the lower part black and sharp.

W. A horizontal line appeared and disappeared, and below where it had been there was a wide grey rectangle of light tint at the top, medium in the middle, and dark at the bottom. Below appeared a second line.

STIMULUS II

C. First there was a black oblique at the top, which immediately became greyer, and then was replaced by a grey patch, which extended down to the horizontal, where a line appeared.

D. The oblique came in first; then below it there was a grey which extended down to a horizontal line below; this grey was not uniform but light and dark in streaks.

H. An oblique line appeared and then a horizontal, and between them the field was grey in a solid fan-shape.

M. An oblique appeared; then below it there came a sector of pale grey which was darker near the bottom, where it became a more definite line. The first line had faded out before the last one came.

S. A black oblique line appeared; then something grey came below it which was rather indefinite, and extended down to the horizontal line.

Ta. I saw an oblique line which was blurred downward. The upper part was black and clear-cut. The lower part shaded off into a grey, at the bottom of which came a horizontal line.

Tu. I saw an oblique line and a horizontal, and the space between them was grey. The oblique was wide and had no definite outline on the lower side.

W. An oblique line appeared and disappeared, and then a horizontal came below. Between the two was a triangular light grey patch, which was darker near the bottom line.

UNIMEMBRAL MOVEMENT; MEANING REPORTS; STIMULUS I

C. A horizontal line moved down smoothly but not continuously. It moved part-way, then disappeared, then reappeared lower down and stationary.
D. The first line appeared and remained stationary. Below it another line came in, moving downward. The movement was over most of the space between the two positions, but was of the lower line.
H. A line appeared in the upper part of the field for an instant; then a little lower down something moved into the field, and stopped as a black line at the bottom.
M. A horizontal line came into the part of the field moving, then another appeared lower down and stationary.
S. A line moved down smoothly, getting faster as it went, and then the end-position stood out, sharp and stationary.
Ta. A line moved down, and as it moved another appeared lower down and remained stationary.
Tu. First I saw a line which wavered but did not change position, and at the same time there was something moving down into a position below.
W. A line dropped into a lower position. The motion was best near the bottom, and there was a blank space just below the top position.

STIMULUS II

C. An oblique and horizontal appeared, and the first moved down toward the second but never reached it.
D. I saw two lines, one oblique and one horizontal. The latter remained stationary, and the oblique moved down toward it, but was gone before reaching it.
H. An oblique line came in and moved downward, and below it stood a horizontal line.
M. An oblique line moved down toward the horizontal. The movement was jerky and definitely of the oblique, not of the horizontal.
S. I saw a line move down very rapidly through an arc. The first part of the movement was not clear, *i.e.*, it seemed to be part way down before I saw it; above was a blank.
Ta. An oblique line appeared, then a horizontal, and the oblique moved down to the horizontal.
Tu. An oblique line appeared in the upper part of the field and moved down faster and faster until I could not see the movement. Then the horizontal came in.
W. An oblique line moved rapidly to the horizontal. The movement was best at the beginning, and just at the bottom there was a space of no movement at all.

PROCESS REPORTS; STIMULUS I

C. I saw a black line; and below it was a grey band which was not uniform in quality, but which had no definite lines in it. Below was a second sharp black line.
D. Two lines came successively. The upper was clear-cut and stationary; the lower was at first a band of greys, the upper edge of which was indefinite and concave.

H. A black line appeared in the upper part of the field. Below and detached from it by a bright streak was a rather indefinite grey, which fused into a black line lower in the field.

M. First came a line in the upper part, then a grey, and another line below. The grey joined the lower line but was separated from the upper by a bright streak.

S. I saw a horizontal line, then lower down another with a grey patch above it.

Ta. The first line was wide; the upper part black, the lower part grey and blurred. There was a bit of white background between this and the lower line.

Tu. There was a sharp definite line. Just below it was white, and all the rest below was grey, darkening downward until it joined the lower line.

W. I saw two horizontal lines, one below and after the other. A band of greys extended up from the lower toward but not to the upper.

STIMULUS II

C. There was a wide patch of grey in the shape of a sector. It was dark at the top and shaded off into light grey downward. The grey did not reach to the horizontal line which came in at the bottom.

D. A sharply defined oblique line appeared and then a horizontal. When the horizontal came, the field just above it was greyish.

H. First there was an oblique line with a patch of bright field just below it; then at the bottom a narrow sector of light grey, which was later replaced by a horizontal.

S. A black oblique appeared first; below it was white. Then came a horizontal line, with an oblique grey on its upper edge.

Ta. The oblique was grey on its lower side and blurred downward. There was a white space between it and the horizontal line.

Tu. I saw an oblique line clearly; then the background below it became dark at the top and light lower down. At the bottom there was a horizontal line with a white streak above it.

W. A distinct oblique line appeared at the top, then a horizontal, and between them a series of grey triangles, light at the top and dark at the bottom and joined on to the lower line.

BIMEMBRAL MOVEMENT; MEANING REPORTS; STIMULUS I

C. A horizontal line moved down and stopped. The movement was not continuous; there was a place in the middle where it disappeared entirely. Farther down it came back again.

D. A horizontal line appeared and started to move, then disappeared, but reappeared lower down, and moved into the lower position.

H. A heavy black line appeared and started to move, but disappeared. Part way down it reappeared, and moved into the lower position and stopped.

M. Two black horizontal lines appeared and both moved down a little.

S. An object appeared and moved downward. A little way below another line appeared, and moved down and stopped.

Ta. A line appeared, moved down and disappeared; after a short space another appeared, and moved down and stopped.

Tu. A horizontal line moved down very rapidly, disappeared for an instant, then moved down again into the lower position. The upper part of the movement was the more rapid.

W. The line moved down quickly but with a flicker, *i.e.,* it started to move, there was a break, then the last part of the movement came out distinctly.

STIMULUS II

C. An oblique line moved down to the horizontal. The movement was not smooth; it disappeared in the middle so that there was a blank space.

D. An oblique appeared and began to move, but very soon disappeared. Then a horizontal appeared, which seemed just at the end of a movement down.

H. The oblique started moving, then for a bit I saw nothing, then the line moved from just above into its horizontal position.

S. An oblique line moved down to the horizontal, but there was a pause in the movement part-way down.

Ta. An oblique line came in and started to move, but soon faded out; then something below jumped in, and moved down to the horizontal.

Tu. An oblique line moved down to the horizontal but not smoothly; there was a flash of good movement at the beginning and end.

W. An oblique line dropped to the horizontal, but I did not see movement all the way. There was a blank in the middle.

PROCESS REPORTS; STIMULUS I

C. I saw a line in the field above the middle. Its edges were not sharp, and it shaded off at the lower side. Below appeared another line, which was surrounded with grey at first and then became quite distinct.

D. A horizontal line came in with a grey blur on the lower side. The grey extended farther down from the extremities of the line than from the middle, so that its lower side was concave though not sharply outlined. The second line had a similar grey above it. A roundish patch of white background was left between the two.

H. I saw a black line a little above the middle of the field, then another below the middle and equal in extent to the first. Between was a grey, with a slight gap of bright field in it.

M. There appeared two lines of grey. Both were indistinct on the edges that were toward each other.

S. I saw a line that was clear and black at the upper edge but indistinct below. Then came a space in which there was nothing, and then I saw a hazy line at the bottom.

Ta. The first line was grey all over and a little wider at the ends; the middle part had no definite outlines. The second line had black and clear outlines at the ends but the middle part was indefinite at the top, and the grey margin of it was a little concave.

Tu. First an upper and then a lower horizontal appeared, and the space between was grey except at the center, where it was the same as the background.

W. I saw a horizontal line, then another below it. Between them was a blur of grey, darker at the top and bottom and white in the middle.

STIMULUS II

C. I saw an oblique line. It disappeared and the field was only white, then a horizontal appeared. Both lines were indistinct and blurred toward the interior of the angle.

D. I saw an oblique line. It disappeared and I saw a horizontal. The oblique was at first a sharp line, then it shaded off into the background below it. The horizontal was indefinite along its upper edge when it came.

H. An oblique line appeared which was not sharply outlined. Just below it was a very light grey. Then a horizontal appeared, which was indefinite on its upper edge at first but soon became sharply outlined.
S. First there appeared an oblique line, which was not clear at the lower edge but shaded off into grey. Below there was an interval of white, and at the bottom a horizontal which also lacked definition.
Ta. The upper side and end of the oblique had clear-cut outlines, but the lower side was greyish and blurred down. The upper side of the horizontal had no definite outline.

For the demonstration of bimembral movement, Wertheimer recommends a stimulus consisting of two squares for the first member and a horizontal line for the second. This stimulus aroused bimembral movement in 94% of the times it was presented to our *O*'s and corresponding process-configurations in 84% of the times it was given under the process-instructions. The typical reports are as follows:

MEANING REPORT; SPECIAL STIMULUS

C. I saw a horizontal line and above it two squares. All three moved down a little and then disappeared.

PROCESS REPORT

C. Two squares appeared in the upper part of the field. They were grey and indefinite at the bottom. Below them came a black horizontal line, which shaded off into grey at the top.

The parallel values for the percentage of the three forms of movement are sufficient evidence that every perception of movement of these sorts has a corresponding process-configuration. The conclusion is further substantiated by the fact that in a number of reports the *O*'s have stated that a certain movement had a particular process-aspect and conversely that when, for instance, the grey was spread over the whole field, the movement was best.

Wertheimer's fourth movement phenomenon, intra-membral movement, appears according to his statement in the stage of simultaneity. Our *O*'s, likewise, reported a number of cases, when the temporal interval was shortest, in which the lines stood still, but in which there was a quick " jerking back and forth " or an " unsteadiness " as if the line moved inside itself. Under similar conditions, with the instruction to report process, the lines were sometimes of a multiform grey, variously patterned. Further analysis of intramembral movement, with the stimuli which Wertheimer says are especially favorable for its arousal, confirms what we have already said. Typical reports are as follows:

INTRAMEMBRAL MOVEMENT; MEANING REPORTS; STIMULUS III[7]

C. The line seemed to double over on itself at the middle; that is, there seemed to be a smooth gliding movement from left to right within the line which did not, however, change position.

W. I saw a horizontal line in which there seemed to be movement across, left to right. It was not movement from one position to another, for there was only one line and the movement was within it.

STIMULUS IV[8]

S. There was a square which contracted and expanded as if it were elastic.

Ta. A square appeared and stretched to right and left.

STIMULUS V[9]

M. An oblique line appeared and shrank towards its upper end until it became almost a square.

Tu. There appeared a line in the oblique position which seemed to move toward its upper end, but did not change its general position.

PROCESS REPORTS; STIMULUS III

C. I saw a horizontal line which was black at the right end, dark grey at the left, and light in the middle. The left-hand part was not evenly outlined, and there was no definite line where it joined the black part.

STIMULUS IV

S. First a black square appeared; then in its place was a greyish rectangle longer in the horizontal, then one darker and longer, then one black and slightly shorter.

Ta. I saw a black square which was fairly clearly outlined at first. Then it became greyish, and to the right of it was a horizontal line of dark grey. Then the whole figure was a horizontal line black at the left end and lighter toward the right.

STIMULUS V

M. An oblique line appeared. Then it was lighter at the lower end, and shaded into a dark grey upward. At the top was a sharp black square.

Tu. First I saw a dark grey oblique line; its upper end was darker, with a light place just below it. The lower part of the figure faded out into a light grey.

Movement within an object without change of position of that object thus presents nothing new. Under the characterization-attitude, it is movement on a par with the change of position of a form or object; descriptively, it is reflected in consciousness as the visual quality grey.

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[7] Stimulus III consisted of two horizontal lines, the second of which was in a line with but to the left of the first.

[8] Stimulus IV consisted of a square and a horizontal line of the same width. The centers of the two forms came at the same point.

[9] Stimulus V consisted of an oblique line and a square of the same width. The square came at the upper end of the position of the line.

The Pure Phi Phenomenon

Finally, a number of cases of Wertheimer's pure Phi were reported by our *O*'s. Frequently the line was not seen during the movement; there was simply "something moving" or just "movement." Sometimes neither of the lines was clearly perceived; just a bit of movement downward. The cases in which both lines were perceived with only movement between are entered in Table I as optimal movement, and are paralleled by total process-configurations. The appearance of movement detached from its object occurred, as Wertheimer notes, when the stimulus was new or for some other reason was incompletely perceived. Under exactly these same conditions, but with the instructions to report process, our *O*'s noted a flash of grey without the usual limiting lines above and below.

Object-Stimuli

The remaining stimuli which were given by Wertheimer and which we repeated yield results agreeing with those already reported, but add nothing new to our analysis of the perception of movement. The six object-pictures which we added to Wertheimer's list showed the effect of central associations by giving optimal movement over the whole range of our conditions, and total configurations of process in all but a very few cases.

Colored Stimuli

In the description of the stimuli used in his experiments, Wertheimer tells us that he varied the color of the members. In another place he remarks that partial movement results when the two members are not of the same color. Aside from this, the implication is that one color gives movement as well as another. After our main series had been completed, we presented stimuli of various colors to our *O*'s. The stimuli were shown in series of ten exposures at the optimal time (90σ) and with a pre-exposure field of the same quality as the background. The colors of these stimuli were red, green, and blue on white backgrounds, white and yellow on black backgrounds, red on a green background, and blue on a yellow background.

Movement, optimal in all but a few cases, was reported by all *O*'s for all colors and backgrounds when the instructions were to 'characterize.' Under the descriptive attitude there was always noted a *grey* patch covering the space between the two members. When the backgrounds were white or a light color, the grey was a little darker than medium. On the black field it appeared whitish. We may state, then, with assurance that the grey which is the psychological equivalent of the perceived movement is under our conditions independent, for its quality, of the quality of the members of the stimulus, but shows an influence of the background which has the appearance of a brightness-contrast effect.

Korte's Reverse Movement

Korte,[10] in a later investigation, studied the perception of movement backward from the second member to the first, which, he said, resulted from making the second member more "intense" and by directing the attention to it. He varied "intensity" either by making the second member wider than the first, or by putting short perpendiculars across its ends. With an increase in "intensity" merely, only a slight backward jerk appeared. Neither was attention to the second line sufficient alone; but with an increase of "intensity" and a direction of the attention to the second member, the backward movement was plainly evident.

We attempted to repeat Korte's conditions. Our stimuli with members of different "intensities" were a grey line on white followed by a black line of the same width; a grey line on black followed by a white line of the same width; a black line of 1 mm. width on a white background followed by a black line of 4 mm. width; and a white line on black followed by a line of the same size with short perpendiculars across the ends. At first the *O*'s were given the usual meaning-instructions, and a series of ten exposures was presented. Then they were instructed to attend to the more "intense" line, and ten more exposures were made; and finally they were asked to try to get the line to move from the more "intense" position to the other. The perception was always from the member presented first to the second, save for a very few instances with the third instruction when *O* reported that perhaps there might be a hint of a jerk backwards; but even then no positive movement, such as they had seen with the other stimuli, was perceived.

We have repeated Korte's conditions as nearly as his description of them permits; and to his stimuli, which give spatial and form insistence, we have added intensive insistence. Since we never get a positive movement-perception as we do under Wertheimer's conditions, it seems evident that we are not in the presence of compulsory conditions, and that the two perceptions are not of the same order.[11]

The Nature of the Process-Configuration

Now that we have pointed out the quality of the psychological correlate of Wertheimer's Phi, we must emphasize that this is but one of its attributive aspects. It must not for a moment be assumed that it is a simple text-book grey, the grey that lies at the center of the color-pyramid, or the grey of a piece of grey paper. The temporal attribute is beaten up with the quality into an integration of the first order. All *O*'s describe the experience as a "flashing," "flickering," "unsteady," "shimmery," "flame-like," "liquid," "live" grey. It is further reported as a "curtain" or "film" which is not superficial but hardly bulky. The background seems at times to shine through the interstices of the grey, giving it a sort of transparency. In texture it resembles the *Flächenfarbe* described by Katz[12] and the adaptation-film of Sheppard.[13]

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[10] A. Korte, Kinematoskopische Untersuchungen, *Zeit. f. Psych. u. Physiol. d. Sinnes.*, *72*, 1915, 193.

[11] We are engaged in further experiments of this kind, the results of which will be reported later. We are also repeating certain experiments of F. Kenkel and V. Benussi.

CONCLUSIONS

We have, in this investigation, been concerned primarily with a critically descriptive analysis of the reflection in consciousness of a stimulus-complex which is compulsory for the visual perception of movement. We have found that this reflection in consciousness takes the form of a primary integration of a visual quality (grey) with a duration which is characteristically brief. The integration is a multiform grey flash which is independent of the quality of the stimulus but is directly determined by the temporal interval between the exposures of the two stimulus-members. The spatial attribute is a constant factor for any one exposure, but may vary from exposure to exposure (as the perception of movement also varies between optimal and partial). There is, then, no movement in the multiform grey flash; the space is constant; the integration is of time and quality.

Wertheimer, finding that there "is no visual filling-in of the field of movement," argues that the perception of movement must have as its physiological correlate a short-circuit in the cortex. The grey flash which we have found to be the psychological correlate of the perception of movement obviates the necessity of such recourse to novel and speculative conditions.

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[12] D. Katz, *Die Erscheinungsweisen der Farben,* 1911, 6ff.

[13] H. Sheppard, Foveal Adaption of Color, *Amer. Jour. Psy.,* xxxi 1920, 58.

A PSYCHOLOGICAL INTERPRETATION OF MODERN SOCIAL PROBLEMS AND OF CONTEMPORARY HISTORY: *A Survey of the Contributions of Gustave Le Bon to Social Psychology*

By Harry Elmer Barnes, Clark University

1. General Nature of His Method and Procedure

Of the three chief psychological sociologists that France has produced—Tarde, Durkheim, and Le Bon, the latter is the most versatile, and yet by far the most superficial. In fact, the last may be regarded as a popularizer of the more striking ideas of the first two, especially of Tarde's views on imitation and Durkheim's notion of crowd-psychology. The range of his interests, however, is certainly remarkable. Trained originally as a physician, he gave up the practice of medicine, but has contributed several works on physiology and hygiene. Next he was employed by the French government as an archeologist and paleographer in the Orient. In recent years he has been editor of the *Bibliothéque de philosophie scientifique.* In addition to these activities he has occupied himself by producing a general work on social evolution in two volumes; studies of the chief historic civilizations; several contributions to mathematical chemistry and physics, among them a paper on intra-atomic energy which was published in a number of the leading scientific journals; a statistical study in physical anthropology; a work or two on education; and the some half-dozen books on social psychology which will form the basis of the present discussion.[1]

Of course it is obvious that a man who ranges at will over a dozen fields of research, any one of which could only be partially traversed with thoroughness in a lifetime, is not likely to have excelled in any of them. This is certainly true of Le Bon, whatever his mental powers may be. Professor G. E. Vincent has thus characterized him in a fairly accurate manner: "M. Le Bon may be described as an intellectual kodak fiend. His books are filled with snapshots at truth, interesting in themselves, but sadly unconnected and out of focus."[2] At the same time, Le Bon's works are all highly interesting, and many of his generalizations sound plausible. His arguments are bolstered up by copious citations of a pertinent nature. Le Bon is one of those writers who exploits his theories in his own works. In discussing crowd psychology, for example, he tells the reader that the sure and certain method to be successful in convincing an audience of the truth of an assertion is to affirm the matter repeatedly, and, at the same time, to be careful to avoid any attempt at thorough analysis or any reference to a possible exception to its applicability. Nothing is more characteristic of Le Bon's own procedure than this very method. Taking a few rather striking psychological postulates which have the virtue of modernity, novelty, and suggestiveness, he applies these conceptions to nearly every phase of contemporary life in general and to French social conditions in particular. These theses are repeated and reiterated without detailed analysis or candid statement of exceptions to their application, until even a wary reader is likely to be beguiled by the facile phraseology of the author. Aside from his brilliant but uncritical dogmatism and "cock-sureness," another characteristic of Le Bon's socio-psychological writings should be noted. That is what Herbert Spencer would call his "anti-patriotic bias" and his "class bias." At least up to the outbreak of the World War, Le Bon could see little good in what he alleged to be the characteristics of the modern Romance peoples. Their assumed tendency towards a crowd-psychological condition and their desire to suppress individuality and put into power the incompetent masses reveal little of promise from his viewpoint. The oft-asserted Anglo-Saxon initiative, energy, will-power, and individualism, attract him as strongly as the alleged French traits repel him.[3]

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[1] For a list of Le Bon's contributions see the article on Le Bon in *La Grande Encyclopédie* for his earlier works, and the biographical note in the *New International Encyclopedia* for a list of his main works. Another list of his works is given in the French biographical annual *Qui Êtes-Vous?* His productivity is perhaps only exceeded by that of his fellow-countryman, Solomon Reinach, who can hardly boast an equal breadth of interests. The writer has attempted to summarize the contributions of Tarde to this subject in an article in the *Philosophical Review* for May, 1919, and of Durkheim in the *Political Science Quarterly* for June, 1920.

Again, Le Bon finds little to arouse his enthusiasm in the traits of the masses; from his viewpoint progress and civilization are almost exclusively the contribution of the intellectually *elite*. There can be no doubt that Le Bon's exaggerations are in part due to his generalizations from French conditions, though even these he views in an extreme and exaggerated light. The relation of Le Bon's doctrines to his social environment is not of that subtle type which is likely to escape the attention of the reader, but is so prominent in all his works as to make them full of generalizations which are highly inaccurate and distorted when viewed as sociological propositions of general import. His fundamental doctrines—the idea of national character, the psychology of crowds and revolutions, his " anti-patriotic " and " class " bias, his continual scenting of impending calamities, and his bitter attacks upon socialism and syndicalism, are all directly and in large part traceable to his reactions to his French " milieu." At the same time, no one can deny that Le Bon has pointed out tendencies, conditions, and psychological laws which had previously been overlooked or undeveloped, and, when his works are read with the understanding which allows the discounting of his exaggerations and prejudices, they constitute an important contribution to sociological literature. It seems probable that Le Bon's contributions to social and political theory can best be understood through an examination of his main works on social psychology, noting their general doctrines briefly and devoting special attention to their bearing upon social and political problems.

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[2] G. E. Vincent, in review of Le Bon's *The Psychology of Peoples, American Journal of Sociology,* January, 1899, p. 555.

[3] Probably Matthew Arnold would have found Le Bon rather comforting reading.

2. The Psychology of Social Evolution

Le Bon's first considerable work in the field of social psychology was the volume entitled, *Lois psychologiques de l'evolution des peuples.*[4] This work purports to be a summary of the main psychological generalizations reached in his earlier works upon social evolution and the history of the civilizations of Asia, Africa, and Europe.[5] It consists mainly of what might be called psychological prolegomena to the study of history, though few historians or psychologists would agree to all of his generalizations. His main theme is the nature and importance of national character, or " the soul of a race," in the explanation of history and modern social problems.[6]

This all-important " racial soul " is the sum total of the moral and intellectual characteristics that lie at the foundation of the civilization of a race and determine the course of its evolution. The soul of the race finds objective expression in the totality of the type of civilization which distinguishes the particular race. "The moral and intellectual characteristics, whose association forms the soul of a people, represent the synthesis of its entire past, the inheritance of all its ancestors, the motives of its conduct."[7] In the formation of the racial soul the influence of the dead is preponderant. The racial soul is primarily unconscious; it underlies the rational phases of national thought and is, on that account, much more dominating in its influence. It is over this field of unconscious motives of conduct that the influence of the dead is particularly potent. "A people is guided far more by its dead than by its living members. It is by its dead, and by its dead alone, that a race is founded. Century after century our departed ancestors have fashioned our ideas and sentiments, and in consequence all the motives of our conduct."[8] These psychological characteristics which go to make up the soul of a race are composed of a relatively few fundamental ideas which are very permanent in character and are changed only very slowly, except through the effect of racial inter-mixture. Only the more superficial and secondary characteristics of a race are modifiable with any degree of rapidity.[9]

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[4] Paris, 1895, English translation, N. Y., 1898, reviewed by Professor Vincent, *Amer. Jour. Soc.,* January, 1899, pp. 554-6. Cf. also Bristol, *Social Adaptation,* pp. 133-138.

[5] *The Psychology of Peoples,* p. 230.

[6] Le Bon's notions of social evolution and of the contrasting characteristics of the French and Anglo-Saxon peoples are but a holdover of the doctrines of the " Romanticists," given a modern dress through a dash of psychology.

Le Bon contends that these races may be classified psychologically as well as anatomically. There are primitive races, or "those in which no trace of culture is met with," made up of peoples like the Fuegians; inferior races composed mainly of negroes; average races represented by the Mongolians; and superior races mainly exemplified by the Indo-European peoples.[10] The higher the race the more highly differentiated it is psychologically and the more superior minds it contains.[11] Though there may be a vast difference in the intelligence of the different individuals that go to make up a superior race, nevertheless the race is practically uniform in those fundamental psychological factors which determine its character.[12] This explains why it is that national character and not intelligence is the dominant factor in social evolution—why the English can hold in subjection the millions of India who equal or surpass the English in pure intelligence.[13] Even the most superior races cannot change the fundamental elements of their civilization with any facility. Cross-breeding of racial stocks is the only agency which will effect a rapid and fundamental change in national character. Social and physical environment have little strength as compared to heredity and inheritance.[14] " The history of civilizations is thus composed of slow adaptations, of slight successive transformations. If these latter appear to us to be sudden and considerable, it is because, as in geology, we suppress the intermediate phases and only consider the extreme phases."[15] According to Le Bon's view, therefore, history is nothing more than a product of racial character:

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[7] Op. cit., pp. 5-6, 63-64.

[8] Ibid., pp. 11, 15-16, 51ff. Le Bon admits that it is practically impossible to find a pure race at the present time in the sense of anatomical purity, and states that what he refers to are "historical races"—a product of psychological rather than physical evolution. An historic race is produced when two or more not too dissimilar peoples are brought together in fairly equal numbers and subjected to the same environmental conditions for a very long period of time. The apparent confusion which might arise from identifying the soul of a race with national character he explains by taking the ground that nations are normally subdivisions of some well-defined historical race and thus partake of the general characteristics of the race of which they form a part.

[9] Ibid., pp. 17ff, 154ff, 167ff.

[10] Ibid., pp. 25ff, Cf. Boas, *The Mind of Primitive Man*, Chap. I.

[11] *Psychology of Peoples,* pp. 39ff, 232.

> History in its main lines may be regarded as the mere statement of the results engendered by the psychological constitution of races. It is determined by this constitution, just as the respiratory organs of fish are determined by their aquatic life. In the absence of a preliminary knowledge of the mental constitution of a people, its history appears a chaos of events governed by hazard. On the contrary, when we are acquainted with the soul of a people, its life is seen to be the regular and inevitable consequence of its psychological characteristics. In all the manifestations of the life of a people, we always find the unchangeable soul of the race weaving itself its own destiny.
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> The idea that institutions can remedy the defects of societies, that national progress is the consequence of the improvement of institutions and governments, and that social changes can be effected by decrees—this idea, I say, is still generally accepted. . . . The most continuous experience has been unsuccessful in shaking this grave delusion. . . . A nation does not choose its institutions at will any more than it chooses the color of its hair or its eyes. . . . Centuries are required to form a political system and centuries needed to change it. Institutions have no intrinsic virtue: in themselves they are neither good nor bad. Those which are good at any given moment for a given people may be harmful in the extreme for another nation. . . . To lose time in the manufacture of cut-and-dried constitutions is, in consequence, a puerile task, the useless labor of an ignorant rhetorician. . . . The conclusion to be drawn from what precedes is, that it is not in institutions that the means is to be sought of

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[12] Ibid., p. 46.

[13] Ibid., pp. 33-34, 46-47.

[14] Ibid., pp. 53ff, 56ff, 81ff.

[15] Ibid., p. 96.

> profoundly influencing the genius of the masses. . . . Peoples are governed by their character, and all the institutions which are not intimately modelled on that character merely represent a borrowed garment, a transitory disguise.[16]

The soul of a race is very visibly and strikingly manifested in its political institutions. Applying this idea to French conditions he finds that all the French parties, whatever their name, pursue the identical end of attempting to absorb the individual in the state and destroy individual initiative. In England and the United States, however, a different type of racial soul leads all parties to favor individual initiative at the expense of state-activity. All this goes to prove that, in reality, forms of government and political institutions in general count for very little in comparison to the psychological characteristics of a race. The great historical importance of the psychological characteristics of a race is well illustrated by the conspicuous success and expansion of Anglo-Saxon colonization and political forms in America and the equally apparent failure of the Spanish in this respect.[17]

Because of the very fact that cross-breeding is the only method by which it is possible rapidly to change the character of a nation, immigration on a large scale, with the consequent interbreeding, has a very important effect upon the destiny of a nation. Roman civilization perished more as a result of the peaceful amalgamation with barbarians than as a consequence of the subsequent military invasions. The same threatening conditions are now to be detected in the wholesale immigration into America, but thanks to Anglo-Saxon superiority the Americans may, if they act in time, exterminate these present barbarians as Marius did the Cimbri. If action is long delayed America must sooner or later meet the fate of the Roman Empire and disintegrate into many small and warring nations.[18]

Aside from the violent changes in national character which may result from wholesale racial intermixture, there may come about a more gradual modification, as a result of the infiltration of new ideas.[19] A new idea always arises in the mind of an individual who attracts a few enthusiastic disciples who aid him in zealously affirming its truth without analysis or discussion. But this soon leads to a wider and wider discussion of the merits of the idea by the public. If it gains ground it is spread by contagion and imitation throughout the society, and in time the group becomes as obsessed with the new idea as its originator was in the beginning. But, even with successful ideas, this is a very slow process. An idea never becomes a national obsession until, after years of discussion, it has filtered down into the unconscious strata of national character. When the idea has thus become a matter of dogma or sentiment it has reached its full degree of effectiveness.[20] On account of the sentimental and dogmatic nature of religious beliefs, which renders them especially amenable to fanatic support, ideas of this type have been the most powerful of all factors in the past history of mankind. To a large degree they have tended to shape the other types of beliefs and institutions.[21] In spite of the absurdities of past religious beliefs they have played an immense part in social control and in giving solidarity to society. There can be no doubt that the present tendency towards social dissolution is partially a result of the decay of the religious beliefs that society has outgrown, but which have not been supplanted by a new body of religious thought.[22]

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[16] Ibid., pp. 129-130; *The Crowd,* pp. 97-101. Cf. Ward, *Pure Sociology,* pp. 184-193; 544-575; *Applied Sociology,* pp. 13-17.

[17] *The Psychology of Peoples,* pp. 130ff. "This terrible decadence of the Latin race, left to itself, compared with the prosperity of the English race in a neighboring country, is one of the most sombre, the saddest, and, at the same time, the most instructive experiences that can be cited in support of the psychological laws that I have enunciated." Ibid., p. 152. Le Bon's views are seriously compromised by the fact that he overlooks the historical elements in the situation.

[18] Ibid., p. 154ff.

[19] Ibid., p. 167ff.

Only by an application of social psychology can one comprehend the relation of leadership to social progress. While practically every real and substantial advance in culture is the result of the services of the *élite* in any society, they usually do little more than to synthesize the latent possibilities and tendencies of the age. Again, the truly *élite* never bring about any sudden or startling changes; they affect civilization only gradually. The great dramatic changes in history are the work of fanatics.[23] "At the bidding of a Peter the Hermit millions of men hurled themselves against the East; the words of an hallucinated enthusiast such as Mahomet created a force capable of triumphing over the Greco-Roman world; an obscure monk like Luther bathed Europe in blood. The voice of a Galileo or a Newton will never have the least echo among the masses. The inventors of genius hasten the march of civilization. The fanatics and the hallucinated create history."

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[20] Ibid., p. 169ff.
[21] Ibid., p. 190ff.
[22] Ibid., pp. 197-198.
[23] Ibid., p. 199ff.

As nations are built up by the formation of a national character, so they perish with its dissolution. As an organism decays when it no longer functions, so a nation disintegrates when it has lost its character. Le Bon finds at present many symptoms of decay among the Latin races of Europe, among which socialism, or the cult of state-worship, is the most menacing.[24]

Stated with their bold dogmatism and unobscured by being buried beneath a mass of erudition of another sort, these propositions of Le Bon sound rather novel and startling, but they are by no means new. His idea of national character as a vital reality, his belief in the superiority of certain races, and even his faith in the supremacy of the Anglo-Saxon or Germanic peoples is at the bottom identical with the doctrine preached in the works of Burke and the "Romanticists," of Freeman, Kemble, Green, and Stubbs in England and of Ranke, Waitz, Sybel, Droysen, and Treitschke in Germany. Further, his doctrine of the predominant importance of the ideas and beliefs of a people in their historic development is but an exaggerated statement of the conception of history as a socio-psychic process, stated by Lamprecht in Germany and now championed in America by Professor Robinson, and which seems likely to be one of the most fruitful lines of historical investigation for years to come.[25] This work on the "psychology of peoples" illustrates the main characteristic of Le Bon's sociological writings—an overworking of a very few important and perhaps not sufficiently emphasized principles of undoubted validity and significance.

3. The Psychology of the Crowd

Le Bon's second excursion into the field of social psychology was embodied in his most popular and well-known work, *La psychologie des foules*.[26] The ideas of this work, combined with the theories expressed in his *Psychology of Peoples,* constitute all the really important socio-psychological conceptions developed by Le Bon. His other and later works are but the reiteration of familiar doctrines and an application of them in greater detail to specific, historic, social, economic, or educational problems. Le Bon introduces the reader to his second work by a reiteration of the main thesis of the earlier book, namely, that the really significant historic changes are to be seen in the modification of human thought. "The only important changes whence the renewal of civilization results, affect ideas, conceptions, and beliefs. The memorable events of history are the visible effects of the invisible changes of human thought."[27] Few intelligent historians would disagree with this statement, however much they might dissent from some of Le Bon's exaggerated applications of the doctrine. The present era is a critical period, inasmuch as it is characterized by far-reaching transformations of human thought. The social, religious, and political beliefs upon which our civilization has rested are giving way before the growth of modern science and industry. The coming age seems destined to be the era of crowds, as a result of the growth of cities, the extension of the suffrage, and the improvement in communication. "The divine right of the masses is about to replace the divine right of kings." This prospect does not promise well for the future, for crowds are mainly given to violent action and are little adapted to producing careful and accurate thought. Civilizations have always been created by a small intellectual aristocracy, while the rule of crowds has ever characterized periods of decline and disintegration. There is no longer any hope of being able to overcome the rule of the masses. The popular movement has gone beyond the point where it might have been arrested. The only practicable method of meeting the inevitable tendency is for statesmen to acquire a knowledge of crowd psychology and thus be able to reduce the evil as much as possible through a scientific manipulation of the situation. Le Bon modestly suggests that it is the purpose of his treatise to make this much needed information available for the first time.[28]

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[24] Ibid., pp. 204, 211ff, 219ff.

[25] Lamprecht, *What is History?* chaps. i-ii; Cf. Robinson, *The New History,* chaps. i, iii, iv, viii. See also the article on "Psychology and History" in the *American Journal of Psychology,* October, 1919.

[26] Paris, 1895, translated *The Crowd;* London, 1896, reviewed by A. F. Bentley, *Amer. Jour. Soc.,* January, 1897, pp. 612-614.

In defining what he means by a crowd, Le Bon makes it clear that he does not regard a crowd as a mere group of individuals assembled in physical contiguity, but rather such an organized aggregation that a collective mind is formed and the conscious individuality of the assembled persons is practically lost. Not only may some aggregations fail to constitute a crowd, but on the other hand a whole nation may, with proper facilities for communication and a proper degree of psychic stimulation, assume all the essential characteristics of a crowd.[29]

Def of crow

Le Bon proceeds to enumerate the main psychic traits which, in general, characterize crowds. A crowd possesses a collective mind and a psychic unity which alters the normal emotion, thoughts, and conduct of the individual to a considerable degree. The crowd mind is not the average mind of its members, but is rather a complex of new traits which arise from the combination. The subconscious mind plays the predominant part in the psychic activity of crowds, and, as the subconscious is mainly charged with highly emotional qualities, with the archaic social inheritance of the race, and with the more common and instinctive content of the mind, these very qualities are brought to the front in the mental operations of crowds. In a crowd, therefore, the individual members are assimilated to a common mediocrity and the crowd is never capable of engaging in activities requiring a high degree of intellectual effort.[30] These new psychic traits which arise in the individual, as a result of his participation in a crowd, are brought about by several factors. In a crowd an individual feels a sense of invincible power quite absent in his normal isolated state. His susceptibility to suggestion is very greatly increased, and, as a result of this, the sentiments of a crowd are ultra-contagious. The net result of these factors is that in a crowd the individual behaves in a sort of half-conscious and hypnotic manner. " We see, then, that the disappearance of the conscious personality, the predominance of the unconscious personality, the turning by means of suggestion and contagion of feelings and ideas in an identical direction, the tendency to immediately transform the suggested ideas into acts ; these we see, are the principal characteristics of the individual forming part of a crowd. He is no longer himself, but has become an automaton who has ceased to be guided by his will." At the same time, however, the action of a crowd under certain conditions may not be of an inferior sort. Owing to its tendency to act swiftly in response to a vigorous suggestion it may perform an heroic act if the suggestion it receives is of the type to promote such activity.[31] After thus analyzing the general psychic characteristics of crowds, Le Bon practically exhausts the list of qualifying adjectives in enumerating the special psychic traits of crowds. He finds that they are impulsive, mobile, irritable, suggestible, credulous, ingenuous, prone to exaggeration, intolerant, dictatorial, conservative, capable of entertaining contradictory ideas, of inferior reasoning powers, possessed of an abnormally sensitive imagination, religiously tenacious of a conviction, and likely to hold fundamental convictions with great firmness, while exchanging superficial opinions with amazing alacrity.[32]

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[27] *The Crowd*, 8th edition, London, 1913, pp. 13-14.
[28] Ibid., pp. 14-23.
[29] Ibid., pp. 25-27.

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[30] Ibid., pp. 29-32.

[31] Ibid., pp. 33-38.

Le Bon's chapter on " The Leaders of Crowds and Their Means of Persuasion " is of interest as bearing upon his later discussion of the methods of modern political leaders. He finds that the leaders of crowds are almost uniformly rhetoricians or agitators obsessed by an idea, rather than careful thinkers, and that they tend to be very despotic in their methods of control. The successful leader gets the crowd to accept his belief by constant, dogmatic and repeated affirmation of his conviction without any attempt at reasoned analysis. Once an idea is accepted by a crowd it spreads with great rapidity by contagion and imitation. Leaders of crowds maintain their control by their prestige, which is either acquired by wealth or position or is a gift of nature. Napoleon possessed the quality of natural prestige to such a degree that it was sufficient to make an emperor out of his obscure and mediocre nephew nearly half a century after his glory had passed.[33]

Le Bon then proceeds to apply his theory of crowd leadership to an explanation of the method of persuasion of electoral crowds. In the first place, the candidate must possess sufficient prestige, as a result of ability, reputation, or wealth, so as to be able to force himself upon the electorate without any question or discussion of his lack of merit. Next, he should vigorously affirm, without attempting to prove, that his opponent is a scoundrel, having been guilty of several crimes. Then, he should flatter the electorate without any limit, making wide use of sonorous phrases condemning the wealthy and powerful and praising the virtues of the masses. While a candidate's written platform should be rather vague and moderate, he may make the most extravagant verbal promises, for the electorate always forgets them after the election. The voter forms no independent opinions, but has them forced upon him ready-made by the party leaders and orators. The guidance of the masses is a vital factor in modern civilization, for there is no longer any hope of destroying the doctrine of the sovereignty of the masses which has now become well-nigh a religious dogma. Even if it were possible to restrict the suffrage to the intellectual aristocracy there would be no reason to expect any decided improvement, since, by the laws of the psychology of peoples and crowds, assembled individuals tend to be ruled by their emotions and not by their intellectual faculties, and the emotional traits of the most erudite do not differ materially from those of the average individual. " In a crowd men always tend to the same level, and, on general questions, a vote recorded by forty academicians is no better than that of forty water-carriers."[34]

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[32] Ibid., pp. 38-88, 160-174.

[33] Ibid., pp. 133-159.

Parliamentary assemblies are another example of modern political phenomena which can only be explained by an application of the laws of social psychology. The whole system rests on the erroneous assumption that a large number of individuals are more likely to arrive at an accurate solution of a public problem than a small group. Parliamentary assemblies normally manifest most of the characteristics of a crowd. " The general characteristics of crowds are to be met with in parliamentary assemblies: intellectual simplicity, irritability, suggestibility, the exaggeration of the sentiments, and the preponderating influence of a few leaders." Perhaps the most significant special characteristic in their procedure is their almost invariable tendency to attempt to solve the complicated problems of public policy by a few simple formulas and by direct legislation. On questions of local or personal interest legislators have fixed and unalterable opinions. On general questions of policy and procedure, however, they are open to the suggestion of leaders, and if these leaders happen to be of about equal power but represent different opinions, the legislature will be conspicuous for its indecision and inconsistency, as it will vary in its response to the powerful sources of suggestion. The leaders enjoy their power as a result of their prestige or ability to arouse enthusiasm, and not on account of the logic or profundity of their arguments. Most great parliamentary leaders have been vigorous men possessed of the gift of florid oratory, but with relatively little breadth of mind or intellectual capacity.[35] While the action of a parliamentary assembly is normally on a little higher plane than that of the ordinary crowd, in times of excitement it degenerates into a mob, as in the case of the assemblies of the French Revolution. The sole salvation of parliamentary government lies in the fact that the laws are usually drafted by specialists and experts, and legislators merely vote for laws rather than frame them.[36] In spite of these shortcomings Le Bon concludes that, after all, parliamentary government is the best which has yet been devised. Its chief evils, in addition to the presence of the crowd psychological situation, are two—financial waste, resulting from the fear legislators have of opposing financial bills lest they lose their influence and their local appropriations, and the restriction of individual liberty, due to the worship of state-activity and the faith in the efficacy of the state to solve all social and economic problems. Le Bon finds that an excessive trust in the state is a symptom of national decline and thus ends this work in the same vein as the conclusion of the *Psychology of Peoples,* namely, that the Latin peoples who put their trust in the state are beginning their period of final decay.[37]

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[34] Ibid., pp. 202-212.

[35] Ibid., pp. 215-226.

[36] Ibid., pp. 226-231.

There is little that needs to be added in the way of comment on Le Bon's doctrines regarding the psychic traits of crowds. His general observations are in the main correct, but are highly colored and overworked. His treatment is doubtless inadequate, as he is guilty of the same faults that he finds in legislators, namely, of trying to solve a complex problem by a few simple phrases and formulas. As Professor C. H. Cooley very well says on this point:

> The psychology of crowds has been treated at length by Sighele, Le Bon, and other authors who, having made a specialty of the man in the throng, are perhaps somewhat inclined to exaggerate the degree in which he departs from ordinary personality. The crowd mind is not, as is sometimes said, a quite different thing from that of the individual (unless by the individual is meant the higher self), but is merely a collective mind of a low order which stimulates and unifies the cruder impulses of its members.[38]

4. Socialism in the Light of Modern Psychology

Le Bon applies the fundamental doctrines of the *Psychology of Peoples* and the *Crowd* to an interpretation of socialism in the third of his volumes on the psychology of modern social tendencies.[39] From the standpoint of social psychology there is very little indeed in this work which had not been suggested in the earlier volumes. The text opens in a strain strikingly similar to the doctrines expressed at the outset in *The Crowd.* A nation is controlled by a few fundamental ideas, the changes in which alone can effect any serious alteration in a civilization. Institutions are the effect and not the cause of the psychic traits of a nation. We are now in the midst of a critical period in history in which these basic ideas are in the process of modification.[40] Le Bon states that no one has yet analyzed socialism from the psychological standpoint and he volunteers to supply the lacking treatment. He finds plenty of causes for the recent growth of socialism: " The demoralization of the upper strata of society, the unequal and often very inequitable partition of wealth, the increasing irritation of the masses, requirements always greater than enjoyments, the waning of old hierarchies and old faiths—there are in all these circumstances plenty of reasons for discontent which go to justify the rapid extension of Socialism."[41]

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[37] Ibid., pp. 231-239. He strangely ignores the development of Socialism in Germany. The development of state-socialism in England since 1905 has been subsequent to the publication of Le Bon's work on crowd psychology.

[38] *Social Organization,* pp. 149-150. Le Bon's whole treatment of social psychology should be tempered by a reading of pages 61-205 of this work.

[39] *La psychologie du socialisme,* Paris, 1898; translated *The Psychology of Socialism,* N. Y., 1899, and reviewed at great length and with a large amount of psychological acumen by George H. Mead, *Amer. Jour. Soc.,* November, 1899, pp. 404-412.

Socialism, properly to be comprehended, must be viewed under four different headings; it is a political doctrine, an economic theory, a philosophic conception, and a belief. Its greatest strength, however, lies in its power as a belief.[42] Its importance as a belief arises from the fact that it appeals to the emotions rather than to reason, being in fact a new variety of religious appeal. It is a somewhat inferior type of religious ideal, however, for it does not appeal to the higher attributes of man's nature. Its power in this field grows out of the fact that it came at an opportune moment when the old religious beliefs were in a state of disintegration and before the new faith of the future had been developed.[43] But, aside from its primary appeal to the material elements of man's nature, socialism has a more fundamental weakness as a religious belief, in that it can give no promise of a future life and must make good its promises here on earth. This, however, it can never do, since its ideal is in fundamental conflict with those basic laws of psychology, economics, and political science which are beyond the control of man. Therefore, owing to the fact that it cannot realize its ideal, socialism will inevitably be discredited and begin to decline from the moment it has come into power in any country. The only good that socialism can effect as a religious belief is to act as a dissolving force which will clear away the wreck of the old religious beliefs and make way for the newer and sounder faiths that must characterize the future of humanity.[44] Reduced to their most fundamental terms all the varieties of socialism may be regarded as the cult of state-worship maintained by supporters of the obsession that civilizations are made and altered by institutions and governments. They all advocate the doctrine of "collectivism"—collective ownership of wealth and collective control of industry, a complete absorption of all social activities by the state and the consequent suppression of all individual initiative and liberty, leading to the complete dictatorship of the state. In reality, the whole movement is in large part a futile protest against the inequalities of ability which have always existed and inevitably will continue to exist as long as humanity remains.[45]

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[40] *The Psychology of Socialism,* pp. 1-2.

[41] Ibid., pp. vii-viii, 16.

[42] Ibid., pp. 3-4.

[43] Ibid. Introduction, pp. ix-xii, and pp. 85-103.

[44] Ibid., pp. 83-84.

All this Le Bon holds to be but the logical expression of the fundamental psychological traits of the Latin peoples, who, whatever their form of government, invariably favor the exaggerated rôle of the state and the suppression of individual liberty. They have not recovered from their old delusion that society can be revolutionized by decrees and constitutions.[46] The Latin mind has been prepared for this view for centuries and its present institutions are admirably designed to perpetuate it. This general attitude is favored by the Latin system of state education, with its mechanical drill in dead languages, its uniformity, and its lack of adaptability to individual variations and initiative; by its political institutions and doctrines which always laud the cult of state activity; and, finally, by its religion based on coercive and inflexible dogmas.[47] A proof of the assertion that socialism is essentially an expression of the Latin racial character is to be derived from the experience of the Anglo-Saxon and Germanic peoples in regard to socialism. In none of these countries has socialism assumed the revolutionary and doctrinaire form that it has maintained in the Latin countries. In Germany extreme socialism has gained a very slight foothold. It has there tended to become an opportunist movement under the form of social democracy or parliamentary socialism, in spite of the relatively ideal institutional adaptability of Germany to the socialistic regime.[48] In England and America socialism has not flourished among the native population because it is opposed to the national character, which favors individual liberty and private enterprise. In America, however, there is developing a great army of socialists composed of recruits "from the ever increasing flood of immigrants of foreign blood, without resources, without energy, and without adaptability to the conditions of existence in their new country." The only solution Le Bon can see for this difficult problem in

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[45] Ibid., pp. 27-30.

[46] Ibid., pp. 33-36, and Book III, particularly pp. 140-148.

[47] Ibid., pp. 140-166.

[48] Ibid., pp. 104-110. In this generalization of Le Bon there are serious historical errors in matters of detail.

America is for the native Americans to arise and exterminate this undesirable multitude.[49]

Taken apart from all specific aspects socialism may be regarded in its most fundamental and general sense as the attempt on the part of the unadapted in modern civilization—"the unutilizable superfluity"—to make a place for themselves, and, if successful, to seize for themselves all the fruits of our modern civilized life to which they have not contributed a whit. The most dangerous aspect of the situation is the fact that we not only have at present a vast number of these unadapted individuals, but that our society is so constituted that it is increasing this number at an alarming rate. It will be the main social problem of the future, to which task socialism is obviously unequal, to care for these unadapted.[50]

Le Bon thus arrives at the conclusion that the advent of the socialistic régime would not bring the millenium in its wake, but its result "will be hell, a terrible hell!"[51] Nevertheless, socialism must be tried out in some country in order to convince the modern world of its absurdities. At the same time, it is the duty of every patriotic citizen to prevent the experiment from taking place in his own country. The *élite* in modern society must oppose the leaders of the socialistic movement by making use of the fundamental principles of crowd leadership—affirmation, repetition, contagion, and prestige. The task would not be difficult if there were sufficient desire and will power manifested by able citizens. Especially urgent is the necessity for the reform of the Latin system of education. If the *élite* do not rally to their duty the present inhabitants of the Latin countries may make proper preparations "to give place to more vigorous peoples, and disappear from the face of the earth."[52]

Le Bon's treatment of socialism, like his analysis of national and crowd psychology, suffers from an oversimplification of the factors involved from serious indifference to historical facts, and from an exaggeration of many of the undoubted defects which exist in the socialistic program. Many of his criticisms of the psychology of the movement rest upon a substantial amount of truth, but his account of the historic factors involved in the origin of socialism is lamentably weak, and the economic, political, and philosophic analysis suffers from the excessive emphasis upon the psychological factors and from his extremely evident bias against state-activity and proletarian democracy. Le Bon's defense of individualism and his attack upon state-socialism in all of his works constitute the most frantic and dogmatic psychological defense of *laissez-faire* which has yet appeared.

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[49] Ibid., pp. 111-125. Perhaps Le Bon will regard the present attempt to deport members of the Communist Party in America as a diluted substitute for his solution of the problem of socialism in this country.

[50] Ibid., pp. 358-383.

[51] Ibid., p. 406.

[52] Ibid., pp. 411-414.

5. The Psychology of Politics and of French Political Tendencies

After abandoning the field of social psychology for a decade, during which time he devoted himself to a study of physics and produced his volume on *The Evolution of Matter,* Le Bon resumed his earlier studies in his *La Psychologie politique et la défense sociale.*[53] This is essentially a study of the general tendencies of modern French political life, and hence has a limited bearing upon the general problems of political psychology. Indeed, it is unreliable even as a study of French conditions unless the reader is aware of Le Bon's decided " anti-patriotic " and " class " bias, which was clearly in evidence in his earlier works. Again, it should be borne in mind that Le Bon is constantly on the alert for instances which will confirm his preliminary assumptions and does not confine himself to a purely objective study of actual tendencies and conditions. There is a distressingly small amount of new theoretical material in this volume, and Le Bon excuses his repetition of his earlier dogmas on the ground that repetiton is the most powerful agent to produce conviction as to the truth of an argument.[54] Le Bon justifies the need of such a work as he had written by stating that while a knowledge of political psychology is absolutely essential to a successful statesman, the present available information on this subject exists in nothing but a few over-simplified formulae which are derived from experience and tradition.[55] He asserts that Machiavelli's *Prince* is the only real treatise on political psychology that has yet appeared, and, as this is somewhat out of date, he generously applies himself the task of supplying this serious gap in human knowledge.[56]

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[53] Paris, 1910, reviewed by Prof. Vincent, in the *Amer. Jour. Soc.,* Sept., 1910, pp. 267-269.

[54] Op. cit., pp. 11-12.

[55] Ibid., p. 5.

[56] This naive oversight or omission, ignoring as it does the previous and much more valuable works of such writers as Bagehot, Tarde, Durkheim, Ratzenhofer, Simmel, Ross and Giddings in the field of political psychology, is not a high testimonial to the knowledge of the literature of the subject possessed by either Le Bon or his adviser, Ribot.

The work opens with a reiteration of the familiar dogmas that civilization is the product of national character and not of institutions; that it is futile to attempt to remodel society by transforming institutions; that government has great powers for destruction but very feeble ability in constructive work; that the place of reason in the psychic traits of society is very small; and, finally, that in France there is in reality only one political party, the sole aim of which is to increase the scope of state activity. Aside from these premises that are basic in all his works, Le Bon expands another conception which was suggested in his *Psychology of Socialism*, to the effect that the former rule of kings and laws is now being replaced by the domination of economic forces.[57] This doctrine is important in his theorizing, for he finds that most of the dangerous tendencies in modern political life, both despotic and anarchical, are the result of economic causes operating upon or through national character.

In discussing the subject of the psychology of laws and the effect of ill-advised legislation, Le Bon points out the fact that all real and valuable laws are nothing but the codification of customs previously recognized in judicial action. The rôle of law-making should, then, be confined to the codification of persistent and well-established customs.[58] The legislator who attempts to change the fundamental trends in social evolution does not differ fundamentally from Xerxes who whipped the sea as a punishment for the loss of his ships. These ill-advised laws, passed under the obsession that state-activity can remedy all social evils and maladjustments, not only fail to accomplish the intended results, but also create new evils which are often more menacing than those which the laws sought to correct.[59]

In their fundamental reality, political activities have never been anything except a struggle between phantoms. History looked at from a broad viewpoint appears as a continuous effort on the part of the people to create and destroy phantoms. These phantoms are of three main classes. The most powerful type is to be seen in the great religious, philosophical, and political beliefs of history; the intermediate type is represented by the myths which grow up about alleged heroes like Ulysses, Alexander, Caesar, Charlemagne, Barbarossa, and Napoleon; and the lowest variety is manifested by the petty and transient phantoms that guide the ordinary beliefs and daily activities of the masses.[60] These phantoms gain power in proportion to the fear they create. The growing power of the working-classes has made them feared by the government, and, as a result, the government of France for twenty years has been solely in the interest of this class. Among the Latin peoples one of the most important transformations of phantoms in recent years has been that of the substitution of the phantom of the divine right of the state for the older phantom of the divine right of kings. This has made the present what may be called the era of functionaries.[61]

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[57] Ibid., p. 15.

[58] " La coutume résulte des nécessités sociales, industrielles, économiques de chaque jour. La jurisprudence les fixe. La loi les sanctionne." Ibid., p. 45.

[59] Ibid., pp. 50ff.

Le Bon applies his psychological analysis to what he regards as some of the more obvious present-day political " phantoms." He believes that the modern agitation for the cessation of war is a threatening sign. Wars are apparently not on the decrease and civilization seems powerless to remove their causes. Therefore, they may be regarded as necessities imposed by nature to secure national vigor and discipline.[62] The important services of wars are overlooked by the " sentimental pacifists," since, in reality, wars are the main forces which create and give stability to national character, and they are the most powerful of stimulants in developing the moral life and industrial activities of a nation. The detrimental effects of an enforced peace are to be seen in the cultural stagnation and the famines in India during the period of artificial peace imposed by the British régime. Thus the ill-advised humanitarianism of pacifists is really a menace to the public welfare. In the place of political wars, there is an equally deadly, though bloodless contest going on in the economic strife between nations.[63]

Again, the French system of education with its mechanical uniformity in the technique of instruction, its antiquated subject-matter, and the tyrannical state control of university instruction is contributing to the intellectual decay of France, when in reality it should be the fountain head of its intellectual life. While it is true, as Leibnitz maintained, that a proper system of education is able to transform a people in a century, it is equally true that a poorly designed and deadening system can bring about national degeneration in an equal period.[64]

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[60] Ibid., pp. 61-62.

[61] " L'Etatisme a pour expression et soutien le fonctionnarisme." Ibid., pp. 69ff.

[62] Ibid., pp. 84ff.

[63] Ibid., pp. 91-102. This section of Le Bon's work should be compared with Nicolai's *Biology of War;* see also his own revised post-war views, section 9 below.

Le Bon next proceeds to investigate the question as to whether there is any scientific basis for the modern extension of political power among the masses. He decides, in agreement with Faguet, that this tendency is in direct opposition to the dictates of all intelligence and experience. Modern scientific and political development has tended to accentuate the differences in mentality between the *élite* and the masses. The refined methods of modern science and the exacting requirements of the conduct of modern large-scale industry call for the highest type of minds. On the other hand, the introduction of machinery into industry and the great extension of the principle of the division of labor have tended to reduce very greatly the necessity of high mentality on the part of the laborer. Yet, in spite of this, the tendency in political theory and practice of late has been towards giving greater political authority to the masses whose mental powers are continually on the decline.[65] Fortunately, however, the masses themselves have little initiative and their action depends upon the nature of the leadership which they receive. It is evident, thus, that to save society the *élite* must assume control of the leadership of the populace and check the evil influences of demagogues and revolutionists.[66] Prestige, affirmation, repetition, and contagion are the fundamental principles to be employed in the art of persuading the multitude, and the *élite* must make use of them if they hope to wrest the control of the masses from the dangerous leaders who now direct them.[67]

Parliamentary government has even become unpopular in France. This has come about as a result of the indiscretion of the different parties in trying to outbid each other in promises to the masses in the hope of being successful in elections. Naturally they have been unable to fulfill these exaggerated promises, even by the most arbitrary and debasing use of the law-making power. As a result, they have stirred up bitter animosity against the very system of government they represent.[68]

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[64] Ibid., pp. 103-117. Cf. Le Bon, *La Psychologie de l'education,* Paris, 1904. This view seems to contradict his theory of the stability of national character.

[65] " Tandis que les progrès scientifiques amenaient les élites de mentalité supérieure à diriger le mécanisme de la vie moderne, les progrès des idées politiques conféraient de plus en plus à des foules de mentalité inférieure le droit de gouverner et de se levrer par l'intermédiaire de leurs représentants aux plus dangereuses fantaisies." *La Psychologie politique,* pp. 118ff.

[66] " C'est donc aux élites à s' adapter au gouvernement populaire et à endiguer et canaliser les fantaisies du nombre, comme l'ingénieur endigue et canalise la force d'un torrent." Ibid., p. 122.

[67] Ibid., p. 136. Pages 118-141 are mainly a summary of his previous doctrines regarding the psychology and leadership of crowds.

The mind of the working-class is essentially identical with the conventional mind of the crowd, but to these general characteristics are added the possession of certain special dogmas which are the result of their peculiar history, position and interests. The most prominent of these accessory beliefs in the mind of the workingman is the notion that wealth is created by labor and appropriated by the rich, and the consequent deduction that justice requires that the present class constitution and economic organization of society shall be overthrown and the laboring classes installed in their rightful position.[69] A new and particularly menacing development in the aspiration of the masses is to be found in the pretensions of syndicalism, which proposes to substitute for loyalty to and action by the state, loyalty to a particular industrial profession, and autonomy and self-government for this profession. They desire to substitute *l'égoïsme corporatif* for *l'intérêt générale du pays*.[70]

Socialism and syndicalism are the two most dangerous tendencies in modern political life which have developed out of proletarian activity, and the French government, pressed by the pretensions and demands of these two movements, has tended steadily towards a popular despotism.[71] Le Bon examines, criticizes and contrasts what he calls the " illusions " of socialism and syndicalism. His treatment of socialism is essentially a brief repetition of the doctrines advanced in his *Psychology of Socialism* and need not be repeated here. Syndicalism is one manifestation of the general modern tendency towards the association of similar interests. While essentially a revival of the ideas of the medieval guild, it has received its vital impulse from the new conditions in industry that have grown out of the Industrial Revolution.[72] Though both socialism and syndicalism are a menace to civilization they are fundamentally opposed in principle. The former would augment the rôle of the state until its activities become all-embracing, while the latter would divide society into many autonomous professional groups and dispense with the state, thus tending more towards anarchy than towards political absolutism. This fundamental divergence between the two movements, and the struggle which will be inevitable, may have the beneficial effect of preventing an extreme development of either tendency, but there will be the accompanying danger that society will be crushed between the opposing forces.[73]

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[68] Ibid., pp. 163-175. Le Bon seems serenely unconscious of the direct contradiction between this view and the statement in *The Crowd* that a political leader could safely make the most extravagant promises and successfully rely upon the electorate to forget them after the election.

[69] Ibid., pp. 142-153.

[70] Ibid., pp. 153-162. Durkheim is much more sympathetic with certain phrases of syndicalist doctrine.

[71] Ibid., pp. 176-186.

[72] Ibid., pp. 202ff.

Le Bon next turns to a long discussion of the psychological blunders involved in the French colonial system and procedure,[74] and then analyzes the various forces in French society leading to social anarchy, enumerating the main symptoms which are indicative of such a tendency. He finds that there is a general trend towards social anarchy and an accompanying anarchic mentality; that there is an increase of crime and a spread of criminal tendencies; that the habit of assassinating rulers and statesmen is becoming common; that the persecution of religious orders is popular; that there is a dangerous enmity and struggle between the different classes in society; and finally that there is a threatening fatalistic attitude, even among the most learned, which inclines them to regard human powers as unable to conquer the tendencies in external events.[75]

Le Bon devotes the last section of his work to a discussion of the chief steps which should be taken by society to save itself from the disintegration which threatens it as a result of the many anarchical and revolutionary tendencies that he has noted, in other words to an analysis of the " défense sociale." When a national mind disintegrates the people tend to revert in mental traits to the state of intellectual barbarism from which they were raised by the formation of a national character. This return to barbarism is being witnessed in France today. Society remains apathetic in spite of the assaults upon its integrity. As a matter of fact, however, the leaders in the defense of society need more enthusiasm than the leaders of the masses who are assaulting the foundations of society, for it is harder to convince people of truth than to get them to accept error.[76] But civilization cannot be maintained without effort. The apathy of the French *bourgeoisie,* who must bear the brunt of the social defense, is an appalling contrast with the ardor of the revolutionary element. Besides renewing its vigor and enthusiasm the *bourgeoisie* must give up the ostentatious and insolent luxury which is the most potent force in stirring up the animosity of the working classes. The only real evidence of a rallying of the *bourgeoisie* to the defense of society is to be seen among its more humble members, such as the small shopkeepers who have banded together to carry on a collective and cooperative struggle.[77] To be effective the social defense must not only have able and enthusiastic leaders, but also some fundamental doctrines to guide them. The corner-stone of this program should be the ideal of national defense.[78] To this must be added the consideration " qu'un peuple ne peut vivre sans armée, sans hiérarchie, sans respect de l'autorité, sans discipline mentale." These ideals would suffice, but they must be supported by the *élite* who at present manifest an alarming degree of apathy, cynicism, and fatalism.[79]

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[73] Ibid., pp. 202-225.

[74] Ibid., pp. 226-284.

[75] Ibid., pp. 285-357. Le Bon evidently overlooks the fact that his own doctrine that events are the inevitable expression of national character which cannot be changed by legislation is of a decidedly fatalistic cast. He also seems unconscious of the fact that there is a contradiction between this position and his frequent assertion that there is a most powerful tendency in the direction of extensive state-activity.

[76] " L'erreur passionne, les froides vérités n'enthousiasment pas." Ibid., pp. 360-362.

Le Bon's somewhat questionable premises and his " antipatriotic " and " class " bias, which tend to make his conclusions rather suspicious, have already been referred to, and the detailed refutation of his specific charges against French society could be accomplished only by a detailed exposition of actual conditions. It would seem that his picture is scarcely accurate in general outline, and is even more highly distorted in matters of detail.[80]

6. THE PSYCHOLOGY OF OPINIONS AND BELIEFS

Le Bon's next venture in his system of social psychology is entitled, *Les Opinions et les croyances, genèse, évolution.*[81] From every logical standpoint this work should have been the starting point of his system, for it consists of an elaborate psychological defense of the main theses which have been the guiding principles in all his works. It seems that Le Bon has pursued the rather vicious circle of starting with some preconceived notions of social psychology,[82] using these to develop a system of social psychology, and then ending by employing the works built upon these ideas to substantiate the psychological analysis. The main thesis of this work is his familiar doctrine that opinions and beliefs have an affective and mystic, rather than a rational, origin and foundation. After dealing with the psychology of opinions and beliefs in the mind of the individual, he turns in books six to eight to a consideration of the nature and effect of their emergence in society at large.

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[77] Ibid., pp. 363-366.

[78] " L'amour de la patrie forme le véritable ciment social capable de maintenir la puissance d'un peuple." Ibid., p. 370.

[79] Ibid., pp. 371-372.

[80] Several short works tending to modify Le Bon's view of French society are Brownell's *French Traits;* Wendell's *France of Today;* Guérard's *French Civilization in the Nineteenth Century;* Dimnet's *France Herself Again;* and Bracq's *France under the Third Republic.*

[81] Paris, 1911.

Under the caption of "collective opinions and beliefs" are passed in review his stock doctrines regarding the influence of racial character in the formation of opinions and beliefs; the importance of social environment, tradition, and custom in building opinions and beliefs; the peculiarities of opinions and beliefs as held by crowds; and the relation of the mind of the individual to the mind of the group, especially in crowds.[83]

In the division of his work devoted to a consideration of the "propagation of beliefs and desires" he deals with the spreading of opinions and beliefs by affirmation, repetition, example, and prestige; the influence of mental contagion in spreading opinions and beliefs; the significance of fashion in this process; the importance of literature and the press in the propagation of opinions and beliefs; and the effect of currents of opinions and their explosion, as in the emotional outbursts at the time of Peter the Hermit, Joan of Arc, Mohammed, Luther, and Napoleon.[84]

Finally, in discussing the subject of the "life of beliefs," Le Bon considers the intolerance and dogmatism of beliefs; the impotence of reason in the formation of beliefs; the maintenance of permanent beliefs by means of mental contagion and repeated suggestion; and the slow modification of beliefs, except by the contact of different cultures. He ends by maintaining that beliefs never die, but simply change their name according to the same principle as that of the transformation of energy and matter in physics.[85]

7. THE PSYCHOLOGY OF REVOLUTIONS

Le Bon's reflections on the psychology of revolutions, which are more or less present in all his works, are brought together and expanded in the volume entitled *La Révolution française*

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[82] Drawn, as he says, from his previous studies, and said by Sighele and Tarde to have been mainly appropriated from their works.

[83] Ibid., pp. 168-193.

[84] Ibid., pp. 194-232.

[85] Ibid., pp. 233-268.

et la psychologie des révolutions.[86] He introduces this work with a brief discussion of what he calls "the psychological revision of history." His thesis is that the seeming difficulties of historical interpretation vanish as soon as one recognizes that the irrational and often involuntary beliefs, and not the rational and intellectual factors, have been the dominating influences throughout history. "The solution of the historical difficulties which had so long been sought was thenceforth obvious. I arrived at the conclusion that besides the rational logic which conditions thought, and was formerly regarded as our sole guide, there exist very different forms of logic: affective logic, collective logic, and mystic logic, which usually overrule the reason and engender the generative impulses of our conduct."[87]

In discussing the psychology of revolutions Le Bon first proceeds to classify and characterize them. A revolution is any sudden or apparently sudden transformation of beliefs, ideas, and doctrines. The real and enduring revolutions are those that transform the character of a people, but such transformations are normally so slow a process that the word evolution is more descriptive of them than the term revolution. The most important of all revolutions are the scientific. They alone are accomplished by rational factors and they are the only type which really advances civilization. Nevertheless, their gradual and undramatic character has caused their significance to be overlooked by the conventional type of historian. Political and religious revolutions, which, from their dramatic nature, attract the attention of historians, are not derived from rational influences, but from affective and mystic forces. This gives them their dynamic power, but also renders them likely to be violent, absurd, and futile. Religious revolutions are even more violent than political revolutions. In this type the participants cannot be disillusioned by the results, because the truth or falsity of their principles can only be demonstrated in another world. Religious revolutions also have the most important results of the two, for, while they do little or nothing to advance the intellectual factors of a civilization, they are the most influential medium in transforming the sentiments of a people. Religion, particularly when intensified during a revolution, gives a people a moral unity and cohesion which could be obtained in no other manner.[88]

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[86] Paris, 1912. English translation by Bernard Miall, N. Y., 1913.
[87] Op. cit., p. 15.
[88] Ibid., pp. 23-48.

Le Bon next analyzes the part that governments and the people play in revolutions. Governments are usually very feeble in opposing a revolution, giving way immediately and with little resistance. A wise and efficient government, however, may check a revolution by following a proper policy. The menacing Russian revolution after the Russo-Japanese War was thwarted by the action of the government in temporarily conciliating the discontented masses and then exterminating or exiling the fanatical leaders. Governments may at times attempt to produce a revolution by their own action, but they are rarely successful unless the national character is not yet sufficently developed to render an effective resistance to the change of institutions, as was the case with the revolutionary reforms in Russia under Peter the Great. Yet, however profound may be the apparent change in the system of government, such a revolution rarely has a serious effect upon the mental characteristics of a nation. "To create a revolution is easy, but to change the soul of a people is difficult indeed."[89]

The mental characteristics of a people play a prominent part in determining the nature of its revolutions. The more stable the mind and institutions of a nation in normal times the more violent are its revolutions, for such people are not adapted to making gradual non-revolutionary changes. A nation with a flexible, adaptable mind may not escape revolutions, but those which it experiences are usually slight and but the final stage in a long period of gradual changes. France is a good example of the former type of nation, England of the latter. Contrary to the belief of many historians, the people, strictly considered, never conceive or direct a revolution. They simply obey the dictates of leaders, though they give the aspect of violence to the movement. To be strictly accurate, the people must be differentiated when one attempts to describe the part played by masses in revolutions. The great body of peasants, tradesmen, and honest artisans, who form the solid and substantial element in the masses, take little part in the violence of revolutions. Those who are guilty of violence, under the guidance of obsessed leaders, are the "degenerates of alcoholism, and poverty, thieves, beggars, destitute 'casuals,' indifferent workers without employment—these constitute the dangerous bulk of the armies of insurrection."[90]

Le Bon finds that there are several special varieties of mentality prevalent during revolutions. Each individual may have different traits of character made more prominent at various times owing to changes in the stimulation from the social environment. In revolutions the sentiments of hatred, fear, ambition, envy, variety and enthusiasm, which are normally more or less suppressed, are given full vent. One of the most prominent types of mentality developed by revolution is what Le Bon calls the "mystic mentality." This is characterized by the attribution of a mysterious power to superior beings or forces, which are incarnated in the form of "idols, fetiches, words, or formulae." It is at the bottom of all religious and most political beliefs and is especially important during revolutionary periods. Another influential type of mentality which is conspicuous in revolutions is the "Jacobin mentality." This is based upon the mystic mentality to which are added feeble reasoning powers and strong passions. The typical "revolutionary mentality" adds to the mystic and Jacobin traits chronic restlessness and discontent—the spirit of perpetual rebellion. Finally, there is the "criminal mentality" which characterizes the degenerate anti-social class which is normally restrained by the hand of the law. This type constitutes the majority of the savage and violent element in revolutionary mobs. When one considers that revolutions are conducted under the combined direction and impulse of mystic, Jacobin, revolutionary, and criminal mentalities he can be little surprised at their violent nature, their absurd direction, or their ephemeral results.[91]

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[89] Ibid., pp. 49-59.

[90] Ibid., pp. 60-74.

Le Bon concludes his introductory and general treatment by a repetition of his overworked views regarding the psychology of peoples, crowds, assemblies, and leadership.[92] In the second part of his treatise he applies these already venerable conceptions to an interpretation of the French Revolution. While this analysis is at times most brilliant and suggestive, his method of procedure is open to the most severe criticism. He apparently reached certain general conclusions regarding revolutions from a study of the French Revolution, and then applied these views to an interpretation of this very period. Of course, this gives a high degree of apparent plausibility and concrete substantiation to his theories, but quite fails to impress the critical reader. He concludes that the general result of the French Revolution, in France, at least, was to substitute the tyranny of the state for the oppression of the individual monarch.[93]

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[91] Ibid., pp. 75-101.

[92] Ibid., pp. 102-120.

[93] Ibid., p. 286. For his general summary of the psychology of the French Revolution, see pp. 326-330.

In the concluding portion of his treatise Le Bon discusses the after-effects of the French Revolution, particularly in regard to the development of democratic ideas. The Revolution produced a crop of idealizers and theorizers who have planned the complete democratization of society. The ideal of " equality," rather than the shibboleth of " liberty " and " fraternity," has received the greatest emphasis, and is at present the pivotal doctrine of socialism. Le Bon finds that there are two distinct varieties of democracy. One is that of an intellectual aristocracy under democratic forms; the other is the popular notion of democracy based upon the ideal of equality or, perhaps better, upon the hatred of superiority. This latter variety is in direct opposition to nature's principle of inequality, and has rarely received the support of great minds. Though the popular ideal of democracy is an illusion, it has great vogue because it is a belief. The Jacobin mentality has become general in Latin countries, as evidenced by the growth of anarchy, syndicalism, the hatred of superiority and restraint, and the incessant craving for the extension of state-activity. The distinctly new element in the modern situation is the struggle between capital and labor, which is an outgrowth of the Industrial Revolution. The vicissitudes, uncertainties, conflicts, and extension of duties in modern political life have compelled most states to develop an elaborate administrative system which constitutes the real government and threatens society with the tyranny of a caste of functionaries.[94]

8. THE PSYCHOLOGY OF THE WORLD WAR

It could scarcely be expected that so important and interesting an event as the World War would fail to elicit from Le Bon a psychological explanation, and before the conflict was half over he brought forth his *Enseignements psychologiques de la guerre européene*,[95] in which he set forth with great assurance a psychological interpretation of the causes and progress of the world conflict, based on his stock theories of social psychology. The only notable new element in this work is that the extreme laudation of Teutonic racial characteristics, which was so prominent in Le Bon's earlier works, has been replaced by a thorough-going acceptance of the view of the unique perversity of the Teuton, while his anti-patriotic bias against the French has disappeared in favor of a worship of

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[94] Ibid., pp. 289-325.

[95] Paris, 1916, translated by E. Andrews as *The Psychology of the Great War*, N. Y., 1916.

French traits, as revealed by the war, which almost equals the adulation of Maurice Barrès. His main theoretical contention is that the war was primarily produced by psychological forces and that its genesis and course can be understood only by means of a psychological analysis. He stresses the importance of the affective, collective and mystic forces "forged in the dim realm of the unconscious," and holds that they have almost totally submerged the intellectual and rational factors and elements during the progress of the war. The conflict between psychic forces and tendencies in the world war is so deep-seated and comprehensive that it may produce a new psychological era in human development.

Le Bon views the war as a fundamental struggle between psychic forces, particularly those operating on an emotional and a sub-conscious level:

> The present war is a contest between psychological forces. Irreconcilable ideals are grappling with one another. Individual liberty is drawn up against collective servitude, personal liberty against the tyranny of State Socialism, old habits of international integrity and respect for treaties against the supremacy of the cannon. . . The present contest has more than one analogy with the religious wars of olden times. It is begotten of the same illusions and shows traces of the same incoherent frenzy and brutality. It is ruled exclusively by irrationality, for if reason had been able to dominate the aspirations of kings and nations, there would have been no war today. . . Never in the course of the ages has there, perhaps, been a better opportunity of seeing how men's conduct is sometimes dominated by unconscious influences whose pressure is so great that no will can withstand them.[96]

It is interesting and amusing to note that in this work the German people are represented as the exponents of state socialism and collectivism, while the French are pointed out as the champions of individual liberty and personal initiative—a complete reversal of the rôles assigned to these two nations in Le Bon's earlier works.

Le Bon's present anti-German bias appears most clearly in his analysis of the causes of the war, in which he places the burden primarily upon the Germans, whom he believes to be intoxicated with the delusion as to their superiority and their mission to rule the world:

> The victory of the Teutonic theory of the absolutism of force would carry the nations back to the most distressful periods of their history, back to the eras of violence when the law of the strongest was the sole foundation of justice. . . Like the Arabs of Mohammed's day, the Teutonic nations are deluded by a dream which

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[96] Op. cit., pp. 18-20, 173.

> makes them fancy that they are a superior race, destined first to conquer the world and then to regenerate it.[97]

Le Bon is now disposed to contest vigorously this view of German superiority:

> The German of our day is not by any means the transcendent creature whom the vanity of his historians has imagined him to be. He is the heir of the men whom Napoleon conquered at Jena with such ease, and he has nothing really superior about him except an exceedingly strict discipline and a meticulous organization which is well adapted to the needs of the present era.[98]

Yet so deep-seated is this mystic German illusion as to the imminence of Teutonic world leadership that it will take more than one defeat to free the German mind of this national psychosis:

> Whatever success Europe may win in the end over Germany's attempt at hegemony, there is no hope that it will be lasting, for the ideal of domination is one of those mystic beliefs whose duration is never brief. A nation which has been chosen by God to conquer and regenerate the world does not readily abandon such a mission, and Germany will not relinquish it until she has been defeated many times.[99]

Holding that the influence of rational and intellectual factors over group action is almost wholly an illusion, Le Bon turns to analyze what he believes to be the really fundamental causative psychological elements in producing the Great War. He classifies these as affective, collective, race-psychological and mystic. The affective forces are the chief creator of race-hatreds:

> Affective forces are among the great regulative forces of history. By the strength with which they clothe our ideas they lead us to look at things in different ways, according to the varying degrees of our sensibility. All nations possess an aggregate of inherited feelings, which are determinative of their mental orientation, which cause individuals of unlike ancestral equilibria to take different views of the same questions, and which occasion those inextinguishable race-hatreds that are among the chief causes of the European War.[100]

Added to these racial antipathies are those forces which arise from the collective psychological influences which produce that crowd-psychological state to which Le Bon has devoted so much attention. This comes to the front in times of excitement like war and destroys all that rational self-control which may exist in normal periods:

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[97] Ibid., p. 19. Le Bon apparently does not recall that a study of his own writings by Germans would have contributed materially to this inflation of the Teutonic national ego which he has described.

[98] Ibid., p. 465.

[99] Ibid., p. 468. For Le Bon's equally changed views on French national character, see pp. 21, 466ff.

[100] Ibid., p. 30.

> In ordinary times the intellectual forces of a country's best elements easily predominate over its collective forces, but in great crises, such as wars, revolutions, and the like, this is not the case; for the collective forces, which are derived from individual influences, are then capable of becoming so powerful as to sweep whole countries into an irresistible whirlpool, and to cause the emergence of new manifestations of the national mentality.[101]

This war mentality is not only produced by contemporary agencies, but is also strongly reinforced by tradition and those cultural factors which Le Bon groups together as race-psychology or national mind. Any nation has its view of its neighbors shaped by the hatreds of past generations as well as by present differences. "It is no exaggeration to say that the fiercest fighting upon our fields of battle is due to the innumerable hosts of the dead more than to the living."[102] But of all the non-rational factors determining group conduct today the most powerful are the mystic forces. Especially threatening is the mystic Teutonic obsession of world domination:

> Generally speaking one may say that mysticism is characterized by a taste for mystery, love of the supernatural, contempt for experience, and a belief that superior powers intervene in mundane phenomena. . . . The mystic forces, which science long disdained or knew not, rank foremost among the motives which rule mankind. . . Motives of mystic origin have always been the strongest of mankind's various incentives; for it is they which have created the illusions that quicken history, they under whose influence great empires have been destroyed and others founded, and they upon which even now rest the foundations of civilization. The modern world deems itself free from their sway, and yet humanity has never been more enslaved by them. . . If Europe is today in conflagration, if the flower of our youth is dying on bloody battle-fields, and if countless families are left desolate, it is because one nation believes itself destined to regenerate the world, upon which it means to impose its own mystic chimera of universal domination.[103]

After this preliminary theoretical analysis of the chief psychological factors involved in the war, Le Bon applies them to an explanation of the diplomatic negotiations which immediately preceded the war;[104] to the clarification of the methods and processes of modern warfare;[105] to the exposure of the psychic basis of German terrorism;[106] and to a forecasting of the new problems which may grow out of the World War.[107] He holds that the terroristic methods of the Germans well demonstrate the slight influence which education has over character and collective emotion. Under the excitement of the collective-psychological state the ancestral barbarism breaks through the slight veneer of civilization. The unique nature of the German *Schrecklichkeit* Le Bon holds to be due to the fact that there was in Germany less restraint upon the primordial barbarism and that this barbarism was of a lower and more animal sort than in the rest of Europe. Hence it was but natural that the inevitable savagery of war would reveal itself most rapidly and most terribly in Germany.[108]

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[101] Ibid., pp. 34-35.
[102] Ibid., p. 35.
[103] Ibid., pp. 37-40.
[104] Ibid., pp. 177-279.
[105] Ibid., pp. 283-361.

Le Bon believes that the World War is likely to prove one of those great cultural crises which alone seem able to bring into existence a new psychological era:

> It would seem that when nations reach a certain point in their history they cannot progress except under the influence of these great crises which are, perhaps, necessary for their release from the embrace of a past which clasps them too closely, and from habits and prejudices which have become too firmly established.[109]
>
> The European War marks the beginning of an era of upheaval in our manner of life, our feelings, and our thought. We have perhaps reached one of those historical periods in which, as at the time of the French Revolution, the ideals and principles of mankind are changed and a new aristocracy makes its appearance. The nations are being hurried towards a future which is not yet illumined by the faintest glimmer of light. Something they cannot foresee holds dominion over them, and political and moral ideas which they had considered incapable of change now seem destined to disappear. Theories and doctrines are vanishing one after the other, and no longer is the future assured, for the psychological forces which are locked in mortal combat are but beginning to work.[110]

9. The Psychological Changes Produced by the World War

In order to keep his social psychology thoroughly abreast of current historical developments Le Bon published a year before the Armistice a work on the psychological consequences of the World War, entitled *Premières conséquences de la*

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[106] Ibid., pp. 365-407.

[107] Ibid., pp. 411-462.

[108] Ibid., pp. 381-398.

[109] Ibid., p. 463.

[110] Ibid., p. 23. Le Bon feels reasonably assured, however, that France will lose her dangerous illusions of the past and that the rest of the world will come to view Germany in her true light.

guerre: transformation mentale des peuples.[111] In the introduction to this work Le Bon reiterates the chief propositions set forth in his *Psychology of the Great War,* namely, that the war is one of the great cultural crises of history; that it was caused and is governed almost wholly by non-rational forces; that the most ominous of these forces is the mystic Teutonic conception of world-hegemony; that the war is the price which the world is paying for its past illusions; and that these illusions are now passing and a new cultural era is about to be entered.[112]

Le Bon contends that before the war the western world was as much dominated by illusions as ever before in its history. The only difference from earlier illusions was the fact that social and political illusions had replaced the religious. Among the more dangerous and fatal of the pre-war illusions were those of state socialism, and the pacifist illusion that individuals and states are controlled by rational factors and that wars could never take place again in so advanced a civilization as that of western Europe. The war has shown the reality of the complete sway of the emotional and the unconscious psychic factors and the potency of the mystic elements. These illusions not only existed before the war, but also continued for some time afterward. It was the German ignorance of crowd psychology which led them to commit such colossal blunders as the sinking of the Lusitania, the execution of Edith Cavell, and the Zeppelin raids on British cities. The war has also borne out the theory of the Romans, the Crusaders and Napoleon, that no great military success can exist unless the soldiers are caught up in some great mystic enthusiasm which gives them a superhuman power and endurance. Experience gained during the war has wiped away most of these older illusions and has left the world wiser if much sadder for the grim lesson.[113]

From these general observations on the psychological changes occasioned by the war Le Bon turns to an analysis of the more obvious mental and social transformations which have taken place among these European states which have been at war. He contends that French degeneracy before the war was so great and deplorable that it can be fully comprehended only by a comparison with the heroism revealed in France by the crisis of the war. Before the war France was divided into selfish and competing economic and social classes; a general strike against any war was threatened; state activity and state socialism had become a fetish; politics were paralyzed and corrupted by the inferior men drawn into the French parliamentary system. The war wrought a great transformation in French society. It brought to the front much abler leaders in politics; it improved the personality of men of indifferent capacity and gave a better moral fibre to the general body of the citizens; the common experiences of all classes and both sexes during the war produced an unprecedented social solidarity in France and secured a marked advancement in the status of women; a renaissance of religious interest was evident and a unique prevalence of religious toleration existed; and there was a search for a better and higher philosophy of life, but none could be found. Not only was there a great transformation within France itself, but also the position of France in the esteem of the world was greatly improved. The pro-Germanism which existed everywhere before the war melted away and France began to appear as the guardian of civilization.[114]

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[111] Paris, 1917. No English translation has yet appeared.

[112] Op. cit., pp. 1-12.

[113] Ibid., pp. 13-45.

Germany appears to Le Bon to be the one nation that was not changed by the war in a psychological sense. The Germans seem to have retained the same illusion as to the superiority of the Teutonic race and its destiny to rule the world. Yet the Germans exhibited plenty of signs of degeneracy during the war if they had been willing to recognize them.[115] In Austria-Hungary the most interesting feature of the war was the grotesque propaganda and the intense repression of speech and news which were necessary to hold so diverse a people together as a unified fighting force.[116] Great Britain underwent a great transformation. Before the war she was isolated from the rest of Europe, was supreme on the seas, and had no fear of an attack. Hence Great Britain was more dominated by pacifism and less prepared for war than any of the major continental states. The facts of the war brought about a "terrible awakening" for the British people. They were slow in getting adjusted to the war situation and the military emergency because of their lack of preparation and because of the importance of precedent and tradition in English political life and theory. Great Britain is ruled more by her dead than other European states, and to get a new and adequate military organization she had to conquer not only the opposition of the living, but also the traditions established by the dead.[117] Russia in 1914 was on the eve of a complete

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[114] Ibid., pp. 48-140.

[115] Ibid., pp. 141ff.

[116] Ibid., pp. 196ff.

[117] Ibid., pp. 211ff.

German conquest of its industry, commerce, education, administration and army. In fifty more years a Russian war with Germany would have been impossible, and even in 1914 the German penetration well-nigh paralyzed Russian military activities.[118] The war also vitally affected the neutrals. The United States was kept out of the war by the prosperity produced through the sale of munitions, by the German propaganda, and by the inability to develop a coherent national policy on the subject of intervention. Japan not only derived great economic benefits from the war, but was also able to carry forward Japanese domination in China to an unprecedented degree. This Le Bon regards as a fact of nearly as great importance to the world as the war itself.[119]

Le Bon closes his last work with certain reflections upon the future of international relations. He contrasts the operation of the principles of law and force in human society and in history. He finds that the Latin peoples are the great champions of the reign of law, while the Teutonic peoples support the rule of force—the principle dominating the animal world. Force and anarchy have dominated international relations in the past because no common power has been found with sufficient strength to enforce international law. International law in the past has been weak and inadequately enforced, and its operation has been further handicapped by the fact that the Germans hold that in war all laws are abandoned.[120] Nor do economic losses seem adequate to prevent wars. It had been shown at great length before 1914 that wars were not good economic investments, even for conquerors, but rulers and people alike think not of the probable losses in present or future wars, but of the alleged gains in wars of the distant past, and in any war, however expensive to the nation at large, many individuals and classes become wealthy therefrom.[121] The chief hope for the future pacific adjustment of international relations must rest upon that growing interdependence of nations which renders wars more repugnant and more costly. Yet one can hope for ultimate peace and disarmament only when false ideas have been dispelled, and for this task many repeated experiences of war will be necessary, especially to teach the Germans the folly and hopelessness of their dream of world domination.[122] Les expériences répétées finiront par enseigner aux peuples l'inutilite des guerres, mais nous n'en sommes pas encore là.[123]

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[118] Ibid., pp. 222ff.

[119] Ibid., pp. 254ff.

[120] Ibid., pp. 268ff.

[121] Ibid., pp. 296-301.

[122] Ibid., pp. 302ff. Le Bon's immediate contact with the realities of war seems to have led him to modify radically his pre-war views as to the great social, moral and spiritual benefits of war.

10. SUMMARY OF LE BON'S BASIC THEORIES ON SOCIAL PSYCHOLOGY

Le Bon's salient doctrines may be summarized as follows: Every race possesses certain definite psychic traits built up by the slow accumulations of experience, and perpetuated by tradition. These psychic traits, rather than institutions, are the determining factors in civilization, the latter being simply an objective expression of the former. Among these psychic traits which constitute national character, or the soul of the race, the affective, mystic, and unconscious factors are the most powerful, quite overshadowing the conscious, rational and intellectual elements, though it is to the influence of the latter that progress is due. Without a coherent and unified group of psychic traits constituting the soul of the race, the civilization of that race cannot develop or be perpetuated. It is futile to attempt to change these fundamental psychic traits by a revolutionary or any other artificial transformation of institutions. Therefore, an excessive degree of state activity is worse than useless; law-making power should be confined to the codification of well-established and persistent customs; and government, in general, should be limited to that minimum of activity which is necessary to preserve order and secure the proper degree of mental discipline for the individual citizen. Both socialism and syndicalism are, thus, dangerous movements; the former wishing to procure excessive state activity, and the latter desiring to abolish the state altogether. Owing to such results of the Industrial Revolution as the growth of cities and the consequent concentration of population, the improvements in communication, and the extension of the suffrage, modern political life has tended to become dominated by crowds. The crowd is abnormal in its psychological characteristics, being highly emotional, exceptionally weak intellectually, and exceedingly susceptible to suggestion. It is easily guided, however, by leaders possessing prestige, who, to be successful, make use of those principles of affirmation, repetition, contagion, and imitation, whereby a crowd may be persuaded and convinced. Hence, it is highly essential that society shall assure the highest quality of leadership for crowds, and thus be able to direct their dynamic energy into activities which are conducive to the public welfare. If this is not done, and crowds are left to the exploitation of shortsighted and selfish demagogues, they must continue to be a constant menace to the integrity, well-being and even the existence of modern society. The World War was produced by the domination of the mob or crowd mind, operating over national areas and submerging all rational factors and processes. In its most fundamental aspects the conflict was a psychological struggle between contending sets of national ideas and emotions. It constituted a great psychic upheaval and transformation, destined to bring in its wake a new psychological and cultural era. War can be eliminated only when society is brought under the control of that leadership of the real intellectual aristocracy which is needed to guide the crowd mind in times of peace. Only under such leadership can society be brought to understand the growing and vital interdependence of nations, international relations be brought under the control of legal forms and processes, and those false ideas regarding the biological and social benefits of war and the conquering mission of any nation be forever destroyed.[124]

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[123] Ibid., p. 312

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[124] Le Bon has summarized his theories in a little volume entitled, *Aphorismes du temps présent.*

A PSYCHO-ANALYTICAL STUDY OF EDGAR ALLAN POE

By Lorine Pruette

The life of Edgar Allan Poe might be considered an unhappy record of that "disaster" which "followed fast and followed faster" this man of brilliant capacities till it drove him into opposition with most of the world, deprived him of the love he so inordinately craved, paralyzed his creative abilities, seduced him to seek a vague nepenthe in the use of drugs and stimulants, and, its relentless purpose achieved, cast him aside, a helpless wreck, to die from the darkened tragedy of a Baltimore saloon. Without further following such an anthropomorphic conception of fate, we must be impressed that both environmental circumstances and natural inheritance seem to conspire to cast the young poet in a rôle that is both sombre and wild, with a beauty that chills even more than it saddens.

The psychoanalyst who seeks to probe into the earliest details of life to find there the causes of many of the associations and complexes, which even then shadow forth the developments of later years, will be troubled in the case of Poe with both scanty and conflicting data. The attempt has been made in this paper to follow those accounts which seem to be stamped with the strongest degree of authenticity or at any rate probability.

Edgar Poe was born in Boston, January 19, 1809, while his parents were playing in a local theater. They were at that time very poor, so that his birth, in spite of distinguished ancestors, may be considered lowly, both from the standpoint of wealth and of the social status which players then occupied. Yet from his paternal grandfather, a general of revolutionary fame, he boasted of high lineage, and he consistently asserted his pride in being descended from a woman at once so beautiful and so noble as his mother, who both honored and was honored by her profession. Such assertions as these suggest that he spoke quite as much to assure himself as to convince others. While the reality allowed him full right to take pride in his descent, yet circumstances so contrived to cloud over that reality as to make his proud and sensitive spirit constantly alert against the possibility of an indignity.

The father of Poe was a victim of consumption, as was later his cousin-wife. Soon after the father's death, the mother died in abject want in Richmond, leaving her three small children to the mercy of strangers. Edgar was then two years old. The orphans were adopted by different families and seem to have known very little of each other. The elder brother William was, to quote from the words of a cousin, J. P. Poe, " a man of taste and genius, and wrote many fugitive verses, which have been lost, but which are said to have exhibited poetical power of a high order." He was not averse to the flowing bowl, and after his rejection as a lover, went to sea, where through recklessness he got into a sailor's scrape; he died at the age of twenty-six, leaving behind him the reputation of great but wasted talents. The youngest child, Rosalie, was so hopelessly dull that she could never attain proficiency in anything at school; she was utterly incapable of procuring her own maintenance, and after the family which had at first taken her in ceased to befriend her, she led for many years a precarious existence till she was finally admitted to a charitable institution in Washington. There she was credited with many eccentricities; she died at the age of sixty-four. Samuel Poe, a notable oddity of Baltimore, is said to have been the poet's uncle. His father, educated for the law, found such an existence unbearable, and after several rebellions separated himself from his family, marrying the English actress, Elizabeth Arnold, and adopting her profession. These facts seem to indicate a decided neurotic taint in Poe's paternal inheritance. This inferior nervous system predisposed many of the family toward flights from reality, alcoholism being their favorite form of erethism. Practically nothing is known of Poe's maternal inheritance, Elizabeth Arnold having been an orphan born at sea. She is said to have been extremely talented in singing, acting and painting. Edgar inherited her artictic ability, was very clever at drawing and passionately fond of music. Along with these gifts of the muses he inherited also those characters which were to bring him at last to an end sadder and more terrible than even that of his young actress mother, starving in Richmond.

A few weeks after Mrs. Poe's death, the Broad Street theater where she had been acting, was consumed in the awful conflagration of Christmas Eve, 1811. The death of so many distinguished people caught in this fire-trap sent a thrill of horror through the United States and was discussed in hushed tones by the Virginians for many years thereafter. The story of this event and the sight of the burnt building where his beautiful mother had so often graced the stage, must have had a powerful effect upon the dawning imagination of young Edgar, so that even at this early age there appeared to him the trilogy which so possessed his mind in after years—death, love, and beauty.

Now a tubercular father and cousin, an eccentric uncle, a drunkard brother as well as many relatives known for a "too free use of the bottle," and an imbecile sister, coupled with the peculiar genius of the poet himself, his sensitiveness to the effects of stimulants and temporary fits of insanity toward the last of his life, form fairly conclusive evidence that there was in the Poe family a decided organic inferiority. The lesion on the brain from which Edgar suffered in later life may have been either the result of syphilitic infection or apoplexy, or caused by an inherited inferior brain for which, according to Lombroso, genius is an over-compensation. (1, b.)

On the death of his mother, Edgar was adopted and baptized into the family of John Allan, a Virginia planter. From the childless wife of Mr. Allan the boy received considerable affection, though it is improbable that she was ever able to give any real understanding to her brilliant foster son. The adoptive father seems to have regarded him with an ambivalent feeling of good and ill will. In early years he was undoubtedly proud of the boy's beauty and precocity, and delighted to have him in to entertain guests after dinner by reciting long passages of poetry. The strain of such occasions must have been severe to the sensitive, excitable child. It is in such festive gatherings that he is said to have acquired, at an early age, the taste for alcohol which was later to play such a sinister part in his undoing. Dr. Bransby, under whom he studied in England for five years, described him as "a quick and clever boy" who "would have been a very good boy if he had not been spoilt by his parents," who "allowed him an extravagant amount of pocket-money, which enabled him to get into all manner of mischief." (*7*) His early boyhood, then, was passed as the spoiled child of indulgent parents, who gave him pocket money rather than love and sympathy. Poe himself, in speaking of his foster parents, says that he never received the parental affection or family sympathy for which he longed. But according to his biographer, John H. Ingram, "Throughout life a morbid sensitiveness to affection was one of Poe's most distinguishing traits," and it is highly probable that any normal affection would have seemed insufficient to the neurotic boy.

The Allans sailed for England in June, 1815, taking with them their adopted son. Edgar was placed at school under Dr. Bransby in Stoke-Newington, then a suburb of London. This historic old place, with its shadowed walks and memories of great and ill-fated lords and ladies, became the home of the child of genius during the next five years; years in which the childish imagination was quickening into life, fed on the lore of the classics and the pervading atmosphere of antiquity; years in which the passionate love of beauty turned to the loveliness of the old English town for gratification; years, too, in which the first attempts at verse-making were begun. It goes without saying that had the sensitive boy spent those five impressionable years in a different atmosphere—had he then known a normal home life and formed the normal associations toward a father and mother—his story must have been vastly different. His absence from the Allans at this period definitely precluded the establishment of ties of affection which might in later years have changed both his attitude and theirs. In 1820, again in the summer, Edgar returned with the Allans to America. The importance to the dreamy child of these two long sea voyages must have been tremendous. Sea voyages predispose to introspection, and six weeks at sea on these two occasions provided a great stimulus to the boy's imagination and love of the beautiful. Many of his stories show clearly the effect of the sea upon him, and the close observation which he must at some time have given it.

Returning to Richmond Edgar Allan entered a fashionable preparatory school. As he grew older he came to realize the anomalous position which he occupied in the Allan household and among the arrogant, aristocratic sons of Richmond. "His supremacy in intellectual training and his easy physical prowess made him the most illustrious school boy in Richmond, but he was not allowed to derive pleasure from this high eminence. His playmates, too well trained in genealogy and taught an extravagant pride of ancestry, did not let him forget that his mother was an actress and that the privileges he enjoyed and they envied were owed to the beneficence of a Scotch merchant. These reminders of his inheritance and environment forced him into an unnatural moodiness and deprived him in large part of that frank and friendly companionship based upon a sense of total equality." (9b, Intro.) Such a situation, of course, increased Edgar's rebellious pride, made him feel the necessity for defending himself against the slightest suggestion of inferiority. Feeling or imagining himself either tolerated or scorned steadily intensified his desire for superiority. He became a daring swimmer, took great risks to show himself above others, and would endure no implication that any one was his equal in his chosen sport. His early fondness for Byron possibly led him to make this identification with the British poet who was also noted for his prowess as a swimmer. His leadership in intellectual pursuits must have caused him to incur still further the enmity of his schoolmates. He appears to have made no effort to ingratiate himself with the young aristocrats; he would be sought rather than seek others. One of his schoolmates at this time says, " Poe, as I recall my impressions now, was self-willed, capricious, inclined to be imperious, and though of generous impulses, not steadily kind, or even amiable; and so what he would exact was refused to him." (7) The result of such a strained situation was to be expected. The adolescent boy withdrew more and more within himself, seeking consolation in his own dreams and mental imagery, so that by the time he reached the University of Virginia, at the age of seventeen, he is spoken of as having many noble qualities and being endowed by nature with great genius and diversity of talent but with a retiring disposition and possessing few intimate associates.

Poe was registered at the University of Virginia from February 14 to December 15, 1826. He was in good standing with the faculty and obtained distinction at the final examination in Latin and French, then the highest honors to be obtained. It had been the theory of Jefferson, whose creation the new University was, that there should be no restrictions on the students, other than the expectation that they would conduct themselves as gentlemen. The young bloods threw themselves with zest into the freedom of such a life; most of them wealthy and of high lineage, they set themselves to lead the life of reckless extravagance, of mingled bravado and chivalry, which they considered characteristic of a gentleman at that period. Gaming and drinking were indispensable to their ideas of what a gentleman should do. Poe fell readily into both these diversions, gambled recklessly and left the University owing $2,000 as debts of honor. In his drinking he was noticed to drink a glass of punch at a gulp, apparently craving the stimulation rather than the flavor of the drink. One glass, too, was said to be all that he usually desired. This habit of drinking *en barbare*, as Baudelaire calls it, he kept up all through life. (12, p. 33.) At the University, he was known for his ability in writing extravagant stories, as well as in verse-making.

The organic inferiority of both lungs and mind, if we follow the theories of Adler, demanded compensation, which the youth found in drawing and in writing stories and poems. (1, b.) His " will to power," as we have seen above, would brook no superior, nor even equal, in either physical or mental pursuits, and it was this intolerance of the claims of mediocrity which brought upon him in later life the enmity of much of the literary world. He, himself, was known to both deny and affirm his great ambition, but his ideal goal of superiority as a " litterateur " must be considered the " guiding fiction " of his life. In his poems and in his stories he consistently narrowed his attempts to the one field in which he was preeminent, the depicting of beauty and horror. His insistence, as a critic, that beauty is the sole motive of poetry, may be regarded as an attempt to place the stamp of critical approval on that which he himself did best. His feeling of " degradation " and of " inferiority " fired him with the passionate determination to be " on top." If his " masculine protest " could be satisfied in no other way he turned to the degrading of others, as witness his attacks on Longfellow and other poets as plagiarists. Poe himself believed that his absolutely unswerving devotion to truth was responsible for his scathing criticisms, but even when true, as they generally were, such criticisms represented not an abstract devotion to truth, for which he was willing to suffer deprivation and hardship, but were rather due to the pressure of his own guiding fiction, striving toward the maximation of his ego-consciousness. With women poets, Poe was seldom, almost never, critical. His desire for superiority seemed with women to take an entirely different form. He had the characteristic over-valuation of the opposite sex which, according to Adler, is invariably connected with the neurotic constitution. (1, a.)

The conclusions of Brill, based on studies of only or favorite children are quite applicable to Edgar Poe. Brill found that the adult only child shows one prominent feature, namely, he is a very poor competitor in the struggle for existence. (2.) After leaving the University the young Poe entered Mr. Allan's office, where he found the work intolerable. Just as his father had done before him, he shirked facing the hard facts of prosaic, everyday life, to seek the freer atmosphere of the artistic world, where he could obtain greater gratification for his egoistic impulses. Going to Boston, the city his mother had loved and where she said she had found her " best and truest friends," he published a thin volume of youthful poems which attracted little attention. He was then nineteen and some of the poems had been written when he was twelve. Unable to support himself by literary endeavor in the city of his birth, he joined the army as a private, where he distinguished himself by his exemplary conduct and became a sergeant-major at the age of twenty. Lieutenant Howard testified later that "his habits are good, and entirely free from drinking;" Captain Griswold considered him "highly worthy of confidence;" Colonel Worth said that his deportment was "highly praiseworthy and deserving of confidence." (6, p. 71.) His foster father then secured him an appointment to West Point, which he resigned in five months. A. B. Magruder, a contemporary, writes: "He was an accomplished French scholar, and had a wonderful aptitude for mathematics. . . . He was a devourer of books, but his great fault was his neglect of and apparent contempt for military duties. His wayward and capricious temper made him at times utterly oblivious or indifferent to the ordinary routine." (6, pp. 84-5.) His resignation not being accepted, and finding the life of the cadet so irksome, he deliberately infringed the rules and brought upon himself a courtmartial and dishonorable dismissal. His conduct and his foster father's speedy remarriage after the death of Mrs. Allan in 1829 definitely severed his relations with Mr. Allan and removed any possibility of his securing the inheritance which he had been brought up to expect. From this time on his life is a weary record of struggles for the bare necessities of life. At the time when his first tale was accepted he was in such a deplorable state as to be too ashamed of his clothes to appear in decent society. In spite of his acknowledged genius and great industry, he was condemned to the life of a very poorly paid hack writer and magazine editor, was never able to make any headway towards securing anything approaching financial independence, his young wife dying without even enough covering for her bed.

Brill further found that the only boy, constantly associating with grown-ups, "is usually precocious even in childhood, and as he grows older he finds it very hard to associate with persons of his own age." (2, p. 258.) Bohannon by his extensive studies of only children confirms this judgment. He says, "134, out of a total of 269, get along badly with others, 54 only fairly well, while only 81 seem to be normal in their social relations. When they disagree with other children it is usually because of a desire to rule. If they fail in this desire they are likely to refuse to associate with the children who cause the failure, and in a measure succeed in the wish to have their way, either by choosing younger companions whom they can control, or older ones who are willing to grant indulgence." (3, p. 489.) While Poe impressed many with his brilliancy and charm, his erratic conduct constantly estranged from him even those friends he did make—and he seemed to make enemies far more easily than friends. He had the characteristic tendency of the neurotic of beginning life anew with each new friend and each change of circumstance, feeling a new enthusiasm and another opportunity, which he lost through the vagaries of his own conduct. In this connection his will to power manifested itself through his frequent reiteration that he had thousands of friends. Bohannon found precocity also to be the most prominent trait of only children. Poe's precocity has already been mentioned. It seems probable that no other English poet has written at so early an age such good poetry.

Likewise Brill found that the only child, through the coddling of his parents, is in adult life unable to bear the slightest depreciation. Poe showed this characteristic time after time in his undignified replies to criticisms which a more evenly balanced nature would have ignored. Also the only child develops into a confirmed egotist and is conceited, jealous and envious. After precocity, Bohannon found as most prominent characteristics: selfishness, imaginativeness, affection, jealousy, mental defects, temper, self-will, vanity—all of which are more or less exemplified in the character of Edgar Allan Poe. His depreciation of so many of his literary brethren has already been noted. He himself declared, "my whole nature utterly *revolts* at the idea that there is any Being in the Universe superior to *myself!*" (7.) We see, then, the years of his life spent as the spoiled child of the Allans combining with his neurotic inheritance to effectively inhibit his making the necessary adjustments to the demands of reality. Bohannon's studies trace a further parallel. He found the only child has a less healthy and robust constitution, more commonly suffers from mental and physical defects, his social relations are characterized frequently by friction and his peculiarities more pronounced. Finally, Brill concluded from a study of 400 patients that the "majority of only children do not marry at all or they marry some near relative whom they unconsciously identify with their parent image." (2, pp. 260-1.) Edgar Poe also fits into the scheme, he marrying his first cousin, Virginia Clemm, although it seems doubtful if he married her for the above reason so much as because he was able to find security in the maternal love of her mother, as perhaps an unconscious manifestation and inhibition of the incest desire.

In considering the love life of Poe there were so many women whom he addressed in enraptured terms of adoration that it is difficult to know which, if any, had any lasting influence upon his development. His mother was to him an idealization of feminine charms, about as tangible as the vanished fragrance of unseen flowers. His boyish poet-soul, musing over the memory of a beautiful and unknown mother, whose tragic fate could not but win his sympathy, and clothing her image in all the matchless virtues of a fertile imagination, created around her name the first of those sadly lovely and unreal women who move softly through his stories and poems.

His foster mother provided his wants and even luxuries as well as some affection, but seems in no way to have satisfied his passionate desire for love and approval. It is significant that, as one of his schoolmates remarked, he was never known to take any boy into his home, always preferring to go to theirs. While in school in Richmond, at the age of fourteen, he met Mrs. Helen Stannard, the mother of a boy friend, and on her speaking kindly to him, became at once her humble adorer, offering to her the white flame of his adolescent worship. She became the confidant of all his boyish sorrows and the redeeming influence of his turbulent and passionate youth. On her death he felt himself intensely bereaved and could not endure the thought of her lying lonely in her tomb in the neighboring cemetery. So for months he is reputed to have gone nightly to the graveyard for solitary vigils by her tomb, keeping warm the memory of her who had been kind to him. His analytic mind showed always a peculiar fascination for the secrets of the tomb, a desire to probe into the last hidden process of life's disintegration. His feeling regarding his friend's loneliness was but one example of the idea by which he was haunted through life: that the dead are not wholly dead to consciousness. This theme is repeated many times in his writings; for instance, in the revival of his dead wife in the tale "Ligeia," or in the terrible return of the Lady Madeleine in "The Fall of the House of Usher." His broodings in the darkened cemetery by the tomb of the one person he felt had understood him must have laid a foundation for much that was weird and abnormal in his after life. The melodious poem, "To Helen," was inspired by the memory of this lady, whom he called "the one idolatrous and purely ideal love" of his boyhood. At another time he said, "The boyish poet-love is indisputably that one of the human sentiments which most nearly realizes our dreams of the chastened voluptuousness of heaven."

As a youth in Richmond he fell in love with S. Elmira Royster who lived opposite the Allans. She remembers him as a beautiful boy whose general manner was sad, "warm and zealous in any cause he was interested in, being enthusiastic and impulsive." The two young people became engaged, but her father, thinking her too young, intercepted all the poet's letters from the University of Richmond, and not until a year or so later when she became Mrs. Shelton did he learn why his passionate appeals had met with no response. It was at this time that Poe left Richmond and the office of Mr. Allan to seek his fortune in Boston, and it is entirely possible that the hurt to his vanity, to his insistent "guiding fiction" that he must be supreme, drove him away that he might avoid seeing the girl he desired possessed by another. In his youth he appears to have loved two other young women, one a cousin, Miss Elizabeth Herring, the other Miss Mary Devereaux, both of Baltimore, where he went after leaving West Point. Mordell (9) calls attention to the fact that by the age of twenty-three he had lost his mother, his foster-mother and Mrs. Stannard by death, and had parted from three sweethearts. These deaths and rejections Mordell held to be the cause of Poe's preoccupation with the subject of the death of beautiful women. It would seem, however, that such cause must be sought in the peculiar quality of the poet's own temperament rather than in external circumstances. The happenings of his early life undoubtedly must have conditioned his emotional reactions, but the close connection between love and death seems to have been the particular obsession on which his neurotic temperament fastened itself.

At the age of twenty-seven Poe married his fourteen-year-old cousin, Virginia Clemm. For some time previously he had made his home with his aunt, Mrs. Clemm, and from an early age Virginia had adored him. The attachment between him and his aunt was always very strong, she laboring and suffering for him as would a mother for an only son. Virginia's beauty and grace and sweetness, as well as her talent for singing, identified her with the image of the young and beautiful mother he had never known, while the maternal care and devotion of Mrs. Clemm offered him a refuge and safety from the troubles and disapproval of the outside world. His mother-image, being a creation of his own imagination, may be said to have split to take in the two personalities of his wife and mother-in-law—the one a radiant young creature, satisfying his esthetic cravings, the other tender and untiring in maternal devotion satisfying the neurotic craving for protection. In later life Poe found many women friends, women of the highest attainment and character, who were his warmest defenders and for whom he expressed the deepest sentiments of affection. Among these were Mrs. Osgood, Mrs. Richmond, Mrs. Lewis and Mrs. Helen Whitman. From the peculiar character of his " will to power " it is doubtful if individual women ever influenced him much. His nature demanded the adoration and approval of " woman," rather than sexual conquests, and he worshiped in his poems a feminine idealization to which he ascribed various names. These women are never human; they are not warm flesh and blood, loving, hating or coming late to appointments—they are simply beautiful lay figures around which to hang wreaths of poetical sentiments. His emotional interest lay in himself, rather than in outer objects; he wished to be loved, rather than to love.

The poetry of Poe reveals two things, a very considerable degree of introversion (in the sense in which Jung uses the term) and a flight from reality. His poems are to an unusual degree " Out of Space—out of Time." Where a Byron or a Shelley revolted against political injustice and became the ardent apostles of liberty, Poe passed serenely through the troublesome years of anti-slavery agitation apparently untouched by the passions of those around him, worshipping only the Beauty whose expression is Art, interested only in the inner conflict within his own soul. In his themes he is neither American, nor Virginian, nor of the nineteenth century. For him the world was depreciated till it scarcely existed; finding reality not to his satisfaction he fled to a world of his own creating. As he said in his story " Berenice ": " The realities of the world affected me as visions, and as visions only, while the wild ideas of the land of dreams became, in turn,—not the material of my everyday existence—but in very deed that existence utterly and solely in itself." Barbey d'Aurevilly speaks of what he calls Poe's " sècheresse," the terrible dryness of his art, and says, " His intellect was real; everything else about him was exquisite feigning. His passion, his human sympathy, his love of nature, all the emotions that go into his fiction, have a counterfeit unreality." (5, pp. 127-8.) This view accords well with Jung's theory of the introvert who, interested in thinking rather than in feeling, assumes the conventionally correct emotions. Poe himself wrote: " In the strange anomaly of my existence, feelings with me *had never been* of the heart, and my passions *always were* of the mind." This last is in line with his general attitude toward women which allowed him to love intellectually so many women, without craving the physical expression of that love. The Freudians would consider this the result of the damming of the libido, perhaps due to some early experience, and they would support their view by calling his stories of horror expressions of anxiety, which to them always has a sexual connotation. It is quite as plausible to accept his own explanation and to hold that his passions were largely of the mind, which, as noted above, is entirely consistent with the introverted type. Lowell, in his "Fable for Critics," confirms this, saying that Poe "has written some things quite the best of their kind. But the heart somehow seems all squeezed out by the mind." Poe wrote to Lowell at the time when he was thinking he had found in the New England poet a congenial spirit: "I have been too deeply conscious of the mutability and evanescence of temporal things to give any continuous effort to anything—to be consistent in anything. My life has been *whim*—impulse—passion—a longing for solitude—a scorn of all things present, in an earnest desire for the future." Mrs. Whitman, in her introductory letter to Didier's Life of Poe, says that "his proud reserve, his profound melancholy, his *unworldliness*—of nature made his character one very difficult of comprehension to the casual observer."

The small volume of verse on which rests Poe's substantial claims to poetic genius, represent the work of his life which he was constantly refining. He composed with the care and effort of the introvert, revising his poems many times, usually to their improvement, intent on the perfection of quality rather than quantity. His themes are few, not from any paucity of imagination but from a complete absorption in a few dominant ideas. He sought, not the varied pleasures of the world, but the interpretation of Beauty alone, the highest form of which he felt to be linked always with melancholy. In "The Assignation" he says "there still lurked (incomprehensible anomaly!) that fitful stain of melancholy which will ever be found inseparable from the perfection of the beautiful." He constantly moved in his poetic imaginings among

> " . . . the ideal,
> Dim, vanities of dreams by night—
> And dimmer nothings which were real—"(Tamerlane)

In struggling with the hard, unlovely realities of life, he cried out

"Oh! that my young life were a lasting dream!
My spirit not awakening, till the beam
Of an Eternity should bring the morrow.
Yes! tho' that long dream were of hopeless sorrow,
'T were better than the cold reality
Of waking life, to him whose heart must be,
And hath been still, upon the lovely earth,
A chaos of deep passion, from his birth."

And later in the same poem entitled "Dreams":

"Dreams! in their vivid coloring of life
As in that fleeting, shadowy, misty strife
Of semblance with reality which brings
To the delirious eye, more lovely things
Of Paradise and Love—and all our own!
Than young Hope in his sunniest hour hath known."

In "A Dream Within a Dream" we find rebellion against the disappointments of life.

"I stand amid the roar
Of a surf-tormented shore,
And I hold within my hand
Grains of the golden sand—
How few! yet how they creep
Through my fingers to the deep,
While I weep—while I weep!
O God! can I not grasp
Them with a tighter clasp?
O God! can I not save
One from the pitiless wave?
Is *all* that we see or seem
But a dream within a dream?"

In still another poem called "A Dream" is seen the contrast between what he has and what he has wanted, between the real and the ideal world of fancy:

"In visions of the dark night
  I have dreamed of joy departed—
But a waking dream of life and light
  Hath left me broken-hearted."

In his "Sonnet—To Science" he calls science the vulture which has

" . . . torn the Naiad from her flood,
The Elfin from the green grass, and from me
The summer dream beneath the tamarind tree."

The shimmering irridescence of "Al Aaraaf," that mosaic of sensuous beauty of sight and sound, the music and glowing images of which haunt our senses even while the mind does not comprehend it, opens with the description:

"O! nothing earthly save the ray
(Thrown back from flowers) of Beauty's eye
As in those gardens where the day
Springs from the gems of Circassy—
O! nothing earthly save the thrill
Of melody in woodland rill—
Or (music of the passion-hearted)
Joy's voice so peacefully departed
That like the murmur in the shell,
Its echo dwelleth and will dwell—
Oh, nothing of the dross of ours—
Yet all the beauty—all the flowers
That list our Love, and deck our bowers—
Adorn yon world afar, afar—
The wandering star."

Of his "Politian" he says:

"He is a dreamer and a man shut out
From common passions."

He speaks to the singing Israfel:

"Yes, Heaven is thine; but this
Is a world of sweets and sours;
Our flowers are merely—flowers,
And the shadow of thy perfect bliss
Is the sunshine of ours."

He is constantly manifesting in his poems the desire to flee the imperfections of this world. But even in his dream world he is sad; he loved melancholy and kept her ever close to his side. In his "Fairy-Land" are

"Dim vales—and shadowy floods—
And cloudy-looking woods,
Whose forms we can't discover
For the tears that drip all over."

His "City in the Sea" is a picture of beauty desolated, of death reigning in the courts of life and love:

"There open fanes and gaping graves
Yawn level with the luminous waves
But not the riches there that lie
In each idol's diamond eye—
Not the gaily-jewelled dead
Tempt the waters from their bed;
For no ripples curl, alas!
Along that wilderness of glass—
No swellings tell what winds may be
Upon some far-off happier sea—
No heavings hint that winds have been
On seas less hideously serene."

The poems of Poe are songs of sorrow: beauty is in them, most often dead beauty, love is there, most often the love of those who are dead to him, and madness is there, as if the expression of the prophetic powers of his unconscious. Often enough, in moments of extreme depression, under the influence of drugs or in the temporary insanity induced by the use of stimulants, must he himself have felt those "evil things, in robes of sorrow," which "Assailed the monarch's high estate." The "Imp of the Perverse" came to him in actual life, and forced him to make appearances which he could neither have desired nor have calculated to benefit himself. His behavior in Washington which lost him the government appointment he desired, and his gratuitous insult to the Bostonians when, on being asked to lecture, he delivered a poem he had written as a child, must be considered as the perverse manifestation of "the will to power" which is gratified by putting a depreciation on others through one's own unbecoming conduct. This same striving for superiority is evidenced in "Israfel:"

"If I could dwell where Israfel
Hath dwelt, and he where I
He might not sing so wildly well
A mortal melody;
While a bolder note than his might rise
From my lyre above the skies."

Poe's heroes are largely autobiographical; they are melancholy men, pursued by unrelenting fate; they are neurotic, hypochondriac, monomaniac, victims of vain delusions; they are the prey of melancholia, insane from sorrow or from the thirst for revenge. In "Eleanora" he seeks to reassure himself, to take the proudly characteristic attitude of defiance to the views of the world. He writes: "Men have called me mad; but the question is not yet settled, whether madness is or is not the loftiest intelligence: whether much that is glorious, whether all that is profound, does not spring from disease of thought,—from *moods* of mind exalted at the expense of the general intellect." In the study of his own diseased thought he is distinctly psychological. He writes: ". . . . what the world calls 'genius' is the state of mental disease arising from the undue prominence of some one of the faculties. The works of such genius are never sound in themselves, and, in especial, always betray the general mental insanity." (6, p. 230.) This appears to be the artist's foreshadowing of some of the views of Lombroso and Adler, as if Poe himself felt that only through his own defects was he able to secure that superiority his soul demanded.

Poe's favorite poem, "The Sleeper," is occupied with his dominant theme, the linking of sex and death. "All Beauty sleeps!"

"The bodiless airs, a wizard rout,
Flit through thy chamber in and out,
And wave the curtain canopy
So fitfully—so fearfully
Above the closed and fringed lid
'Neath which thy slumb'ring soul lies hid."

This is only one of the many pictures he has given us of the couch of beauty which is also a bier. "To One in Paradise," the "Sonnet to Zante," "Lenore," "Ulalume," "Annabel Lee" and the world-famous "Raven" are concerned with reflections over a beautiful woman who is loved and dead. "Annabel Lee," written after his wife's death, is reminiscent of his experiences as a boy when he kept his lonely vigils in the cemetery beside the tomb of his friend.

"And so, all the night-tide, I lie down by the side
Of my darling—my darling—my life and my bride,
In the sepulchre there by the sea—
In her tomb by the sounding sea."

In the same poem his proud spirit defies even death, when he cries

"And neither the angels in heaven above,
Nor the demons down under the sea,
Can ever dissever my soul from the soul
Of the beautiful Annabel Lee:"

"The Philosophy of Composition" claims to explain the conception and composition of "The Raven," and while it is very doubtful if he really wrote the poem in any such impersonal, intellectual and rational manner, nevertheless his analysis contains some valuable hints, as when he writes: "Now, never losing sight of the object *supremeness* or perfection, at all points, I asked myself—'Of all melancholy topics, what according to the *universal* understanding of mankind, is the *most* melancholy?' Death—was the obvious reply. 'And when,' I said, 'is this most melancholy of topics most poetical?' From what I have already explained at some length, the answer here also is obvious—'When it most closely allies itself to *Beauty;* the death, then, of a beautiful woman is, unquestionably, the most poetical topic in the world—and equally is it beyond doubt that the lips best suited for such a topic are those of a bereaved lover.'" The psychoanalyst is prone to seek farther into the poet's life to explain his preoccupation with death. It will be recalled that his parents died early and under most unhappy circumstances; many an hour the sensitive, imaginative child must have brooded over the tragic end of his beautiful, gifted mother. Then at the impressionable adolescent period came the death of the woman he worshipped, Mrs. Helen Stannard. A few years later died the foster mother. Three sweethearts were lost to him. Then there were the six years of dread for Virginia's life. These he has described in a letter to a friend, dated January 4, 1846 (7, p. 215): "Six years ago, a wife, whom I loved as no man ever loved before, ruptured a blood vessel in singing. Her life was despaired of. I took leave of her forever, and underwent all the agonies of her death. She recovered partially, and I again hoped. At the end of a year, the vessel broke again. I went through precisely the same scene. . . . Then again—again—and even once again, at varying intervals. Each time I felt all the agonies of her death—and at each accession of the disorder I loved her more dearly and clung to her life with more desperate pertinacity. But I am constitutionally sensitive—nervous in an unusual degree. I became insane, with long intervals of horrible sanity. During these fits of absolute unconsciousness, I drank—God only knows how often or how much. As a matter of course, my enemies referred the insanity to the drink, rather than the drink to the insanity. I had, indeed, nearly abandoned all hope of a permanent cure, when I found one in the *death* of my wife. This I can and do endure as becomes a man. It was the horrible, never-ending oscillation between hope and depair which I could *not* longer have endured, without total loss of reason. In the death of what was my life, then, I received a new but—Oh God!—how melancholy an existence." Allowing all due discounting for the poetic picturing of himself in constant agony, there can be no doubt that "the horrible, never-ending oscillation between hope and despair" of those six years must have made an indelible impression upon the poet's brain. Little wonder, then, that he wrote of the death of beautiful women.

In his stories Poe continues this linking of death with sex. Another factor manifests itself, namely, the sadistic delight in torture. Let us recall a few of these stories. In "Berenice" the lover tears out the teeth of his beloved before her body is cold within the grave; in "The Black Cat," having cut out the eyes of the cat he abhors, and later killed it, he is tormented by another cat whom his wife protects; maddened by her attitude he seizes an ax and cuts through her brain, after which he walls up the body in his cellar and laughs with glee at the fumbling search of the police; there is the murder of Marie Roget, mutilation of the body and sinking of it in the river; in the "Murders in the Rue Morgue" he dwells over all the gruesome details of the masses of hair torn from the old woman's head, the throat cut with a razor, the daughter stifled in a chimney. The record was kept of seventeen stories of Poe as they appeared in an ordinary edition. Of these tales only three did not record violent or tragic deaths; these were "The Purloined Letter," which ends with a revenge; the "Man in the Crowd," a personification of crime, and the "Pit and the Pendulum," as terrible a delineation of torture as has ever been penned. Here is the death list from the other fourteen: Two dead in the "Gold Bug," two in "The Descent into the Maelstrom," the crews of two ships in the "Mss. Found in a Bottle," two in the "Murders in the Rue Morgue," two in the "Mystery of Marie Roget," two in the "Fall of the House of Usher," one in "William Wilson," one in "The Black Cat," one in "The Tell-Tale Heart," two in "The Assignation," the prince and his thousand attendants in "The Masque of the Red Death," one in "Berenice," two in "Morella," one in "Shadow." The list might be extended to cover the majority of the tales, but it seems unnecessary to pursue such inquiry further. Poe's mind was not only preoccupied with death but with violent death, with murder and with pestilence.

The sadistic impulse is readily linked with sex. This is shown in a more subtle form in his favorite story "Ligeia." Here is the description of the bridal chamber prepared for his second wife:

> "Some few ottomans and golden candelabra, of Eastern figure, were in various stations about—and there was the couch, too, the bridal couch—of an Indian model, and low, and sculptured of solid ebony, with a pall-like canopy above. In each of the angles of the chamber stood on end a gigantic sarcophagus of black granite, from the tombs of the kings over against Luxor, with their aged limbs full of immemorial sculpture. But in the draping of the apartment lay alas the chief fantasy of all. The lofty walls, gigantic in height—even unproportionably so—were hung from summit to foot, in vast folds, with a heavy and massive-looking tapestry—tapestry of a material which was found alike as a carpet on the floor, as a covering for the bed, and as the gorgeous volutes of the curtains which partially shaded the window, the material was the richest cloths of gold. It was spotted all over, at irregular intervals, with arabesque figures, about a foot in diameter, and wrought upon the cloth in patterns of the most jetty black. But these figures partook of the true character of the arabesque only when regarded from a single point of view . . . To one entering the room, they bore the appearance of simple monstrosities; but upon a farther advance this appearance gradually departed; and step by step, as the visitor moved his station in the chamber, he saw himself surrounded by an endless succession of the

ghastly forms which belong to the superstition of the Norman, or arise in the guilty slumbers of the monk. The phantasmagoric effect was vastly heightened by the artificial introduction of a strong continual current of wind behind the draperies—giving a hideous and uneasy animation to the whole." Here again is the picture of the bed which is a coffin, and the heavy curtains move uneasily like the pall which covers the last resting place of the dead. In the center of the room swings the censer around which writhe serpent-like flames; the censer is of gold. In tending the sick lady Rowena the husband offers her a cup of wine, into which he sees fall from an invisible hand a few ruby colored drops. Insensibly the reader feels that this is the revenge of the first wife, Ligeia, on the one who has taken her place. Rowena dies, then revives, dies again, revives. Each time the husband does what he can to assist her, although he seems to desire that she shall die utterly and leave him in peace. When he thinks she is at last quiet in her final sleep she rises and reveals to him the wild eyes and streaming hair of his lost love Ligeia.

In this tale are the three colors: gold and black and red, which Poe uses most; there is also beauty and death and sex. The bed and the entire room suggest a coffin in a tomb, the censer which is so frequently present in his descriptions is reminiscent of the church services which he attended twice each Sunday in England and so may very easily have become associated with funerals and death, the serpent-like flames represent the sex symbol. Here, too, is found another characteristic of much of his writing: the death wish, this time directed against the second wife. Then comes again the old feeling that the dead are not wholly dead and the dead woman is revealed as his first wife. Her expression is the projection of his own feelings of remorse both for taking another bride and for the death-wish against Rowena.

The imagery of this story Poe uses over and over again. "The Assignation," another story of the love of a neurotic and the death of a beautiful woman, gives the picture of a strange room where "Rich draperies in every part of the room trembled to the vibration of low, melancholy music." Here also are the swinging censers; the windows are of crimson tinted glass. In the "Pit and the Pendulum" the condemned wretch says, "I saw, too, for a few moments of delirious horror, the soft and nearly imperceptible waving of the sable draperies which enwrapped the walls of the apartment." In "The Raven" is heard the "silken, sad, uncertain rustling of each purple curtain." "Metzengerstein" has tapestry hangings which swing gloomily upon the walls. In "Shadow" are sable draperies. We read in "The Masque of the Red Death:" "The seventh apartment was closely shrouded in black velvet tapestries that hung all over the ceiling and down the walls, falling in heavy folds upon a carpet of the same material and hue." It was not merely the thought of death which obsessed the poet, but death in all its details and in its visible symbol, the casket of the dead. From contemplation of the coffin the funeral pall or draperies came to have a peculiar significance, and when he wrote of rooms in which terrible deeds transpired he hung those rooms with the draperies of the dead. Not only that, but the curtains stir, to sad music, to strange winds, heightening the effect of horror, as the breezes which lift the pall over the face of the dead, increasing the agony of the spectator by giving the semblance of life where life has fled. In his poems are the same figures: a curtain is a funeral pall ("The Conqueror Worm"), "there passed, as a shroud, A fleecy cloud" ("Evening Star"),the eyes of the beloved "desolately fall on my funereal mind Like starlight on a pall." (To—.)

Then there are the colors: gold sometimes, but the principal contrast being the black and the red, symbols of death and of sex, the two ideas to which his thoughts ever returned. Black ships, black cats, clocks of ebony, sable condors, black marble flagstones, blackened goblets, black wings of pestilence, ebony tables, black silk-velvet palls, sable hearse plumes—these are a few of the symbolizations of death. For the sex motif are red lights, crimson-tinted glass, scarlet panes, the ruddy reflection from burning buildings, the fiery colored horse, fiery colored clouds, blood-red metal, intense light of rubies, the red of poppies, wine red as blood, rain that changed to blood, the fiery wall of the horizon, red clouds, the red eye of the sun, the crimson moon. No attempt has been made to cover all the uses Poe has made of the two colors, but it is very significant that these are the two which he used most often, and on which he rang so many changes. "The Masque of the Red Death" shows the effectively vivid combination of the two. The seventh chamber, completely covered with black draperies, has for windows scarlet panes, outside of which lamps are placed, so that the light falling on the black curtains must pass through the scarlet and so blend the two in a weird and unearthly aspect.

After the coffin, the poet's mind turned to the grave or tomb, and a vast number of allusions to this may be found in his writings. In "The Cask of Amontillado" he walls up his enemy in a living tomb; in "The Fall of the House of Usher" Madeleine is placed in a vault under the house; in "The Assignation" he rides in a "funereal gondola;" the "Gold Bug" is the story of treasure buried along with two men in a single grave; the Red Death masqueraded in grave clothes; he buries his victim under the floor in "The Tell-Tale Heart;" he thinks "what sweet rest there must be in the grave" ("The Pit and the Pendulum") and in the same story "dreaded to move a step, lest I should be impeded by the walls of a *tomb.*" He sings of the worm that never dies and prays that worms may creep softly around his sweetheart's body. In this connection also will be recalled his poem, "The Conqueror Worm."

The creeping of worms through dead bodies is considered a sexual symbol, and in this connection seems to show the presence of an erotic gratification at the thought of the dissolution of the bodies of beautiful women. The thought of the sweet rest within the grave is the desire to creep back into the mother's womb, which Jung and Silberer have considered so characteristic of the introvert.

The sadistic element is a compound of the sexual desire and the desire to give pain. These desires, repressed in Poe's life, are embodied in his poems and tales. With the child-wife, who was for years almost an invalid, he could never have enjoyed the satisfaction of a normal sex life, while his love for other women seems to have been of the mind rather than the body. Thwarted on this side of his nature, he turned to literary and artistic creation. In this he eked out the barest livelihood, and made enemies constantly by reason of his genius as well as of his intolerance of the claims of the many mediocre. Through the courts and by means of powerful invectives from his own brilliant, bitter pen he sought revenge on those who slandered or opposed him. But his victories were never wholly won; he never triumphed utterly over his enemies. His will to power, however, would be satisfied with nothing short of their annihilation, and, failing that in the actual physical world as well as in the literary and social world, he turned loose his sadistic impulses upon the creatures of his stories, some of whom must have represented very real figures to himself. In "The Cask of Amontillado" he says a wrong is never righted till the aggressor is punished and knows he is punished by the injured one. His imagination fairly gloats over the ingenious tortures it devises, many of which are equal to the most fiendish of the Inquisition.

Freud (4) has written on the effect which whipping has on children in developing both sadism and masochism. On seeing other children whipped the child feels that he is then the favored one, and a sadistic element of joy at suffering is introduced. On being whipped himself the child may be animated either by a sadistic or a masochistic desire. Masochism can be explained as a turning back of sadism upon the ego and is not itself a primary sex urge. It is a sort of narcissistic or autoerotic sadism, or sadism focussed on the person's own genital organs. Poe went to school in England at a time when fagging was in order and as a little fellow he must have come in for his due amount of punishment. In school at Richmond, under a schoolmaster who believed greatly in the efficacy of flogging, Poe was never known to be punished, this fact being remarked on as quite unusual by one of his classmates. His experience in England would allow for development of either masochistic or sadistic elements; that in Richmond would reinforce the sadistic. The son of his foster father's partner remarked on the mean delight which Edgar, then a boy, took in tormenting a sensitive girl by pointing an imitation serpent towards her. (6, p. 25.) The serpent being the sexual symbol, the victim of the cruelty a girl, there seems an undoubted sadistic element here. The same gentleman tells of being thrown into some falls by Poe, who was then obliged to rescue the weaker boy, such an incident gratifying both the sadistic tendency and the will to power. The scene which he is reported to have made with the second Mrs. Allan when she was sick in bed indicates the same sadistic trend. All through his life the two things are found together: his will to power, thwarted, demanding sadistic revenge, his sadism gratifying and reinforcing his will to power.

There is an obvious death wish in many of the stories of Poe directed against a man. In "The Tell-Tale Heart" it is against an old man, whose murder he accomplishes; not satisfied with this he cuts off the limbs. He commences the story in an extenuating tone, but objects to being called mad. "True—nervous—very, very, dreadfully nervous I had been and am; but why *will* you say that I am mad?" Then telling of the murder, he continues: "It is impossible to say how first the idea entered my brain; but once conceived, it haunted me day and night. Object there was none. Passion there was none. I loved the old man. He had never wronged me. He had never given me insult. For his gold I had no desire. I think it was his eyes! yes, it was this!" This detailed setting forth of lack of reason for a murder indicates in the writer a defense mechanism against the unconscious wishes. There was one man for whose murder all such reasons would apply: the foster father, Mr. Allan. There had never been love or understanding between the two. He had brought up the gifted boy as his son, giving him his name, then had cast him off without a cent. He had insulted him and driven him from his house. He had refused to pay Edgar's gambling debts which the boy regarded as debts of honor, and so had placed him in a position of galling humiliation at the University. Had he died earlier Edgar would have inherited his gold. Had he not married again the fortune would probably have gone to the poet. In all the years of destitution, of pitiful struggle against a pitiless fate, never able to realize his cherished desire of founding a magazine because of lack of funds, seeing a young wife fade away lacking the money to buy her either the drugs or food her condition demanded—is it not reasonable to suppose that hatred should turn against the man who might have prevented all this, against the man who had taught him to expect and to desire luxuries and had disappointed him, against the man who had humiliated and insulted him? Object and passion were surely here sufficient for the unconscious if not the conscious death wish.

We come now to the masochistic tendency of Poe, which while not nearly so striking generally, is extremely well displayed in a few stories. It will be recalled that masochism is the regression of the sadistic impulse from the outer world onto one's own person. In "Loss of Breath" he describes his own tortures in tones of considerable enjoyment, his ears were cut off, incisions made into his stomach, the skull fractured, he was hanged and obligingly went through spasms for the benefit of the populace which encored. "A Predicament" tells the story of his being caught in a huge clock and the minute hand being imbedded in his neck till it cuts off the head.

> "The ticking of the machinery amused me. *Amused me*, I say, for my sensations now bordered upon perfect happiness. . . . The eternal click-clack, click-clack, click-clack, of the clock was the most melodious of music in my ears. . . The bar had buried itself two inches in my neck. I was aroused to a sense of exquisite pain . . . My eyes, from the cruel pressure of the machine, were absolutely starting from their sockets . . . one actually tumbled out of my head . . . I was presently relieved . . . by the dropping out of the other eye.
>
> The bar was now four inches and a half deep in my neck, and there was only a little bit of skin to cut through. My sensations were those of entire happiness, for I felt that in a few minutes, at farthest, I should be relieved from my disagreeable situation . . . At twenty-five minutes past five in the afternoon precisely, the huge minute-hand had proceeded sufficiently far on its terrible revolution to sever the small remainder of my neck. I was not sorry to see the head which had occasioned me so much embarrassment at length make a final separation from my body. It first rolled down the side of the steeple, then lodged, for a few seconds, in the gutter, and then made its way, with a plunge, into the middle of the street."

The detail with which this is told and such expressions as "amused me," "a sense of exquisite pain," "entire happiness," are expressive of masochistic delight in suffering while the sardonic humor which laughs at his own dismemberment is a defense mechanism which the sensitive poet must have had to set up early in life, as protection against the ridicule of others.

"The Pit and the Pendulum" is the tale which best of all illustrates this masochistic tendency. It is told in the first person by a victim of the Inquisition. The man swoons on hearing his sentence and awakens to find himself in utter darkness in an underground cavern. There he narrowly escapes falling into a grewsome pool of nameless horrors. The invisible torturers plan for him one death after another, each more terrible than the last. In reality one feels that it is the author himself who takes delight in planning first, one affliction, then another, and who enjoys the thought of his own body suffering such agonies. The story is like a dream in which we watch ourselves take part, feel with ourselves and yet are detached from ourselves. After a swoon the poor victim awakens to find himself bound flat upon the floor, utterly helpless save for one free arm with which he can reach the scanty food at his side, which is carefully calculated just to keep life in his body. He hears above him a strange ticking and perceives in the shadows what appears to be a large pendulum. As the hours pass he sees this pendulum slowly descend. It approaches his bound body, hour by hour coming nearer; after every swoon he awakens to find it closer; he sees the sharp steel of its point; it cuts the air in ever wider circles; fascinated, yet horrified, he watches for days the approach of death; in detail the author describes his body sensations. The rats have come out to snatch the last remnants of food; at length it occurs to him that by smearing the meat upon his bonds they will gnaw away his fetters. While the rats gnaw at his cords the steel pendulum sweeps nearer and nearer, its sharp blade cutting the air just above him. It touches his garments, it cuts the cloth above his breast, and just at the last second he is freed and crawls away. Immediately the pendulum is drawn up, showing how incessantly he is watched. Then there comes a light within the chamber, a strange light which shows up the horrible, fantastic figures of devils and ghosts upon the walls; these figures begin to glow with heat, the walls and floor are hot to the touch, he perceives that they are entirely of brass and capable of casting out an intense heat. Tortured by fear and by the increasing heat, he creeps to the edge of the pool, and gazes down, too horrified by what he sees to take the plunge. It is evident that the torturers design to drive him into the pool and he determines to resist at no matter how great suffering. The heat and his agony continue till at the limit of human endurance he is rescued by the armies of his sect who have taken the town. The meticulous detail of this story, the evident relish with which the author describes his sensations as, helpless on the floor he watches the nearing sweeps of the sharp blade of the pendulum, or as he recoils from the ever increasing heat, shows a masochistic delight in the thought of torture applied to his own body.

Mordell (9) calls attention to the stories of ratiocination of Poe which he considers a further indication of masochism. " The Murders in the Rue Morgue," " The Mystery of Marie Roget," " The Purloined Letter," and " The Gold Bug," are masterly examples of the analytical genius of Poe. The careful study of the tedious details of the mysteries of the first three as well as the solving of the cipher in the last tale represent the delight of a mind which loved to torture itself.

This is clearly shown in the man's life as well. As editor of a Philadelphia journal he made the boast that man could invent no cipher which human ingenuity could not decipher, and set himself the task of solving the many cryptograms submitted. The busy editor yet found time to spend hours exercising his mental faculties, as if delighted by the irritating burdens he could place upon his mind. It is reputed that he never failed to make good his boast.

The analytical powers manifested by Poe in such stories as those mentioned above, and also strikingly evident in much of his critical writing, was a characteristic not commonly associated with his other gifts. He insisted, however, that there could be no genuine imagination without this capacity for analysis of the keenest sort. Certainly the verisimilitude of his most extravagant tales is greatly increased by the background of incisive thinking and detailed analysis which they evidence. Oliver Leigh has made an interesting study in the character of Poe as shown by his photographs. One side of the head, if duplicated, gives a picture of the bulging forehead and heavy brain of the hydro-cephalic, the dwelling place of grotesque fancies and weird images of horror; the other side shows the square-headed, intellectual, analytical type, the brain which produced stories of ratiocination, critical essays of penetration and which distinguished itself in mathematics. (8) These two types, confined within one brain, gave the world the dreamer, the neurotic, the poet, the analyst and the thinker.

Poe's life after the death of Virginia in January, 1847, is sadder than any of his stories. The last two years before the end in 1849 are a miserable record of alternating hope and despair, of a body and mind on the downward grade of deterioration, checked now and again before its approaching dissolution to turn and attempt to struggle back to sanity and health, yet never quite succeeding. After his own illness at the time of his wife's death he never regained his former capacity of productivity, although his last years were marked by fitful gleams of that genius which has won for him in Europe the reputation of being America's one really great and original poet. The mad poem, " Ulalume," with its melody and weird effects of nameless horror; the beautiful death poem, " To Annie," in which he thanks Heaven that " the fever called living is over at last;" the onomatopoeism of " The Bells " and the prose poem, " Eureka," belong to this period. In these last years he also met and loved Mrs. Helen Whitman, formed an ardent friendship for " Annie," Mrs. Richmond, and became engaged to Mrs. Shelton who, as Miss Royster, had been his boyhood sweetheart. The last thing he wrote was the lyrical " Annabel Lee," which was not published until after his death.

" Eureka " represents a new departure into philosophical realms. Dedicated to Alexander von Humboldt it is offered to " the dreamers and those who put faith in dreams as in the only realities " as a " Book of Truths, not in its character of Truth-Teller, but for the Beauty that abounds in its Truth; constituting it true." Through all its excursions into Physics, Astronomy and higher mathematics it manifests the neurotic passion for Unity—a seeking after a primal cause from which came all the Universe and to which all must return. In this are passages suggestive of Ward's " primal homogeneity " and theory of filiation. The poet anticipates the time when the myriads of individual intelligences will become blended, as will the bright stars, into One. He closes with the triumphant thought: " Think that the sense of individual identity will be gradually merged in the general consciousness—that Man, for example, ceasing imperceptibly to feel himself Man, will at length attain that awfully triumphant epoch when he shall recognize his existence as that of Jehovah. In the meantime bear in mind that all is Life—Life—Life within Life—the less within the greater, and all within the Spirit Divine." (10c, p. 315.)

The rumors which still go the round of the clubs in Baltimore claim that Poe was definitely syphilitic. This has never been established, although the lesion on the brain and cerebral congestion from which he suffered in his last years increase the probability of the syphilitic infection. Whether this be true or not, he certainly displayed the same diathesis evidenced by many men of genius who are said to have been syphilitic—for instance, de Maupassant, Wilde, Nietsche. In them, as in Poe, the phyletic tendencies died out and the egoistic ones came into dominance. This might be expected as a result of the disease which, depriving a man of the capacity for propagation, so cuts him off from the normal social interest in the future of the race, and turns the full force of the libido in the direction of personal ambition. Senescents and young people—those too young and too old to bear children—betray somewhat this same selfish absorption in the attainment of their own desires. These syphilitic geniuses, driven by the nature of their disease, to concentration upon their own egoistic development as the sole means of satisfying their will to power, became to a great extent hyper-individualized, with an over-expansion of the ego which made them anti-social. Their sex interests, too, seem to make at least a partial transference from the normal object of the opposite sex. Oscar Wilde's glittering descriptions—of pomegranates bursting in the sun, of tall reeds of fluted ivory, of pale poppies, ostrich plumes like white foam, robes of tissued gold, ceiling of fretted silver, green bronzes, agates, lapislazuli, bowls of amethyst, nightingales, and faint perfumes of jasmine,—the manner in which he lingers lovingly over long pages of rare gems, rich embroideries and draperies, strange, heavy odors, and the sound of weird, barbaric music, betrays more than a mere esthetic appreciation of beauty. There is a sensuous, sexual delight in these beautiful objects which far surpasses the normal enjoyment. Poe manifests this same attitude, although his range of enjoyment is more narrowed than that of Wilde, and even in his keenest appreciation of the loveliness of the inanimate world he is still haunted by the conceptions of horror and of death, which were to him inseparable from the realization of the highest beauty. This, too, would indicate a certain sexual displacement because of his constantly allying sex with death—the death of a beautiful woman being to him the most poetic of themes. Likewise his love of women, which was apparently of the mind so much more than of the senses, seems to signify an erethic sublimation which may have been, in the last analysis, based upon his physical condition.

It was in these last two years that Poe met and became engaged to Mrs. Helen Whitman. His love letters to " Helen " as well as his tenderly devoted letters to " Annie " at this time, form some of the finest products of his later writing. These letters to "Helen" have been extravagantly praised as showing the passion of a lover. This they do not. They show the passion of a poet, writing about love. He was writing literature under the form of love letters just as he had under the form of love poems, stories or critiques. This passage has been often referred to as showing his deep devotion to Mrs. Whitman: (11, p. 71)

> "As you entered the room, pale, timid, hesitating, and evidently oppressed at heart; as your eyes rested appealingly, for one brief moment, upon mine, I felt, for the first time in my life, and tremblingly acknowledged, the existence of spiritual influences altogether out of the reach of my reason. I saw that you were Helen—*my* Helen—the Helen of a thousand dreams—she whose visionary lips had so often lingered upon my own in the divine trance of passion—she whom the great Giver of all Good preordained to be mine—mine only—if not now, alas, then at least hereafter and *forever* in the Heavens. You spoke falteringly and seemed scarcely conscious of what you said. I heard no words—only the soft voice, more familiar to me than my own, and more melodious than the songs of the angels. Your hand rested in mine, and my whole soul shook with a tremulous ecstasy. And then but for very shame, but for fear of grieving or oppressing you—I would have fallen at your feet in as pure—in as real a worship as was ever offered to idol or to God. And when, afterwards, on those two successive evenings of all—heavenly delights, you passed to and fro about the room—now sitting by my side, now far away, now standing with your hand resting on the back of my chair, while the preternatural thrill of your touch vibrated even through the senseless wood into my heart—while you moved thus restlessly about the room—as if a deep Sorrow or more profound Joy haunted your bosom—my brain reeled beneath the intoxicating spell of your presence (and it was with no human senses that I either saw or heard you. It was my soul only that distinguished you there.) I grew faint with the luxury of your voice and blind with the voluptuous lustre of your eyes."

Yet at the same time that he was writing to Mrs. Whitman in such a vein, he was writing to "Annie" in letters breathing a spirit of entire confidence and love. Mrs. Whitman was said to have been greatly hurt by the publication of these latter letters, but they in no sense diminish Poe's love for her, nor show any evidence of faithlessness. In all these letters he was simply being true to himself—true to his own needs which required the loving approval of many women rather than the utter devotion of one, which could be satisfied as much by the sexless attachment to a married woman as by his engagement to Helen. After the breaking of the engagement with Mrs. Whitman, brought about by the efforts of friends of hers who objected to the match, Poe became engaged to Mrs. Shelton. His relations with women after Virginia's death represent a pitiful attempt to re-establish himself, to regain from the eyes of those who loved him his own self-respect. Mrs. Clemm was then too old and broken to afford him the solace and safety he so hopelessly craved. But from among all his women friends, from all who had relieved the dark hours of Virginia's death, all those who from their attachment to the poet, ministered willingly to his mother-in-law after his death, no Gradiva appeared, not one was strong enough to rescue him from the perilous path his feet were straying on, not one was able to bring him peace, and faith and a renewed interest in life.

But more than a woman's love he was needing the attention of a specialist and this he did not receive. The libido of the poet, thwarted in its former expression through his poems and his love for his wife, sought here and there for other outlets, in his attachments to other women, in his titanic dream of the universe wherein he sought to grasp the very essence and simplicity of Unity itself, and in his obsessive desire to found a magazine of his own,—sought, but did not find. The lesion on one side of his brain (diagnosed by the famous New York physician, Dr. Mott, and the nurse, Mrs. Shew, Poe's faithful friend for years) would not permit him to use stimulants or tonics without producing total insanity. N. P. Willis, the editor, in a tribute to Poe immediately after the unhappy end in Baltimore, alludes to this: " We heard, from one who knew him well, . . . that, with a single glass of wine, his whole nature was reversed, the demon became uppermost, and, though none of the usual signs of intoxication were visible, his *will* was palpably insane." (6, p. 361.) Yet in times of deepest depression even insanity must have come to appear preferable to his own sad thoughts, and he sought feverishly " surcease from sorrow," whatever the means or the result. Before going to Richmond on the final journey he wrote to Annie: " No, my sadness is *unaccountable,* and this makes me the more sad. I am full of dark forebodings. *Nothing* cheers or comforts me. My life seems wasted—the future looks a dreary blank: but I will struggle on and ' hope against hope.' " (6, p. 303)

The attempted suicide from laudanum in Boston was only one evidence of the profound state of melancholia into which he had sunk. His physique deteriorated, and suffering from congestion of the brain, he should have been in a sanitarium instead of struggling about the country in the forlorn hope of founding his magazine. It was in quest of this " will o' the wisp " that in 1849 he set out for Richmond. He was at this time in the most wretched spirits; he expressed a presentiment to Mrs. Lewis that he would not return and arranged all his papers with Mrs. Clemm, telling her what to do in case he died. His delusion of persecution in Philadelphia is extremely suggestive of the beginnings of paresis. His state is described by Mr. Gill (6, p. 307):

> "When he finally reached the residence of his kind friend, Poe was in a highly excited condition, almost distracted indeed. His mind seemed bewildered and oppressed with the dread of some fearful conspiracy against his life; nor could the arguments or entreaties of his friend convince him that some deadly foe was not, at that very moment, in pursuit of him. He begged for a razor for the purpose of removing the mustache from his lip, in order, as he suggested, that he might disguise his appearance, and thus baffle his pursuers. But, unwilling to place such an instrument in his hands, he was prevailed upon to allow his host to effect the desired change upon which he imagined his safety depended. The conditions of Poe's mind was such that Mr. Sartain, after persuading him to lie down, remained watching with him through the night with anxious solicitude, unwilling to lose sight of the unfortunate sufferer for a moment. The following night, Poe insisted on going out. He turned his steps towards the River Schuylkill, accompanied, however, by his devoted friend, whose apprehension was strengthened by the vehemence with which, without cessation, he poured forth in the rich, musical tones for which he was distinguished, the fervid imageries of his brilliant but over-excited imagination. The all-absorbing theme which still retained possession of his mind, was the fearful conspiracy that threatened his destruction. Vainly his friend endeavored to reassure and persuade him. He rushed on with unwearied steps, threading different streets, his companion striving to lead him homeward, but still in vain.
>
> "Towards midnight, they reached Fairmount and ascended the steps leading to the summit, Poe all the while giving free scope to the conversational powers for which he was always remarkable, insisting upon the imminence of his peril, and pleading with touching eloquence for protection. . . .
>
> "He didn't recover from this intense excitement until, subsequently, escaping from the house, he wandered out into the neighborhood of the city, and throwing himself down in the open air in a pleasant field, his shattered nerves found a comfortless but sorely needed repose He woke refreshed. . . .
>
> "All that he could call to mind were the entreaties and persuasions of some 'guardian angel' who had sought to dissuade him from a frightful purpose."

Such delusions would seem to have some correlation with the stories that he was unable to go to sleep after Virginia's death without the presence of Mrs. Clemm or some kind friend at his bedside. The fear of death, which he evidenced in his morbid prepossessions with the subject, may have grown in these "lonesome latter years" to be so portentous and awful a figure that he could never quite escape its shadowy horrors. Or again, the fear of life itself, of the futile mockery of life which he was leading, of himself as he had come to be in his weakness, might have been the cause of much of his terror. Either fear could easily, in his overwrought state, have led to the thought of suicide, which he had already once attempted, and this may have been the "frightful purpose" from which the "guardian angel" sought to dissuade him.

Temptation conquered for the time being, there came to him a momentary peace from his delusions and fears and a new enthusiasm for laying hold on the actual facts of reality. The possibility of a new life beckoned him smilingly, and he arrived in Richmond in unusually good spirits. There he was welcomed and made much of by old friends. He even took the temperance pledge with much earnestness, obtained a position in the city of his childhood and planned to bring Mrs. Clemm down there. It was at this time that he is said to have renewed his addresses to Mrs. Shelton and she, a widow of independent means, to have accepted them. He lectured before a cultured audience, and left Richmond apparently more contented than he had been for some time, carrying with him, according to Bishop Fitzgerald (6, p. 322), the proceeds of his lecture, which amounted to $1,500. Of the tragic, solitary end in Baltimore, little is known with any degree of accuracy. Poe arrived there at election time, at a period when it was customary for bands of men to seize helpless strangers, drug them and carry them from poll to poll, registering their votes for whatever party they desired. It is believed generally that he fell in with such a gang. However that may be, he was found in a barroom, insensible, in the shabbiest of clothes. He was taken to a hospital where he lived about a week. He is reported as being part of the time in stupor, part of the time in delirium, during which he conversed vacantly with spectral or imaginary objects. His replies to questions were entirely unsatisfactory. Two of his doctors report that he made the statement that his best friend would be he who would blow out his brains. (6, pp. 334-336.) So, on October 7, 1849, was extinguished that flame of genius which had blazed, now so fitfully, now so brilliantly, for forty years. In the mystery of his last days, his life did indeed go out like a candle in the dark.

It is doubtful if the circumstances of his death matter so very much. If Baltimore rowdies came in to hasten the end, they were simply an incident in the inevitable close. The man had burnt himself out. He had described his own condition when he wrote: "There are few men of that peculiar sensibility which is at the root of genius, who, in early youth, have not expended much of their mental energy in *living too fast;*

and, in later years, comes the unconquerable desire to goad the imagination up to that point which it would have attained in an ordinary, normal, or well-regulated life. The earnest longing for artificial excitement, which, unhappily, has characterized too many eminent men, may thus be regarded as a psycal want, or necessity—an effort to regain the lost—a struggle of the soul to assume the position which, under other circumstances, would have been its due." (6, p. 231.) He showed at the last very clearly that "psychal" want, the struggle of the soul to regain the lost, and in the nature of the case he was doomed to failure. That last brief lifting of the clouds in Richmond could have been only temporary; his weakened body and the congestion of the brain from which he was suffering made inevitable the return of his former condition of melancholia. That diseased brain which had supplied his writings with all its morbid, beautiful imagery and haunting melodies of death, came finally by its defects to render his further progress and development impossible. And when it failed him, and there was no longer possible a continuation of the superiority which he had won in his chosen field, his proud spirit, his guiding fiction, must have sought relief in some form of insanity or in death. Insanity had begun, under the depression of melancholia and the delusions of paresis; death he had sought before and would have sought again; the darkened close in Baltimore came as a release—a release not to be regretted by any who love Beauty as an end in itself and who crave for its expression a form no less perfect than that which Poe was able to give in the days of his greatest power.

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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by Edwin G. Boring

XXIV. HIGHEST AUDIBLE TONES FROM STEEL CYLINDERS

By C. C. Pratt

The Psychological Laboratory of Clark University has a set of twenty-two steel cylinders designed for the determination of the upper limit of hearing. The set is similar to Koenig's and is manufactured by the Standard Scientific Company of New York. The present list price is $75. The cylinders are computed by the manufacturers on the basis of a calibration made for a greater length of bar to give the frequencies for three octaves of the natural diatonic scale from $c^5 = 4096$ to $c^8 = 32{,}768$ vs. inclusive. Each cylinder is slightly grooved at its two nodal points and is suspended horizontally by loops of fine tough cord which pass about these grooves. A brass hammer is used to secure vibration.

We have attempted unsuccessfully to calibrate these cylinders by means of the Kundt dust method.[1] Schwendt has reported his experiments with this method in determining the frequencies of the Koenig high forks and cylinders.[2] The forks afforded him little difficulty, but only after many trials was he able to secure the Kundt dust figures from the cylinders. We proceeded in the same manner with lycopodium powder distributed in thin glass tubes ranging from 20 to 8 mm. in diameter, but in only two instances did we get the dust to fall in figures which could be considered reliable for the determination of wave-length. With the cylinders d^5 and f^5 the lycopodium fell into periodic loops 3.83 and 3.27 cm. in length respectively. These values divided into the speed of sound at the given temperature give frequencies of 4511 and 5361 as against 4608 and 5461.33 calculated for these particular cylinders by the makers. Koenig[3] was inclined to believe that calculation gave values slightly too high; thus the importance of calibration becomes apparent if the cylinders are to be used for accurate work. It would seem, however, that the Kundt dust method is not very suitable for use with the cylinders. We spent two weeks of vacation-time in fruitless effort to get results, and the only report of the success of this method with the cylinders is Schwendt's brief mention. On the other hand, since calculation and calibration agree here to within 2%, we may perhaps assume that the cylinders as calculated and marked by their manufacturers are accurate for clinical work and class demonstrations, even though calibration will be necessary in research work.

We have used the cylinders at Clark University to determine the tonal TR of sixteen observers, all of whom had had experience in psychological observation. Each observer was seated comfortably in a chair with his back to the cylinders at a distance of four feet, and was given the following instructions: "When the stimulus is presented, you are to report 'yes' if you hear a tone, and 'no' if you hear no tone. Try to report immediately. If you are doubtful of your judgment, say so." Preliminary practice was given to train O and to enable E to determine the critical stimulus. The two cylinders immediately on either side of the critical stimulus were used to complete a series of five stimuli, on each of which 100 judgments were taken. The stimuli were presented in haphazard order in series of 50 judgments with a brief pause between series. No observer experienced more than a temporary initial difficulty in recognizing the presence or absence of tone, and doubtful judgments were rare. They were permitted but not recorded. The whole experiment required about an hour. The accompanying table shows the percentage of affirmative judgments on each of the five stimuli for each observer.

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[1] For a discussion of this method as applied to the calibration of high acoustic frequencies, see E. B. Titchener, *Experimental Psychology*, vol. 2, part 2, pp. 32ff.

[2] A. Schwendt, Experimentelle Bestimmungen der Wellenlänge und Schwingungszahl höchster hörbarer Töne, *Pflüger's Archiv*, 75, 1899, p. 356.

[3] R. Koenig, Ueber die höchsten hörbaren und unhörbaren Töne, *Wied. Ann.*, N. F. 69, 1899, p. 723.

TABLE

Relative frequencies of the judgment "Yes," meaning heard-tonal-quality, for struck steel cylinders of the frequencies indicated. Sixteen observers, one session each. One hundred judgments on each of five stimuli. The frequencies are calculated and not calibrated; see text.

	CALCULATED FREQUENCY AND MUSICAL NOTE									
	10922.67	12288.	13653.33	15360.	16384.	18432.	20480.	21845.33	24576.	27306.67
Obs.	f^6	g^6	a^6	b^6	c^7	d^7	e^7	f^7	g^7	a^7
1	100	100	92	15	1					
2			100	100	58	9	0			
3			100	100	88	14	0			
4			100	100	87	8	0			
5				100	100	8	0	0		
6				100	100	9	0	0		
7				100	100	78	8	0		
8				100	100	67	0	0		
9				100	100	76	0	0		
10				100	100	84	0	0		
11				100	100	85	5	0		
12				100	100	100	0	0		
13				100	100	95	11	0		
14					100	100	29	0	0	
15					100	100	83	6	0	
16						100	100	81	0	0
Average	100	100	99.5	94.7	90.6	58.3	13.9	5.4	0	0

1. The table shows that differences between individuals are much greater than the variations of the single individual from judgment to judgment. Although the d of about 18,000 is most often the critical stimulus there were cases in which the e and f above and the c, b and a below were critical. The instrument, therefore, provides a means for the determination of individual differences, though the number of stimuli is too few to render the calculation of a limen possible, and though the frequencies as calculated are inaccurate within a small per cent.

2. It is extremely probable that under conditions of greater practice not more than one cylinder would turn out to be critical, that all cylinders of lesser frequency would give 100% affirmative judgments and all cylinders of greater frequency no affirmative judgments. Eight of our sixteen observers give this result even under our conditions of little practice. It is also probable that greater practice would produce more often the results given by observer 12, for whom no critical stimulus was present in the series. He always heard tone for b, c, and d and never tone for e and f. He was, it must be added, a more practiced observer than many of the others, and tended for this reason to reject doubtful judgments.

3. In general the cylinders indicate that the limen is less than 20,000 vibrations. The limens for cylinders cited by Titchener[4] are 20,000 vs. Stumpf mentions the "fast allgemein angenommene Tongrenze von 20,000 Schwingungen."[5] If we take the averages in our table and compute a limen by Urban's method (phi-gamma hypothesis) we get a value of 18,610 vs. The limen is also dependent upon intensity, which is seldom controlled in experiments and almost never calibrated. Nevertheless it would seem that the recent theories of vowel sounds that require a c of 32,000 vs. must be failing to take account of the facts of the limen.

4. Observer 1, who has the lowest limen, is older than any of the other observers. There is, however, no other evidence of age differences. Observer 1 is between 40 and 50 years of age; observers 2, 6, and 12 are between 30 and 40; all the other observers are between 20 and 30. It happens that the three observers with the highest limens are the most musically trained.

5. One observer, in characterizing the quality of the high tones, described it as a "singing" sound and likened it to a hiss. This observation suggests that even in instruments so little vocal as struck steel cylinders vowel-quality may be present. It is possible, however, that the "singing" sound arises from the beating of the two adjacent tones that Koenig thinks these cylinders produce.[6]

6. Our work contrasts strikingly with work by the Galton whistle in that doubtful judgments with the cylinders are unusual. The scarcity of doubtful judgments is, of course, to be accounted for in part by the use of stimuli so far above and below the limen, but it also appears that doubtful judgments are exceptional even for the critical stimulus. If this conclusion is borne out in further investigation, it will be desirable to use cylinders for liminal determinations instead of the whistle. The psychologist would need to have made up, however, a set of at least thirty cylinders lying within the octave $g^6 = 12288$ to $g^7 = 24576$.

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[4] *Op. cit.*, 41.

[5] *Bericht über den VI Kongress für experimentelle Psychologie*, 1914, p. 327.

[6] Koenig, *op. cit.*, p. 722.

[7] Titchener, *op. cit.*, p. 40.

7. Although intensity is a factor affecting the limen,[7] its control does not appear to be difficult with this instrument. The determinations of relative frequency given in our table are not significantly altered by slight variations in the strength of the actuating blow of the metal hammer nor by the distance of the observer from the apparatus. A light, hard, rubber hammer in place of the brass gives, however, a noticeably lower limen.

8. The set of cylinders as it stands is extremely useful for demonstrational purposes. We have used it with great success in a small classroom, 21 x 30 ft., seating about 25 persons, to demonstrate individual differences. The observers consistently fail to hear tone at different points in the series and their judgments are apparently not greatly altered by suggestion from the other members of the class. It remains to be seen whether the same demonstrational experiment can be performed for large introductory courses with one or two hundred students.

BOOK REVIEWS

Psychologie und Verkehrswesen. By HANS A. MARTENS. (*Heft* 10 of *Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens.*) Edited by OTTO LIPMANN and WILLIAM STERN, 1919. 14 p.

The author discusses first the general value of psychological principles in the training and selection of traffic personnel, in the certification of professional chauffeurs, and the licensing of private automobilists. He then takes up, in survey fashion, the psychological factors of the signal system. He enumerates the psychological requirements of good signals and states the problems involved in learning and reacting to them and in avoiding accidents and especially collision with pedestrians. His main object is to show that like all other phases of economic life, transportation and traffic need the co-operation of consulting psychological experts in solving all those problems which involve the human element as an important constituent. L. R. G.

Die Psychische Eignung der Funkentelegraphisten. By OTTO LIPMANN. (*Heft* 9 of the *Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens.*) Edited by OTTO LIPPMANN and WILLIAM STERN, 1919. 40 p.

The first part of this article deals with the analysis of the mental functions of wireless telegraphers. Both receiving and sending wireless messages require good memory and ability to learn quickly. In sending messages, accuracy, speed, and certainty in movements are essential, but the sender is usually not limited in speed nor exposed to distractions, which two factors make the receiving a much more difficult task. Since the signals to be received and recorded are faint tones from 500 to 1,000 vibrations, the listener must be able to differentiate accurately both pitch and intensive differences; he must have a good memory for pitch in order to identify thereby the stations communicating with him; and he must be able to recognize different rhythms of sound signals and the changes they undergo when their speed is altered. He will be the more efficient the less he needs to translate these sounds into visual or motor terms. Subjective types of receivers who interpret what they hear are less valuable, at least from the militaristic point of view, than the objective type who merely writes down what he hears. The process of recording involves a division of attention between hearing the present sound and simultaneous writing what had just been heard. Finally, the receiver must be able to disregard a large number of distracting and conflicting noises from other stations and confine his attention exclusively to the particular sender with whom he is trying to communicate. These auditory disturbances offer the greatest difficulty to the beginner in wireless telegraphy and prove sometimes too much for him.

In the second part the author describes a complex method of testing beginners in wireless telegraphy for the purpose of determining their fitness for such work. The five letters, k, m, r, s, and v, of the Morse alphabet were employed as stimuli, at a constant pitch of 1,000 vs., presented automatically by a special sending apparatus which transmitted the sounds through a number of individual telephone receivers to a group of learners. The stimuli were given in successive series in different orders, at four different degrees of intensity, and at seven different rates. In some of the series disturbing secondary sounds of 600, 700, and 900 vs. were introduced. Each observer wrote down what letters he heard, as nearly as possible in the order in which he heard them, and he was scored on the basis of the number of correct replies.

The last part gives the results of these experiments with seventy-one observers and describes also the methods of computing the data from various points of view, in order to take account of practice, fatigue, and distractibility. The index of efficiency for a given observer is based upon the combined results of all the series, which had previously been weighed. Two methods of weighing the series are discussed and compared, and a positive correlation coefficient of .8264 with a P. E. of .02656 is found for them. Unfortunately the experimental results from these 71 observers could not be compared with the outcome of the training which they underwent or with their success in actual service, although another investigator, who has also used this method as part of a much more extensive study, is reported to have had good results with it. L. R. G.

BOOK NOTES

Taboo and Genetics. By M. M. KNIGHT, IVA L. PETERS, and PHYLLIS BLANCHARD. N. Y., Moffat, Yard and Co., 1920. 301 p.

This book is an attempt to help the many who are now thinking much about sex relations and family. It is frank and in simple language and is sociological. The three authors have divided the field between them as follows: Dr. Knight writes the chapters on The New Biology and The Sex Problem in Society; Dr. Peters, those on The Institutionalized Sex Taboo, and Dr. Blanchard on The Sex Problem in the Light of Modern Psychology.

A General Introduction to Psychoanalysis. By SIGMUND FREUD. (With a Preface by G. STANLEY HALL.) N. Y., Boni & Liveright, 1920. 406 p.

The substance of this volume was evidently given in popular lectures to a non-medical audience, and its twenty-seven chapters cover pretty completely the entire field from the standpoint of Freud. After three introductory chapters the next eleven are devoted to phases of The Dream, and the remainder to the General Theory of the Neuroses. All those who want the latest authentic statement of what psychoanalysis is and means, and to know the attitude which Freud takes toward his critics, will welcome this book.

The Psychology of Functional Neuroses. By H. L. HOLLINGWORTH. N. Y., D. Appleton and Co., 1920. 259 p.

The author represented psychology in the U. S. A. General Hospital at the Plattsburg Barracks, N. Y., and saw in all nearly 1,200 cases, most of which were examined, some merely enough to test mental age while several hours each were spent on other cases. Midway in the author's stay came the armistice, the effects of which were full of psychological interest. No attempt seems to have been made to analyze intensively any case, but general ideas of this group of cases were chiefly kept in mind. There are almost no references to the really copious recent literature on the subjects here treated. Perhaps the author's most interesting results are those derived from his 116 questions which he asked his patients to answer. He takes an essentially negative attitude toward Freudianism. Desirable as it is that the psychological standpoint should be exploited to the very uttermost in the study of psychoneurotic cases, it is very essential that the author should have no psychological bias or at least should not display it.

Man's Unconscious Passion. By WILFRID LAY. N. Y., Dodd, Mead and Co., 1920. 246 p.

This book deals with The Total Situation, especially Pleasure-Pain vs. Reality; Conscious and Unconscious Passion; while chapter III distinguishes at great length between affection and passion. Then follow chapters on Insight, The Transfer of Passion, The Emotion Age, etc. The author certainly writes with more abandon here than in his two earlier books.

The Group Mind. By WILLIAM McDOUGALL. N. Y., G. P. Putnam's Sons, 1920. 418 p.

This is "a sketch of the principles of collective psychology with some attempt to apply them to the interpretation of national life and character." The work falls into three parts: I, General Principles of Collective Psychology; II, The National Mind and Character; and III, The Development of National Mind and Character. A book of this ambitious sort will just now challenge attention as never before, but to the mind of the present writer it adds very little to our knowledge and its value will depend upon whether the author's highly schematic mind working in this complex field will succeed in having its *Leitmotive* accepted.

Mind and Energy. By HENRI BERGSON (tr. by H. WILDON CARR). N. Y. Henry Holt and Co., 1920. 262 p.

The publisher boldly tells us on the cover of this work that it "is the most important philosophical volume which has appeared since the same author's world-famous 'Creative Evolution.' It gathers together the threads of 'Matter and Memory,' 'Creative Evolution,' and 'Time and Free Will,' and with additional arguments and undiminished power drives home the conviction of the freedom of the will and the ascendancy of soul over matter." The book consists of lectures delivered at various times and places, with little and certainly no successful effort to bring them into unity.

Ethics and Natural Law. By GEORGE LANSING RAYMOND. N. Y., G. P. Putnam's Sons, 1920. 345 p.

This is a reconstructive review of moral philosophy applied to the rational art of living. This very voluminous author always writes well, and if perhaps too copiously compensates by giving excellent analyses of his chapters.

Human Traits and Their Social Significance. By IRWIN EDMAN. Bost., Houghton, Mifflin Co., 1920. 467 p.

This book was written for use in a course entitled "Introduction to Contemporary Civilization" for Freshmen and gives a bird's-eye view of the processes of human nature from man's simple inborn impulses on to their fulfillment in religion, art, science, and morals. The first part analyzes types of behavior; and the second, the four great activities of the human mind and imagination—religion, art, science, and morals.

Proceedings of the International Conference of Women Physicians. N. Y., Y. W. C. A. Press, 1920.

The proceedings of this most important conference are published in six volumes on as many topics which engaged the attention of the conference in 1919, viz.: General Problems of Health, Industrial Health, The Health of the Child, Moral Codes and Personality, Adaptation of the Individual to Life, Conservation of Health of Women in Marriage. The calling of this conference just after the war to consider these fundamental questions seemed to the writer almost an inspiration, and if all the expectations formed were not realized it should not be ascribed to the fault either of the management or delegates but rather to the vast range and fundamental character of the topics treated.

Industrial Psychology. By BERNARD MUSCIO. N. Y., Dutton, 1920. 300 p.

These lectures were designed for general audiences and were given to workmen in 1916. The author first describes the aim of industrial psychology, then discusses the mental factors relevant to industry, selection of workers on the basis of natural fitness, the best method of work, and closes with a chapter on the desirability of applying psychology to industry.

Activism. By HENRY LANE ENO. Princeton U. Press, 1920. 208 p.

The author of this book is research associate in psychology at Princeton. He here discusses activity as an underlying hypothesis in this field, its planes and their interrelation, and units; consciousness, the meta-psychic plane, and activism and the historic problems of philosophy.

The Child Vision. By DOROTHY TUDOR OWEN. Lond., Longmans, Green and Co., 1920. 180 p.

This is a study in mental development and expression and is devoted especially to the art of composition. It is, on the whole, said to develop a method which seems to be interesting and valuable.

God's Smile. By JULIUS MAGNUSSEN. N. Y., D. Appleton & Co., 1920. 185 p.

This writer takes an unconscionable length of time, with many irrelevant details, to tell us of a very remarkable experience which befell him, a literary man and journalist, the results of which made him more or less of a medium and a believer in spiritism. The conviction that he had really received a letter from his dead father seems to have changed his life. A number of these letters are printed.

Famous Psychic Stories. Ed. by J. W. MCSPADDEN. N. Y., Thos Y. Crowell Co., 1920. 305 p.

Here are twelve tales of very different nature thought by the editor to be psychological. They are by Hawthorne, Poe, Collins, Oliphant, M. R. James, Benson, etc.

Success Fundamentals. By ORISON SWETT MARDEN. N. Y., Thos. Y. Crowell Co., 1920. 307 p.

These are uplift talks on health and success, efficiency, how to find yourself, confidence, the law of opulence, and salesmanship.

Concentration. By CHRISTIAN D. LARSON. N. Y., Thos. Y. Crowell Co., 1920. 94 p.

Intelligence and Social Valuation: A Practical Method for the Diagnosis of Mental Deficiency and Other Forms of Social Inefficiency. By Richard A. Beery and S. D. Porteus. Publ. by The Training School, Vineland, N. J., 1920. 100 p.

Leib und Seele. By G. Sommer. Leipzig, Teubner, 1920. 128 p.

Einführung in die Psychologie. By Alexander Pfänder. Leipzig, Barth, 1920. 382 p.

Das Problem der Gültigkeit in der Philosophie David Humes: Ein Kritischer Beitrag zur Geschichte der Erkenntnistheorie. By Heinrich Hasse. Munich, Reinhardt, 1920. 193 p.

Die Raumwahrnehmung der Blinden. By Wilhelm Steinberg. Munich, Reinhardt, 1920. 151 p.

Theorie der Empfindungsstärke und insbesondere des Weberschen Gesetzes. (*Heft* 3 of *Schriften zur Anpassungstheorie des Empfindungsorganes.*) By Julius Pikler. Leipzig, Barth, 1920. 26 p.

Das Denken und die Phantasie. By Richard M. Freienfels. Leipzig, Barth, 1916. 341 p.

Pengard Awake. By Ralph Straus. N. Y., D. Appleton and Co., 1920. 299 p.

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THE AMERICAN JOURNAL OF PSYCHOLOGY

Thirty-four years ago, in November, 1887, I falteringly issued the first number of The American Journal of Psychology. There was then no other periodical on this subject in the English language except *Mind,* which was established in 1876 by Croom Robertson, who had told me the interesting story of its inception in a way that filled me with emulation. *Mind* represented advanced British thought, although it had printed not a few of my own articles and those of my students before we had an organ of our own. Both its spirit and its field were very different from the journal I intended, which I wanted to have, first of all, an exponent of experimental laboratory psychology. As, however, there was then no such laboratory in the country save my own at the Hopkins, the supply of material for the six hundred pages per annum that our prospectus had announced was very meager. This was fortunate for the *Journal;* for we had to make a virtue of necessity and include non-experimental articles over a wide range, drawing upon the fields of philosophy, logic, aesthetics, and religious, educational, pathological and comparative psychology. Even thus the supply of acceptable materials was scanty. Hence a very large part of the early volumes was devoted to book-reviews, excluding none of these fields. The vast majority of this small-type material was written by myself, and this required diligent and almost incessant reading and epitomizing.

Moreover, the *Journal* was a personal enterprise, and for years it was a heavy drain upon my own purse. My hopes and expectations had been extravagant, as I realized to my grief. Subscribers were few, and while there were some cordial expressions of appreciation, *e.g.* from James and Ladd, there was plenty of criticism from conservative instructors in this general field like Dr. McCosh, who felt that the position he had taken in his psychology, that mind and body were utterly disparate and incommensurable, was challenged in our prospectus. The negative attitude of the *Journal* toward telepathy and the English psychic researchers lost us not a few subscribers and alienated a patron who had made a generous and unsolicited contribution—the only one the *Journal* ever had—on the assumption, as it afterwards appeared, that the *Journal* would favor this cult. Thus the second year saw an actual reduction of our subscription list, and only with the third volume did we find our own; and most of the subscribers to that volume are with us yet. Thus for years I was editor, publisher, chief contributor, and sole financial support of the *Journal,* in which at its darkest period I had sunk over eight thousand dollars of my own meager savings, which I saw little prospect of ever regaining. But I was committed to the enterprise and must "carry on." And in this I was encouraged, first by the spirit of Hopkins, and later by the generosity of Jonas G. Clark, our Founder, who employed Dr. Sanford to edit the *Journal* during 1888-89, when I was abroad. With the growth of departments of psychology in the country, and after the organization of The American Psychological Association at Clark in 1890, the fortunes of the *Journal* began to improve, and they have done so slowly and surely ever since.

The chief good fortune in the history of the *Journal* was the advent of E. B. Titchener of Cornell as one of the associate editors in 1895, and gradually the contributions from him, his laboratory, and his pupils have taken the predominant place in its pages. In all the vicissitudes of the *Journal* he has stood loyally by, and but for his coöperation the *Journal* would have had a far different and less useful and honorable career. New journals have appeared, several of which at first seemed likely to interfere with our circulation, but the record shows that that of the *American Journal* has steadily increased.

In transferring the control of the *Journal,* as I do with this number, to its new editor, I feel, as is only natural, that to part with the child that has occupied so much of my care for so long marks a break which is less only than retiring from Clark, which is only slightly younger than it. Professor Titchener is, however, my natural successor and heir, the only one to whom it ought to go, and under whose management I am rejoiced to feel that the *Journal* will enter upon a new era. I believe that he will not only realize many hopes of my own that I have not been able to bring to fulfillment, but will make the *Journal* a leader and a light as it has never been before. I have asked and received no assurances from him regarding the fate of any of "my policies," so that he is absolutely free to do and make what he will of the *Journal.* But I know something of his ideals, and they have not only my most enthusiastic endorsement, but I bespeak for him all the goodwill and support both from contributors and subscribers that the *Journal* has previously enjoyed, and am confident that under his guidance it will enter upon a larger career of service than it has hitherto known. On the day Clark University opened, the University of Berlin cabled us its *Vivat, Crescat, Floreat.* This is my message to my successor.

G. STANLEY HALL.

The most welcome announcement that I can make to readers of the *Journal* is, I am sure, this: that Dr. Hall does not wholly sever his connection with us, but on the contrary promises us his active sympathy and support. I wish we could make him realize how much this means to us!

As for policies, I did not know that he professed any,—unless, indeed, a wide interpretation of the word 'psychology,' a generous catholicity of attitude toward every movement that touched psychology and might help to advance it, an eager and yet discriminating enthusiasm for new psychological trends, and a steady adherence to the ideals and standards of scientific work, may be said to constitute policies. I shall, at all events, try in these regards to follow as closely as I many in Dr. Hall's footsteps. We cannot improve upon those early volumes of which he speaks so modestly; but I hope, with the valued aid of my co-workers, to make the future *Journal* worthy of the past.

E. B. TITCHENER.

EARLY PSYCHOLOGICAL THEORIES
OF
HERBERT SPENCER

By George Bion Denton
Northwestern University

The mature views of Herbert Spencer upon the science of psychology were conceived somewhat under the influence of British philosophy and more under the influence of Biological Evolution. But earlier, he held views representing, in the main, a quite different development. Though the British philosophy was already present, the evolutionary thinking was replaced by a heterodox phrenology. These earlier views might, perhaps, be disregarded—as Spencer himself disregarded them in his *Principles of Psychology*—if it were not that they "adumbrated" (as he might have said) theories which have been developed—and supposedly have been originated—within recent years. Presenting crude notions of Fatigue and of Attention, they constitute a real, though isolated, chapter in the history of modern psychology.

The later psychological views of Spencer were, of course, developed at length in his *Principles of Psychology*, published in 1855; his earlier views, in their completest form, furnished the basis of psychological theory for the essay *The Philosophy of Style*. It may seem remarkable that these two works, representing radically different points of view, should have been published within three years of each other; and the difficulty is increased by the fact that Spencer's letters of the spring of 1852 show that by that time he had already planned his *Psychology* and was at work upon it.[1] The difficulty is removed by the hypothesis that *The Philosophy of Style*, being a revision of an essay, *Force of Expression*, written about 1844,[2] differed very little in substance from the earlier essay, except in an added paragraph at the close designed to give the essay an evolutionary turn.[3]

---

[1] *Autobiography*, I, 452-454.

At the age of eleven or twelve, Spencer became a believer in phrenology, and continued such for many years.[4] In time, however, his views ceased to be in strict accord with the leaders of phrenological theory,[5] and the three articles[6] which Spencer published in the *Zoist,* a phrenological journal, in 1843 and 1844, were all heretical. Early in 1846 Spencer devised a Cephalograph, an instrument for making accurate measurements of skulls. Before executing a workable instrument of this design, he became entirely skeptical of phrenology,[7] and his interests turned to other schools of psychology. The *Principles of Psychology,* when it appeared in 1855, was, on the whole, opposed rather than inclined to phrenology, and a passage of several paragraphs[8] was devoted to a judicious criticism of phrenology as a system and to a denial of some of its leading positions.

In *The Philosophy of Style,* Spencer did not attempt to set forth a complete system of psychology, phrenological or otherwise, but attempted to expound only such principles of mental operation as would explain the effective use of language in discourse. Three features of this psychology are especially interesting—the implied theories of the Faculties, of Fatigue, and of Attention.

THE FACULTIES

By such expressions as "every faculty"[9] and "group of faculties,"[10] in *The Philosophy of Style,* Spencer implied that the faculties were numerous. He named several, such as the "faculty of reverence"[11] and the faculty of "approbation."[12]

These faculties, he apparently regarded, not merely as subjective entitles, modes of operation of the mind, but as physiological organs. His theory of fatigue, which will be dealt with in the next section, is evidence of their physiological nature.

---

[2] *Ibid.,* I, 258; 468-469; *Life and Letters,* I, 86.

[3] The proof of this hypothesis the writer has undertaken in a separate study.

[4] *Autobiography,* I, 227-231.

[5] *Ibid.,* I, 261, 282; *Life and Letters,* II, 310.

[6] "A New View of the Functions of Imitation and Benevolence," *Zoist,* I (1843); "On the Situation of the Organ of Amativeness," and "A Theory concerning the Organ of Wonder," *Zoist,* II (1844).

[7] *Autobiography,* I, 634.

[8] Part IV, ch. 8, 606-611.

[9] *Westminster Review,* 58 (1852), 446.

[10] *Ibid.,* 455.

[11] *Ibid.,* 456.

[12] *Ibid.,* 456.

The faculties were roughly classified. " . . . In the reflective faculties," he wrote, " in the imagination, in the perceptions of the beautiful, the ludicrous, the sublime, in the sentiments, the instincts, in all the mental powers, however we may classify them . . . . "[13] Apparently this was not meant for a systematic classification, but it seems possible to distinguish three or four groups, such as " reflective faculties," " sentiments," " instincts," and possibly " perceptions." Spencer frequently spoke of " perceptive faculties,"[14] at other places in the essay.

Nothing comparable to these minute and numerous faculties suggested in *The Philosophy of Style* appeared in the *Psychology*. In the latter work, the structural elements of mind were the same as those of the British sensationists. The aspects of Intelligence which Spencer treated were Reflex Action, Instinct, Memory, Reason, the Feelings, the Will,[15] and none of these (as he carefully pointed out) is distinct from another.[16] In the *Psychology* his chief criticism of phrenology was that it made too sharp the demarcation of the faculties.[17] On the contrary, in *The Philosophy of Style* it is the distinctness and the unalterable character of the individual faculty that explains the effect of such devices as Antithesis, Climax, and Anticlimax. " The opposition of two thoughts that are the reverse of each other in some prominent trait insures an impressive effect; and does this by giving a momentary relaxation to the faculties addressed."[18]

This feature of Spencer's early psychology was equally unlike anything held by the British philosophers. The latter, though differing considerably among themselves in regard to the number of faculties, usually conceived a three to five-fold division, comprising such general faculties as Intellect, Emotion, Will. Moreover, none of the British philosophers, not even Hartley, associated the faculties with distinct organs of the brain.

When compared with the faculties as conceived by phrenologists, however, the faculties as represented in *The*

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[13] *Ibid.*, 455.

[14] *Ibid.*, 454.

[15] *Principles of Psychology,* Part IV, chs. IV-IX.

[16] *Ibid.*, 584.

[17] *Ibid.*, 609.

[18] *Westminster Review,* 58 (1852), 456.

Philosophy of Style were (it is hardly necessary to show) very similar to those of phrenology; and such phrenological names as "reverence," "ideality," and "approbativenes,"[19] for example, corresponded to Spencer's "reverence," "beauty," and "approbation."[29] The phrenological faculties, too, like Spencer's, were distinct, physical, and anatomically localized.[21] Similarity in the classification of the faculties may also be noticed, for Spurzheim divided them into Intellectual and Affective, with subdivisions of Reflective and Perceptive under the first head, and Sentiments and Propensities under the second.[22]

Fatigue

Spencer's theory of fatigue, stated most definitely in the latter part of the essay, was implied throughout. In the third paragraph, explaining the general principle of economy of attention in the use of language, Spencer wrote: "A reader or listener has at each moment but a limited amount of mental power available. To recognize and interpret the symbols presented to him requires part of this power: to arrange and combine the images suggested requires a further part; and only that part which remains can be used for the realization of the thought conveyed."

Later, a more explicit statement was made: "Without going at length into so wide a topic as the exercise of faculties and its reactive effects, it will be sufficient here to call to mind that every faculty (when in a state of normal activity) is most capable at the outset; and that the change in its condition, which ends in what we term exhaustion, begins simultaneously with its exercise. This generalization, with which we are all familiar in our bodily experiences, and which our daily language recognizes as true of the mind as a whole, is equally true of each mental power, from the simplest of the senses to the most complex of the sentiments. If we hold a flower to the nose for long, we become insensible to its scent. We say of a very brilliant flash of lightning that it blinds us; which means that our eyes have for a time lost their ability to appreciate light. After eating a quantity of honey, we are apt to think our tea is without sugar. The phrase 'a deafening roar,' implies that men find a very loud sound temporarily incapacitates them from hearing faint ones. Now, the truth at once recognized in these, its extreme manifestations, may be traced throughout; and it may be shown that . . . . in all the mental powers . . . . action exhausts; and that in proportion as the action is violent, the subsequent prostration is great."[23]

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[19] Spurzheim, *Phrenology,* 225-228; 243-245; 212-214.

[20] *Westminster Review,* 58 (1852), 456.

[21] Spurzheim, *Phrenology,* 90-98.

[22] *Ibid.,* 149-151.

Discussions of fatigue as a factor in mental phenomena are common enough in the psychologies of today, but in 1852 and before one would have looked for them in vain in the works of British psychologists and philosophers. Some phases of Spencer's own biological view of psychology set forth in the *Psychology* would have accommodated his earlier theory of fatigue, but nothing of the sort appeared in that work.

Phrenologists, however, made considerable use of the idea of fatigue. "The faculties of animal life," wrote Spurzheim, "cannot act incessantly, they require repose. Study of the same subject, too long protracted, causes fatigue; by changing this we may still continue our labors. Now if the brain were a single organ, that performed all the functions of the mind, why should it not be still further fatigued by this new species of action? Although our eyes be fatigued by looking at pictures, we can still listen to music, because there is a particular organ for each of these sorts of impressions."[24] "As during watching the same organ is not always active, but reposes at intervals; so, during sleep, all the organs do not sink into inactivity together, but a particular one continues its function, and then the peculiar state called dreaming supervenes. . . . Every corporeal organ being fatigued takes rest, and this state of rest is sleep; but single, or even several organs, may be active while the others repose."[25]

ATTENTION

Attention is the most interesting of Spencer's early psychological conceptions. In the first part of the essay (consisting of fifty paragraphs) attention was the most comprehensive power of intellect, and its economy was represented as the great desideratum in the use of language.

In several respects, attention, as Spencer conceived it, had features in common with attention as it is generally regarded by psychologists today.

On the subjective side, the result of attention upon the idea seems to be to increase the effectiveness of the idea.

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[23] *Westminster Review,* 58 (1852), 437.

[24] Spurzheim, *Phrenology,* 95.

[25] *Ibid.,* 96.

Spencer used commonly the terms "vivid,"[26] "forcible,"[27] and "effective"[28] to denote the influence of attention upon the content of mind. Other subjective features, such as the "selective" function of attention and its effect upon retention in memory, scarcely arose for consideration.

The physiological character of Spencer's view of attention is most noteworthy. Attention is apparently a reservoir of physical energy at the service of mental life. Every mental act of any sort requires attention; it uses up energy. The energy of attention is associated with the activity of the whole mind, or rather with every part of it. It is a fixed amount—"A reader or listener has at each moment but a limited amount of mental power available"[29]—and it may be temporarily drained to varying degrees of fatigue or exhaustion. The attention is "absorbed"[30] or "frittered away,"[31] or there is a "strain upon the attention."[32] Moreover, it may be drained through many channels; that is, it is applicable to the needs of any faculty. Finally, attention reenforces the activity of specialized portions of the brain; and apparently this physiological reenforcement corresponds to the mental effectiveness produced by attention. Attention may be taxed by severe demands upon one faculty or by multitudinous demands upon many faculties simultaneously. In one place Spencer began, " . . . If some subtlety in the argument absorb the attention—if every faculty be strained in endeavoring to catch the speaker's or writer's drift—"[33] Very clearly "straining every faculty" is the same as "absorbing the attention;" and indicates as clearly as may be in brief the relation of attention and faculties, explained above.

Dominant as attention was in *The Philosophy of Style,* it did not appear again as a psychological entity of importance in Spencer's works. In *The Philosophy of Style,* the word "attention" appeared seventeen times in fifty paragraphs.[34] In *Principles of Psychology,* the word appeared twenty times in six hundred and twenty pages. Six[35] of these latter instances had no psychological significance, and none of the others[36] indicated anything like a theory of attention.

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[26] *Westminster Review,* 58 (1852), 439.

[27] *Ibid.,* 440.

[28] *Ibid.,* 451.

[29] *Ibid.,* 437.

[30] *Ibid.,* 438.

[31] *Ibid.,* 451.

[32] *Ibid.,* 454.

[33] *Ibid.,* 446.

[34] *Ibid.,* 436, 437 (three times), 438 (twice), 439, 440, 443, 446, 449 (twice), 451, 454 (four times).

[35] *Principles of Psychology,* 122, 127, 209, 363, 449, 616.

Whence could the suggestion for his theory of attention have come? Modern writers upon psychology assert that the British thinkers did not study attention, as it is conceived today. "With some notable exceptions (Wolff, Kant, and James Mill) the attention was greatly neglected until more modern times, notably by the English empiricists. . . . It was considered an unanalysable attribute of the soul, and direct evidence of the independent activity of the mental principle (Hamilton, Carpenter, McCosh)."[37] Professor James wrote: "Strange to say so patent a fact as the perpetual presence of selective attention has received hardly any notice from psychologists of the English empiricist school. The Germans have explicity treated of it, either as a faculty or as a resultant, but in the pages of such writers as Locke, Hume, Hartley, Mill, and Spencer the word hardly occurs, or if it does, it is parenthetically and as if by inadvertence."[38]

Such writers, British and Continental, as may have treated of attention were probably all unknown to Spencer. He was never well read in philosophy, and the little that he knew by 1852 was mostly second hand. His own statements[39] in regard to his philosophical reading would make certain that no suggestion for his conception of attention could have been derived from the philosophers.

Attention was certainly not a fundamental and characteristic conception of phrenology; yet it was a conception that the phrenologists were constantly meeting on the Continent. Such conceptions as Memory, Association, and Will, Spurzheim did not directly attack, but reinterpreted in harmony with phrenology. Similarly, with attention. He wrote: "The word *attention* denotes no more than the active state of any intellectual faculty; or, in other terms, attention is the effect of the intellectual faculties, acting either from their proper force, or from being excited by external impressions, or by one or several affective faculties. Hence there are as many species of attention as fundamental faculties of the mind. . . . It is, indeed, absurd to expect success in an art or science, when the individual power on which its comprehension depends is inactive. Again, the more active the power is, the more it is attentive."[40]

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[36] *Ibid.*, 194, 195, 235, 281, 282, 299, 315, 477, 495, 503, 546.

[37] Baldwin, *Dictionary of Philosophy and Psychology*, article on "Attention."

[38] James, *Principles of Psychology*, I, 402.

[39] *Life and Letters*, I, 145-147.

Coupled with the phrenological view of physiological exhaustion of the faculties, this doctrine would hold that attention is the state of greatest physiological activity of an " organ " of the brain; and the greater the task of the organs or faculties, the greater the exhaustion of the attention. There was much in this phrenological view that is like Spencer's theory in *The Philosophy of Style.*

The conception of attention which Spurzheim was forced grudgingly to admit existed as a mental phenomenon of secondary importance, Spencer, the phrenological heretic, might well have welcomed as a fundamental organ of mind. Certain features of Spencer's article, " A Theory concerning the Organ of Wonder," published in the *Zoist* in 1844, point to such a view.

Spencer there proposed that what was commonly regarded by phrenologists as the Organ of Wonder was in truth an organ whose main function is to recall impressions once received.[41] This theory was proposed by Spencer as supplementary to an article of the previous year, entitled *" Imitation and Benevolence."*[42] In that article he reinterpreted the Organ of Imitation into an organ whose function was to excite sympathetic states of mind, while the Organ of Benevolence became " the grand centre of sensation, and is excited by the affections of all the other organs."[43] The names for the three organs, Wonder, Imitation, and Benevolence, became then Revivisence, Sympathy, and Sensitiveness, respectively. The whole theory was summed up as follows, in the later article: " It was maintained that it is the primary office of the organ entitled Imitation, to excite in the mind of one being the feelings exhibited in another, and it is the aim of the present essay to show that the true duty of the adjoining organ, hitherto called Wonder, is the revival of intellectual perceptions. It is the object of both to bring certain other faculties into activity. By the one, feelings are recalled; by the other, impressions."[44]

The next to the last sentence is to be noted: Revivisence acts as a reenforcement of the intellectual faculties. In this respect, although applied to the service of memory only, it is like attention in *The Philosophy of Style,* and might have been adapted to the needs of that essay.

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[40] *Phrenology,* 380.
[41] *Zoist,* II (1844), 316-325.
[42] *Zoist,* I (1843).
[43] *Ibid.,* 377.
[44] *Zoist,* II (1844), 322.

Interesting in this connection is the fact that Dr. Bernard Hollander has identified Spencer's Organ of Wonder with the modern conception of attention. Dr. Hollander pointed out that Spencer, by assigning to that organ the function he did assign, anticipated the work of Professor Ferrier in regard to localization of functions in the brain.[45] And this conclusion Dr. Hollander arrived at, apparently, without knowledge of Spencer's use of attention in *The Philosophy of Style.*

Dr. Hollander quoted Ferrier in regard to the function of "area 12," which corresponds to the Organ of Wonder: " . . . the excitation of which causes the eyes to open widely, the pupils to dilate, with movements of the eyeballs and head. It gives the appearance of attention, and the movements indicated are essential to the revivification of ideas."[46]

If, as Dr. Hollander believed, Spencer's Organ of Revivisence anticipated the modern notion of the function of attention and its location in the brain, then it is very likely indeed that, in *The Philosophy of Style,* attention is merely a development and new application of the phrenological theory of revivisence.

That Spencer should have changed the name of attention in the essay on style is easily understood. For one thing, the application to memory was not largely involved in the later essay. For another, Spencer admitted that the name "revivisence" was awkward.[47] Finally, it is probable that in writing for *Westminster Review* (in which the essay on style appeared), Spencer was not desirous of suggesting totally unfamiliar terms, or phrenological terms which might call his psychology in question. He probably adopted the name "attention" because it was used by phrenologists in somewhat the same sense as he employed it, and because he found it occasionally in works on rhetoric[48] from which he borrowed considerably in writing *The Philosophy of Style.*

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[45] *Westminster Review,* 139 (1893), 142-154.

[46] *Ibid.,* 148.

[47] *Zoist,* II (1844), 320.

[48] Spencer quoted from Blair the statement that "long sentences fatigue the reader's attention." See also, Denton, "Herbert Spencer and the Rhetoricians," in *Publications of the Modern Language Association of America,* XXXIV (1919), 89-111.

British Sensationism and Associationism

The presence of phrenology in *The Philosophy of Style* did not preclude the use of British sensationism and associationism in that essay. As has already been said, when the theories of rival systems did not directly conflict, phrenologists were likely to admit their validity as explaining phenomena of secondary importance. The doctrines of the British philosophers explained some points where phrenology was vague or non-committal, and therefore they were sometimes given a qualified acceptance by phrenologists. In opposition to *tabula rasa,* the phrenologists believed that varying developments of the organs of mind produced varying experiences and characters,[49] but this doctrine did not exclude the operation of sensation and association but merely complicated it. Phrenologists admitted that it is through the medium of the external senses " that determinate consciousness of the external world is acquired,"[50] and they made some use of association.[51]

In *The Philosophy of Style* Spencer made use of the two fundamental assumptions of British psychology—sensationism and association. Ideas exist in the mind only in the form of " images." When one starts to think " horse," there " tends to arise," (and when one completes the process, there does arise) "in the mind, a picture answering to the word," and the horse must be black, or brown, or some color at least. What is true in this regard of the more specific ideas is equally true of the most general concepts. " We do not think in generals but in particulars." When we think a class-idea, " we represent it to ourselves by calling to mind individual members of it." Or, in other words, to represent the general idea, the thinker, " has to choose from among his stock of images, one or more, by which he may figure to himself the genus mentioned."[52]

The assumptions of British associational psychology are obvious in many places in *The Philosophy of Style.* Associations were designated as " powerful " or " strong " in proportion to the " ease and rapidity of the suggestive action." An association becomes powerful through repetition.[53]

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[49] Spurzheim, *Phrenology,* Part I, sec. IX, ch. I; secs. IV and VII; Part II, sec. III.

[50] *Ibid.,* Part I, sec. IX, ch. I, 253.

[51] Combe, *A System of Phrenology,* 420; 499-503; Spurzheim, *Phrenology,* Part II, sec. I, 387-388.

[52] *Westminster Review,* 58 (1852), 439.

[53] *Ibid.,* 437-438. When the essay was republished in 1858, Spencer substituted the word " strong " for " powerful."

Perceptions of objects and processes of reasoning were regarded as complex, consisting of concatenations of simple ideas or impressions. The mind, after recognizing and interpreting the bare symbols, is obliged to arrange and combine the suggested images to constitute thoughts.[54] Even in the case of perceiving words, the syllables must be connected by the mind.[55] All thoughts are " built up " according to some natural or customary order of associating their elements; and for that reason, the elements of a sentence must be ordered according to such associations.[56] The force of suggestion depends upon association, for " each of the facts mentioned presupposing numerous others, calls up these with more or less distinctness, and revives the feeling . . . . with which they are connected in our experience."[57]

That Spencer was familiar with the terms and positions of British psychologists, at least to the extent shown in *The Philosophy of Style,* is evidenced in his writings known certainly to be of early date.[58] The source of his knowledge was probably not the British philosophers themselves. Much more likely, he took his doctrine second hand from the works of two rhetoricians, consulted in the preparation of the essay on style. Lord Kaimes' *Elements of Criticism*[59] and George Campbell's *Philosophy of Rhetoric*[60] contained accounts of British psychology sufficient to supply Spencer with all the knowledge he needed for his essay.

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[54] *Ibid.,* 436.

[55] *Ibid.,* 438.

[56] *Ibid.,* 439.

[57] *Ibid.,* 451.

[58] The terms are to be found here and there in " A Theory concerning the Organ of Wonder," in " Imitation and Benevolence," in the preface to his father's *Lucid Shorthand.*

[59] Ch. XVIII, sec. 2; also Appendix of Terms Defined, paragraphs 41 to 44.

[60] Bk. I, ch. V, part ii; ch. VII, sec. iii; Bk. II, ch. VII, sec. i.

THE COMPARATIVE INFLUENCE OF MAJORITY AND EXPERT OPINION

By Henry T. Moore, Dartmouth College

The literature on Social Psychology contains numerous references to the influence of the group on the opinions of the individual. This one point has been made the subject of practically an entire volume by Trotter,[1] who refers to group opinion somewhat picturesquely as the voice of the herd. He represents this voice as coming with such a weight of authority that even the most eccentric individual feels compelled to seek some form of herd support for his opinions, and is completely at a loss when no such support is anywhere to be had.

The general fact is beyond dispute, but those who would like to see Social Psychology multiply its experimental findings are tempted to ask more specifically just how great this influence may be expected to be in any given situation. Can we hope to measure it? And if so, how does it compare with other influences that are likely to operate in determining an individual's social attitudes?

The group experiment here reported attempts a beginning at answering these questions for three types of situation,—namely, speech, morals, and music. The method is somewhat similar to one used by Bridges[2] in a study of decision types reported in 1914. In general it consists of measuring a suggestive influence in terms of the number of reversals of judgment occasioned by it, as compared with the number that might have been expected by chance. The first problem was therefore to find out what was the chance of reversal of judgment in regard to each of the three kinds of material used. Ninety-five subjects were given eighteen paired comparisons for each of the three types of situation. The instructions for the linguistic judgments were that the subjects check the more offensive one of each pair of expressions. Examples of the expressions compared are: " Everybody loves their mother." "She sort of avoided him." "The party that wrote that was a scholar." "He never studies nights." The ethical judgments involved the checking of the more offensive of two traits of character in each of the eighteen pairs. Examples of the traits compared are:—disloyalty to friends; willingness to get rich by questionable financial methods; cheating on an examination; willingness to overlook a business error favorable to oneself. The musical judgments involved an expression of preference for one of two resolutions of the dominant seventh chord, played on a reed organ. Eighteen paired resolutions were played, and the preferences recorded after each.

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[1] Trotter, W., The Instincts of the Herd in Peace and War, 1916.

[2] Bridges, J. W., An Experimental Study of Decision Types and their Mental Correlates, *Psych. Rev. Mon. Sup.*, 1914, Vol. XVII, No. 1.

Two days later the same three series were repeated exactly as given before, and without the introduction of any special suggestive influence to alter the original judgments. Each subject was now scored on the basis of his percentage of reversals, and the mean of the ninety-five individual scores was taken as the chance of reversal for judgments concerned with that particular type of material. The average score thus recorded as representing the chance of reversal for linguistic judgments was 13.5 per cent with .55 P. E. of the mean; for moral judgments 10.3 per cent with P. E. of .50; for musical judgments 25.1 per cent, with P. E. of .84. These results are indicated graphically in the upper accompanying chart.

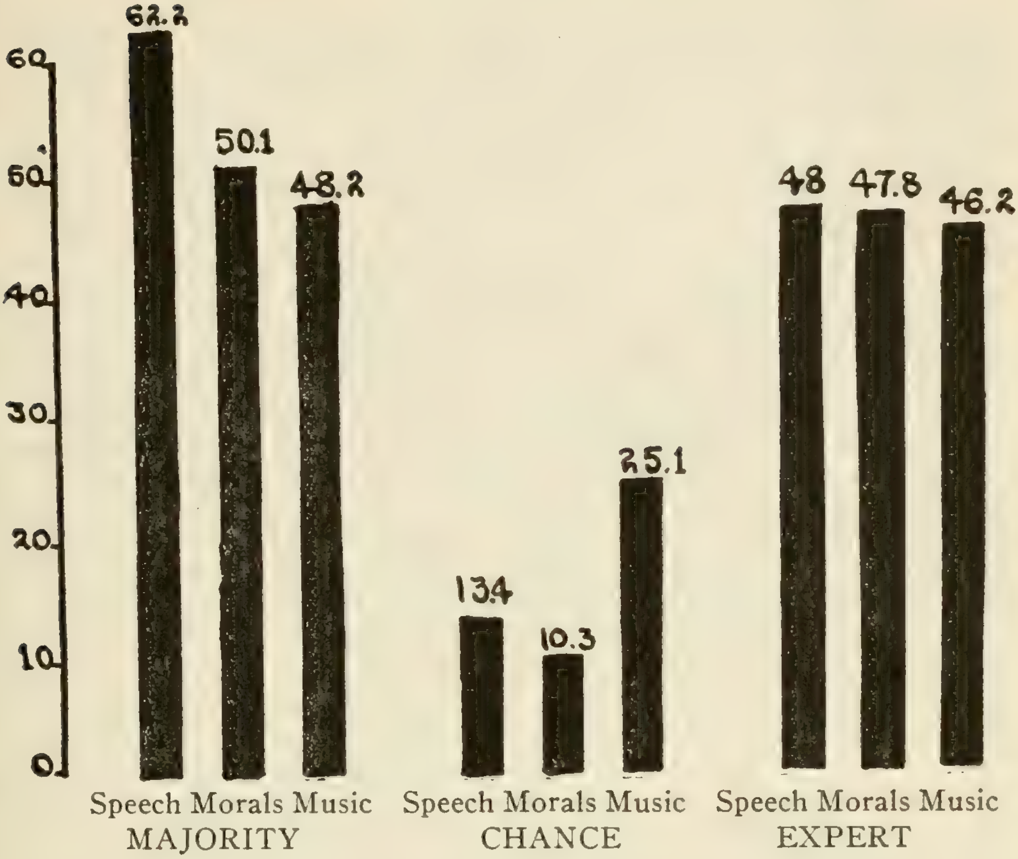
As a partial check on the above figures each of the three series of judgments was tried on a different group. The subjects in the check experiment were on an average about a year older than those in the original experiment, which probably accounts for their slightly lower per cent of reversals. Forty-three subjects gave 11.4 per cent chance of reversal in linguistic judgments; 62 subjects gave 9.4 per cent reversals in ethical judgments; and 49 subjects gave 22.6 per cent reversals in musical judgments.

An interval of two and a half months was allowed between the experiment without suggestion and that in which suggestion was used. This seemed ample time to render negligible any memory effects from the preceding judgments. The experiment was now repeated as before, except for the addition of the suggestive influences. A new set of original judgments was taken, and after a two day interval the subjects were given the same series again, this time with the statement of what had been the majority preference for each pair. Great care was taken to convince them that these statements were being truthfully made, and the influence of suspicion was certainly not great. Each subject was now scored on the basis of the per cent of opportunities he had accepted to reverse his judgment so as to agree with majority opinion. The results are indicated in the three left columns of the upper chart. The average of such reversals in linguistic judgments was 62.2 per cent, with P. E. of 1.63; in ethical judgments 50.1 per cent, with P. E. of 1.69; in musical judgments 48.2 per cent, with P. E. of 1.52.

Two days later still the same comparisons were repeated, and at this time each judgment was preceded by a statement of the opinion of an expert in each field. These statements in some cases coincided with what had been given as majority opinion previously, but as often as not they were at variance with it. From these last records each individual was scored on the basis of the per cent of opportunities he had accepted for reversing his original judgment favorably to the statement of the expert. The results, which are indicated in the three right columns of the upper chart, show an average of 48 per cent reversals of language judgments, with P. E. of 1.4; 47.8 per cent reversals of ethical judgments, with P. E. of 1.6; and 46.2 per cent reversals in musical judgments, with P. E. of .9.

If now we take as our unit of measurement the per cent recorded as the chance of reversal, we find, as indicated on the lower chart, that the probability of reversing favorably to the majority in matters of speech and morals is approximately five times chance; whereas in matters of musical feeling the probability is only about twice chance. By majority is meant here of course only the special type of majority provided in the experiment, but if generalization is permissible on the basis of the evidence available, we may venture the statement that a man is two and a half times as individualistic in his musical likes and dislikes as in his moral and linguistic preferences. Similarly we may conclude that expert and majority opinion hold about equal sway over the individual in morals and music, but that the chances are about ten to seven in favor of majority prestige in matters pertaining to speech. To take a concrete case, the struggle between the expert pronunciation which accents the word 'cantonment' on the first syllable, and the army pronunciation, which accents it on the second syllable, is likely to end with the doom of the expert, whereas classical music has at least an even chance in its struggle with the popularity of the jazz band.

The type of experiment here described can, on account of the great range of individual variations involved, be valid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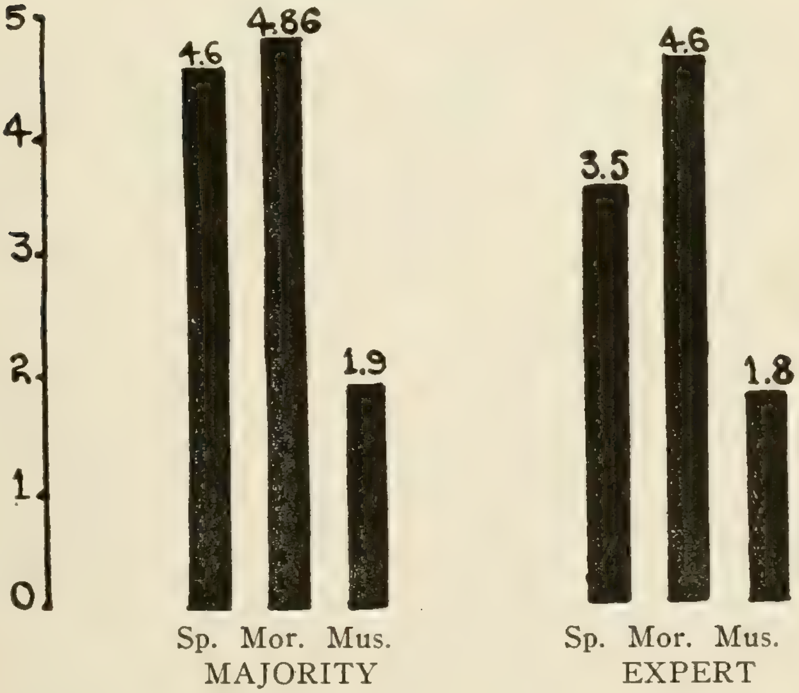
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MAJORITY
Sp. Mor. Mus.
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only if applied to a large number of cases; and inasmuch as each particular experiment measures only a very particular type of suggestibility, any generalization from a single experiment will always be questionable. But it is believed that an extension of the method to cover a large number of typical cases in which such social influences as personal prestige, fashion, orthodoxy, etc., play a part, would give material which would be valid for general purposes. Whether the shrunken prestige of a defeated political candidate or of an abdicated emperor follows any accurately describable laws, one could scarcely venture to say; but it is sufficiently obvious that until so-called social laws rest on more than the personal observations of individual writers, we shall have a great excess of laws, and only a minimum of confidence in applying them.[3]

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[3] Read at the Annual Meeting of the American Psychological Association, Cambridge, Mass., Dec. 31, 1919.

THE NUMBER FORMS OF A BLIND SUBJECT

By **Raymond H. Wheeler** and **Thomas D. Cutsforth**

Number forms in the adventitious blind are sufficiently rare to warrant the description of a case. The reagent whose forms are here described is T. D. C., one of the authors of this article and a former major student in psychology at the University of Oregon. On March 9, 1917, detailed descriptions, in writing, of these forms were obtained and filed away. On August 15, 1919 the reagent was unexpectedly asked to describe these forms again. These latter descriptions were then compared in minute detail with the earlier descriptions.

The reagent has a simple digit form for the numbers 1—10, a tens form for the numbers 10—100, a hundreds form for the numbers 100—1,000, a thousands form and so on into the millions. He also has a week form, an alphabet form, a month form and a century or date form. He also has numerous varieties of synaesthesia.

The lower right hand figure in the accompanying cut is the reagent's form for the numbers 1—10. Each rectangular section is sharply differentiated from its neighbor by means of hue and brightness qualities which stand for separate numbers. The numbers themselves are never visualized and when the subject uses this form he translates the colors into numbers vocal-motor fashion. The form appears in space in front of the reagent's face at a distance of about one meter. The entire form is about fourteen centimeters long and two centimeters in width. The following are the colors for the numbers 1—9, taken verbatim from introspections.

	On March 9, 1917	On August 15, 1919
1.	white	whitish, nothing but brightness
2.	dull grey, tinged with yellow, like old weather beaten boards	grey, slightly yellow, like old weathered straw
3.	like 2 but more reddish	light, faint reddish brown
4.	like 3 but darker	peculiar brown, like fir boards

5.	muddy black	a dull black, like spilled ink
6.	white, like 1, identified by position	white, like ivory
7.	dark, muddy blue	bluish black, somewhat like graphite, but darker
8.	grey, tinged with yellow	very poorly saturated yellow
9.	dull grey tinged with yellowish green	dark grey with greenish cast; a bottle green

The tens form consists of ten of the small digits forms placed end to end in a direction upward and away from the reagent toward the right, beginning at a point in front of his left shoulder. Each of the smaller unit forms represents figures from 1—9, 10—19, 20—29 etc. The extreme right end of the form bends more sharply away from the subject and upward less abruptly enabling him to visualize the entire form almost at once and to make use of it with less extended eye-movements. With the exception of the numbers from 10—19 each intermediate figure in the various ranges of tens is colored according to the final digit.

The hundreds form is a product of tens forms. The colors are determined in similar fashion. The thousands form is a product of hundreds forms and so on. In all of these latter forms the colors represent ranges and not individual numbers. For example in the hundreds form there are ten colors, representing the ten ranges of ten numbers each. In the thousands form there are ten colors representing ten ranges each of 100. When the reagent thinks of large numbers several forms are used and held in consciousness at the same time, the smallest form in the foreground and the larger forms localized back of the first one in order of their size. This can best be understood by reference to an analogy. Let each form be represented by a framed picture. The several forms in use at one time would then be represented by a series of framed pictures set up one behind the other, each one larger than the one in front of it and resting on a higher level. The individual forms retain their identity by their localization and by a colored halo which latter varies with each form. The halo would be represented by the frame of the picture. The halo or background for the hundreds form is black; for the thousands form it is silvery white; for the hundreds-thousands form it is reddish-brown and for the millions form it is yellow.

The following illustrates how the forms are used. When the reagent thinks of the number 3,591 a thousands form appears in the background of a visualized field, with the appropriate section in the focus of attention, namely, the third section from the left hand end. Directly in front of this lies the hundreds form and with a " slide rule " effect the reagent's line of regard shifts to the fifth section of this latter form. A second shift of his line of regard brings the reagent's attention to the ninth section of the tens form which latter lies slightly below and in front of the hundreds form. A final shift carries his regard to the first section of the digits form which lies at the left hand end of the tens form, when thinking of large figures. The rapid use of number forms has been described several times in the literature. Diamandi's success as a lightning calculator was in part due to the use of a number form.[1] In the present case this fact is all the more remarkable for the reason that the numbers are not visualized. Colors are used as substitutes. Hennig,[2] Grüber,[3] and others have pointed out that in cases of synaesthesia colors have been used to represent numbers in performing mathematical operations. Galton[4] seemed to have been able to substitute odors for mathematical symbols in simple operations of adding and subtracting.

As an example of such a substitution of colors for numbers we give the following instance of counting in terms of colors. The authors were giving mental tests at the Oregon State School for the Blind. The reagent was in the act of counting the number of words which one of the blind pupils was giving within the interval of three minutes. He began by counting the words in vocal-motor fashion but in a short time vocal-motor imagery became confused and he was obliged to cease relying upon inner speech. In the meantime, however, each verbal image had been accompanied by its appropriate color, localized properly in the number forms. From this point on until the end of the three minutes the reagent, who was then acting as examiner, was able to keep track of the number of words which the pupil was reciting to him. The last color was then translated into the appropriate figure.

When the reagent is given large numbers to locate in these forms, eye-movements can be seen as he shifts his line of regard from one form to another or from one part of the same form to another part. The extent of eye-movement is proportional to the size of the forms which are being used. Voluntary or artificial checking of these eye-movements impedes the reagent in the use of the forms.

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[1] See Philips, Genesis of Number Forms, *Amer. Jour. of Psychol.*, Vol. 8, 1897, 506-527.

[2] Enstehung und Bedeutung der Synopsien, *Zsch. f. Psychol.*, 1896, Vol. 10, 113-122.

[3] L'audition colorée, Cong. inter. Psychol., Paris, 1889.

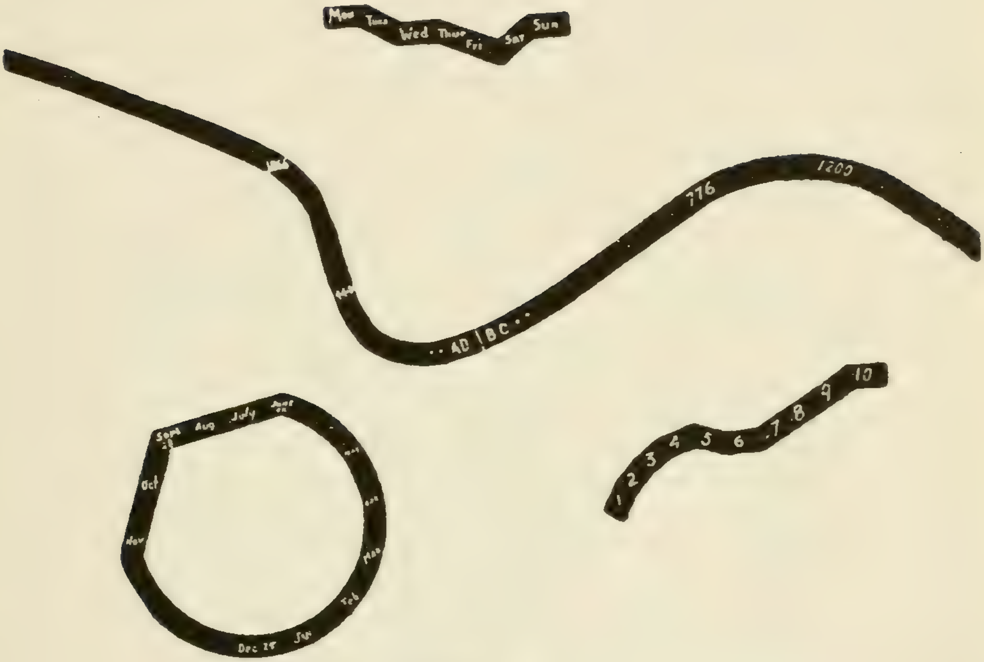
[4] Arithmetic by Smell, *Psychol. Rev.*, 1894, Vol. 1, 61-62.

The upper figure of the cut is a miniature copy of the reagent's week form. Each day of the week is represented by a colored section in the form. Monday is dark, inky blue; Tuesday is pale brown; Wednesday is a very dark green; Thursday is a dark orange-red; Friday is a "russet" yellow; Saturday is a muddy, curdled black and Sunday is white.

The lower left hand figure is his month form. Again no letters or writing are seen in the form but the months are represented in colors. The flattened portion of the form corresponds to vacation periods of the school year. The subject is unable to recall the conditions under which the form originated.

The long figure in the cut represents the century or date form. It is about four centimeters wide and a meter long. It is visualized upon a globe-map of the world and upon this background appear dark, faintly colored areas as continents and light blue areas as oceans. The dates appear in color, not in figures or in writing. 1900 lies above the eastern coast of the United States; 1066 A. D. lies just above the British Isles; the dividing point between A. D. and B. C. in the region of the Mediterranean Sea and so on. The dates are colored according to their component digits. This form originated when the subject was first studying geography, and is used constantly as an aid in fixing and in recalling dates.

The figures in the cut are photographs of card-board models made by the reagent himself. In the actual forms there are no figures, the figures being inserted merely to explain the forms. The photograph was taken in March, 1917. In August 1919 the reagent made new models of all forms. During this interval of time none of these forms had changed except in minor degrees of saturation or in slants of some of the lines, and these differences may have been due to inaccuracies in making the model or in describing the colors. One exception must be noted, however. The century form originally slanted upward at its extreme right end. In the cut it is seen slanting downward. This change occurred during the time that the reagent was in college, and can be traced to the repeated use of this form while the reagent was studying Babylonian History. Since then the form has resumed its original upward slant.



THE NUMBER FORMS OF A BLIND SUBJECT

SOME PROBLEMS IN REGARD TO ALIMENTARY SENSITIVITY

By Ivy G. Campbell

The observations around which the discussion in this article are centred were made under rather unfavorable experimental conditions and are offered not as conclusive evidence on disputed points but rather as suggesting problems and technique for future investigations.

The writer after several months of "stomach trouble" was put in March 1919 under the care of Dr. Einhorn of N. Y. City, whose very ingenious method of treatment gave her opportunity for the following study. The stomach was given a complete rest for two weeks during which time duodenal feedings were given. This was accomplished by direct feeding into the duodenum by means of a tube which passed from the mouth into the duodenum. This tube was not withdrawn during the two weeks period, but every day—from 6 a. m. to 8 p. m.—at two-hour intervals, from 240-300 c. c. of food (milk, raw egg, sugar of milk, and, toward the end of the treatment, butter) were forced through it into the duodenum. The apparatus which permitted this treatment is described by Dr. Einhorn as follows: "The duodenal pump consists of a small metal capsule (14 mm. long and 23 mm. in circum.) which is perforated and can be unscrewed. This communicates with a long, thin rubber tube (8 mm. in circum. and one metre long), and is marked at 40 (I. cardia), 56 (II. pylorus), 70 (III), and 80 cm. distance from the capsule. At its end is a tip, to which a syringe can be attached." (6:86)

The greatest part of this paper will be devoted to a discussion of hunger, but some observations on and interpretations of appetite, fulness and emptiness, thermal sensitivity, will be given.

Hunger.—The experimental findings of Cannon and Washburn, later verified and extended by Carlson, Boring, and others, have convinced most readers that hunger is a sensation or sensational complex concomitant with the periodic, intermittent contractions of the empty or nearly empty stomach. Carlson holds that each separate contraction is synchronous with a hunger pang, and that the intensity and duration of the hunger correspond pretty closely with the strength and the duration of the stomach contractions. He also found that his subjects locate the sensation of hunger in the stomach. His data obtained from observations on fasting subjects is of special interest in connection with the observations reported in this paper.

In the case where Carlson and one of his assistants underwent a five day starvation period they found that there was an increase rather than a decrease of the gastric tonus and the hunger contractions during the period (4:128); that the sensation of hunger was almost continuous after the first day of starvation, i.e. it did not wholly disappear during the intervals between the vigorous gastric contractions; and that during the first three days the hunger was greater than during the last two although the contractions were greater in the last two days than on the first three. (4:135) They also found that appetite was increased during the first few days and then decreased so that toward the last there seemed to be indifference toward food "despite the persistent hunger call of the stomach." (4:136) The discrepancy in the parallelism between the intensity of the gastric hunger contractions and the intensity of the subjective hunger sensations was due, Carlson is inclined to believe, to the depression of the Central Nervous System. (4:136) In the case of the man who under Carlson's observation completely fasted for fifteen days and for the subsequent eight days except for the daily injection of a quantity of cotton fibre it was found that the hunger contractions were continuous with practically normal rhythm and intensity but that the subjective sensations appeared to be somewhat weakened, tinged with an element of general epigastric distress or sick stomach. The appetite sense or desire for food was modified or obscured by a tendency to a persistent bad taste in the mouth, yet the dominant element in consciousness was reported to be the thought of food and eating. (5) In the treatment given the present writer the stomach was kept empty—except for one half-wine-glass dose of liquid medicine (bismuth and magnesium) given three times a day—for a period of fourteen days. Despite this continued emptiness, hunger—except in the few cases noted below—was not felt. This result seems at variance with those of Carlson and considered in connection with the manner of her treatment gives rise to a number of questions: (1) Are stomach contractions inhibited by duodenal feedings; (2) If not, is it then true that stomach contractions are the most important concomitant physiological factor in hunger; (3) Is it possible, as has been suggested, that intestinal contractions play a large, if not the principal part, in hunger and that duodenal feedings inhibit these but do not inhibit the stomach contractions?

Carlson incidentally raises some of these questions and gives some experimental evidence that helps to answer them. He believes that intestinal contractions do take place during hunger, but that they play a very small part, if any, in the sensation of hunger. The proof of such a part is still wanting (4:83). In regard to question (1) above, Carlson describes some experiments made upon dogs which show that, in this case at least, stomach contractions are reflexly inhibited by stimulation of the intestinal mucosa. Intestinal and gastric fistulas were made upon tweny-four young female dogs and records of the effects of intestinal stimulation upon stomach contractions were made. It was found that gastric juice, chyme, acids, alkalies, water, milk, oil, introduced in 10 c. c. amounts into the small intestine inhibited gastric hunger contractions and gastric tonus for varying periods. "The longest inhibition obtained in any one experiment was produced by 10 c. c. of milk in the gut. In this case the inhibition lasted thirty minutes." (4:198)

The writer of this paper has no objective experimental evidence that might help in the answering of the questions given above but believes that such evidence is necessary in order to answer them and makes the following suggestions for further investigations. (1) That by the devising of some apparatus—possibly an adaptation of the apparatus used in her treatment—the presence of and the interval of periodicity of duodenal contractions be obtained;[1] (2) that stomach contractions be recorded during a period of duodenal feedings; (3) if it proves that periodic duodenal contractions are existent that the effect upon these be determined (a) during stomach feedings, (b) during duodenal feedings, (c) during a period of starvation.

It may well be that the duodenal feedings given the writer were frequent enough to cause a continuous inhibition of the stomach contractions and that for this reason during the greater period of the treatment she did not feel hunger. If this does explain her lack of hunger it would be interesting to determine experimentally how long the interval between duodenal feedings could be made and still have hunger absent. Whether or not the intestines play a rôle in the hunger sensation the writer cannot determine from any objective experimental evidence which she has, but that they do play such a rôle is suggested by certain introspections which she made during her period of treatment. As will be shown later in the paper, her introspective study of the sense of " fulness " convinced her that this was located in the small intestine and it was to this same location that she at times attributed " hunger." During the first week of treatment the duodenal feedings were begun at six o'clock in the morning. This generally necessitated waking her and as the feedings were begun immediately, time for introspection was lacking. On the last eight mornings of the treatment feedings were not begun until eight o'clock and on six, possibly seven,[2] of these mornings " hunger " was felt before the duodenal feeding was given.

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[1] Carlson (4:82) mentions that in the duodenal fistula case of Busch contractions of the small intestines were noted in hunger.

Her introspections of this first morning, March 29, when she reported this early morning hunger contain the following statements: " Getting hungry: localized where the ' full ' feeling is felt." On one other occasion, March 25, she wrote: " 11:30. Feel a little ' hungry:' localized in the duodenum(?)." On March 28, 4 P. M. she wrote: " Before the feeding, felt fulness in the colon, also some sensation (pressure?, slight hunger?) localized in the lower stomach." On March 28, 4 P. M. she wrote: " Slight hunger: localization ' pit ' of stomach." Later when she had more definitely localized the sensations in the intestine she felt that " lower stomach " and " pit " had really signified intestine. These introspections of the writer indicate that hunger may be felt at least partly in the small intestine. Perhaps it should be mentioned that at the time when the treatment was being given the previous experimental findings in regard to hunger, etc. were but vaguely known to the writer. The problem which she set herself at the time was to observe *when* she felt hunger, not *where* she felt it. For this reason she is inclined to regard her spontaneous though carefully made observations as of considerable value. During the period of the last three days hunger was reported as having been present at three times other than early in the morning but the exact time was not recorded. Her introspective notes show no localization of hunger except as quoted above. As stated before, localization was not her problem. Altogether then during her two weeks of treatment hunger was felt but twelve times. It may well be that hunger was present at a few other times for strict attention to the problem was not always possible. Perhaps this is true of all hunger reports. At any rate the writer feels that her observations are on the whole truly representative.

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[2] The writer did not positively identify the sensation of this morning as that of hunger.

Cannon, Washburn, Carlson, all make a good deal of the *time* relations between hunger and stomach contractions. They find that hunger is always reported *after* the beginning of stomach contractions. We have stated above that hunger was felt in the early morning before the duodenal feedings were begun and if it is true that duodenal feedings inhibit stomach contractions then this early morning hunger may have been due to the fact that the stomach contractions had not yet been inhibited because no feeding had been given during the night. But the fact that hunger was felt at other times during the day despite the fact that the time of duodenal feedings was kept constant is not so easily explained by this theory. To the writer's mind it raises the question whether or not after all the factor of " general weakness " is not a part of the hunger complex.

Hurst (7) quotes with approval Tiedemann's " admirable analysis of the sensation of hunger " in which hunger is said to be a sensation of emptiness of the stomach and the general sensation of malaise and weakness of the body as a whole. That hunger is more than local emptiness Hurst argues from the fact that in cases of prolonged starvation food relieves the emptiness but not the general feeling of malaise,

> " a much larger quantity of food being required to satisfy hunger of the starved tissues than that of the empty stomach. For the former digestible and nutritious food is required, for the latter bulk is of primary importance. This is well seen in patients with a fistula of the upper part of the small intestine . . . I have myself observed one. The patient was constantly hungry, although he ate enormous quantities of food. His stomach always felt full, but the general sensation of hunger remained, as most of the food escaped from the fistula with the result that the tissues continued to be starved." (7: 40-41.)

This general sensation of hunger Hurst thinks to be dependent upon the requirements of the tissues,

> " it is increased when these are excessive owing to abnormally active metabolism, as in diabetes and some cases of Graves' disease." (7: 40.)

The sensation of emptiness experienced during a period of starvation Hurst believes to be partly localized in the intestines. The cause of this sensation of emptiness is best explained by motor activity.

> "The sensation of emptiness in the intestines, like the corresponding sensation in the stomach, can best be explained as a result of periodical motor activity in an organ which is abnormally excitable as a result of the absence of normal stimuli for an unusually long period." (7: 41.)

Carlson and Boring do not agree that weakness is a part of hunger. Both weakness and emptiness are accessories of hunger, according to Carlson. Emptiness he says is

> "a peculiar feeling in the entire abdominal region. This feeling is continuous, not intermittent like the pangs of hunger. . . . It is probable that the increased tonus of the abdominal muscles in consequence of the empty state of the stomach and the intestines, contributes to the feeling in some way. . . . We question whether any part of the feeling of emptiness originates in the stomach itself. It is not so difficult to understand how a hypertonic and rhythmically contracting empty stomach may give rise to the sensations of tension, pressure, and gnawing pain. But how can it cause the sensation of emptiness, unless this feeling is merely negative of the sensation of fulness? On the other hand, if the tonus of the abdominal muscle does not suffice to maintain the normal intra-abdominal pressure when the stomach and the intestines are relatively empty and strongly contracted in hunger, the tension on all the visceral organs would not be diminished and this in turn would alter the pressure relative to the peritoneum and mesentery. If this is the factor in the origin of the feeling of abdominal 'emptiness' in hunger, the sensation should be diminished in man by lying down, in comparison with that felt when standing." (4: 93-94.)

The writer of this paper has certainly always considered the consciousness of emptiness as a part of her hunger complex. As to whether or not this "emptiness" decreases upon lying down she cannot say. But in her case at least it was experienced while lying down.

As to Carlson's positive analysis of hunger, he writes: "It is more or less an uncomfortable feeling of tension, or pressure and pain referred to the region of the stomach." (4:6) He quotes with approval the following analysis of hunger given by Boring.

> "Upon a background of dull pressure, which is sometimes recognized definitely as kinaesthesis or the equivalent muscular pressure, there is set a dull ache or gnawing pain which characterizes the hunger. Both pain and pressure are referred to the region of the stomach. The pain is noted as fluctuating, as rhythmical, as unstable." (4:25-26.)

Kinaesthesis, pressure, pain, seem then to be the components of hunger for both Carlson and Boring. Perhaps what the writer of this paper called "hunger" in the cases noted was not what these two would call hunger. Certainly she included in it what she calls emptiness. Also at times there was in it or perhaps one should say *with* it a consciousness of weakness. In this connection the following note from her introspections is interesting. "April, 1. Up a little while today. Find myself very weak. Weighed, and found had lost seven pounds up to date since beginning treatment. Felt some 'part' of what have called hunger in the past: but hunger of the kind when I have felt sick. Rather bad, weak feeling; while the hunger in the morning is more localized. Is a gnawing sensation. This is a good feeling: feel fine when have it." This introspection indicates to the writer that the consciousness of weakness can be separated in introspection from the other sensations. Perhaps "emptiness" too could be so separated. But that the sensations located in the stomach should be separated out from the complex and be called "hunger" while those in the intestines should be called "emptiness" seems hardly justifiable. Probably all form a part of hunger. To the writer's mind hunger is a perception. Moreover she finds that in her everyday life what functions as "hunger" contains the sense of "emptiness" more often than it does pain. May it not be that this is a question of individual differences? The writer is reminded of Boring's analysis of appetite in which he points out that the stomach mucosa sensations which Carlson gives as a necessary part of appetite seem to be an individual affair. He writes: "In interpreting this pattern as an essential component of appetite, Carlson and Braafladt presumably put upon it their own individual meanings, which are, apparently, not completely in accord with those of other observers." (3:453) May not the same thing be true of hunger? Boring would probably dissent to this, insisting that the difference in the two cases is that in hunger we are dealing with a fusion of sensations while in appetite we have an attitude or meaning. If this is true then in the former case all normal persons would have the same components fused, while in the latter the sensory processes correlated with the attitude might vary in different individuals. The writer is raising the question whether it may not be true that hunger too is an "attitude" rather than a fusion? She certainly believes that the meaning of hunger is a constant part of hunger and she raises the question whether or not the sensory components (if one can make this distinction between meaning and sensory components) may not vary in individuals? Had she a record of her stomach contractions during her treatment she might conclude that only during stomach contractions did she feel hunger. But may it not also be true that without this sense of emptiness she would not experience hunger? Certainly she always did sense emptiness when she was hungry. And it is true that she experiences what she calls hunger when pain is absent. Carlson and Boring report hunger without emptiness but always have pain in hunger. May it not be that in all of these cases hunger is present but with different sensory components? Or perhaps certain sensory components (perhaps kinaesthetic) are always present in hunger while other components vary in different individuals?

As to the consciousness of weakness the writer is inclined to agree with Carlson that this is not necessary in hunger. In her own experience, as her introspections show, hunger, at times, seemed to be accompanied by a consciousness of buoyancy and strength while at other times by that of weakness. This is also true of the hunger felt in the writer's every day life. On the other hand her introspections during treatment show that hunger was felt after exertion more than at other times.

Before leaving the discussion of hunger it is interesting to add part of a conversation with Dr. Einhorn, who feels that the need of food plays a part in the hunger process and points out the fact that in over 500 cases in which he has given the duodenal feeding treatment in very few instances is hunger reported to him. This is not true of rectal feedings where assimilation of food is much less than in the case of duodenal feeding. It may be, of course, that duodenal but not rectal feedings inhibit stomach contractions and hence the lack of hunger in the first case and the presence of it in the second case is accounted for. This is one of the points which future investigation will probably answer.

Appetite.—As to appetite during the treatment never did this force itself upon consciousness but came only when sight, smell, or the thought of food provoked some desire for it. This never happened however after a duodenal feeding, such a feeding seeming to "satisfy" the appetite. When appetite *was* felt there was decided "mouth-consciousness," i.e. kinaesthetic images of tongue movements, pressure images of "pucker," what seemed to be sensation from increased salivation. At times when the food was seen there was a motor tendency toward it. There seemed to be no gastric component present. Sometimes immediately before a duodenal feeding was given the writer watched food served and "desire" for food was often felt. In no case, however, did this *arouse* hunger. Introspections for 7-7.30, March 29 read: "Getting hungry: watched trays served, great desire for food tastes. 'Coffee-ah!' 'Brown rolls!' (these in vocomotor imagery)." With these images went the accompany kinaesthetic and pressure images and sensations in the mouth. From her experience the writer believes that hunger and appetite are separated conscious states or attitudes. There may be mutual influence in a number of ways.

Sensation of Fulness.—One of the most outstanding features of the treatment was what the writer called the sensation of fulness. When the feedings were first given a rather widespread reaction occurred. Warmth (a sort of flushing) was felt over the whole body, most marked in the face and limbs. A thrilly, tingling feeling in arms and legs was rather dominant. This went up and down the limbs much as a shiver runs up one's back, but in this case it was a warm running thrill not a cold sensation, as in the case of shiver. A feeling of weakness was present and toward the end of the feeding a sensation of fulness. It is hard to describe this sensation for although it has some of the sensory components of what we sometimes term "bloating" or consciousness of gas in the intestines it had more localized muscular sensation than this. It seemed to be localized in one place, high up in the abdomen. Fulness seems the only word to express the sensation. It was somewhat like the sensation occasioned when upon washing out the stomach one feels that as much water as one "can hold" has been poured in and that it must be taken out. In both cases, i.e. in both the stomach and the intestine, fulness may pass over into nausea. In the case of the duodenal feedings there was a consciousness of pushing against the stomach, at least it seemed to be against the stomach for it was against that part where nausea was being felt. Nausea has always seemed to the writer to be localized in the stomach. At least it seemed higher up than the sensation of fulness, when they were present at the same time. Toward the end of the period of treatment the sensations of warmth, weakness, thrilling were not present in any marked degree. The sensation of fulness was present in all feedings and never was the writer able to take the feedings while sitting up. This seemed to increase the sensation of "fulness" and gave an added dragging-down sensation. It also occasioned the nausea more easily.

From experiments Hurst concludes (7:35) that the "sensation of fulness" is due to the stretching of the muscular coat and is probably shared by all hollow viscera. Carlson (4:112) points out that there must be a certain amount of tonus reaction of the stomach before tension or pressure on the walls of the stomach will produce the sensation of fulness. The fact that the writer of this paper did not feel this sensation of fulness until the end of each feeding and that the sensation was felt more easily and markedly when the food was forced in more rapidly lends support to Hurst's contention, although one wonders if the food stays in one place long enough to give rise to muscular distention. At any rate the sensation of fulness *was* present under the conditions mentioned and apparently the small intestine is sensitive to distention. It may be mentioned that although qualitatively the sensation of fulness in the intestine resembled the sensation of " fulness " felt in the stomach after a very large meal, it was not felt in the same place.

One factor that made at least relative localization of sensations fairly easy and certain, was that the writer was experiencing fairly constant pain in the pyloric region of her stomach—due, probably, as tests revealed, to inflammation in this region. Both the sensation of fulness and hunger (at times) were localized lower than this pain. It should be stated that the writer's stomach is considerably dilated and that for this reason the localization of her sensations may be wrong. Even so the relative positions would be correct.

Thermal Sensitivity.—A few rough observations were made upon thermal sensitivity. Of course the tube used in the treatment was very small and single-walled thus allowing heat or cold to be quickly conducted to the surface. But as localization of alimentary sensations has been found to be pretty accurate this fact need not invalidate her observations. The writer believes that during her two weeks of treatment she became pretty competent and accurate in her ability to localize a sensation as one coming from the different parts of the alimentary canal. The strict localization of her pain and of her sense of fulness and hunger gave her what might be termed " land-marks." And then again introspection is not a new business to her.

The problem of the thermal sensitivity of the stomach seems to stand somewhat as follows. Hurst found that water from 40-50 degrees C. and also ice-water introduced into the stomach by means of an india rubber tube placed inside an ordinary stomach tube gave no thermal sensations (7:5). Boring, also using a double-walled tube, found that water at from 50-80 degrees C. produced a sensation of warmth and water at from 0-30 degrees C. a sensation of cold, both being referred to the stomach. (1:40) Carlson too concludes from experiments that " The stomach mucosa is endowed with heat and cold nerve-endings." (4:111)

From her own experience the writer believes the oesophagus, the stomach and the duodenum to be sensitive to thermal stimulation, the latter two being sensitive only when the stimulus is rather extreme, and the duodenum possibly not sensitive to warmth. In most cases the sensation of warmth occasioned by her feedings was confined to the upper part of the oesophagus. On March 28 attention was turned for the first time to this problem and introspections of that day read: " During several of today's feedings gave strict attention to temperature sensations: could sense none although when I forced the food down more rapidly it felt a little different." On March 29 introspections read: " No temperature sensations on forcing it faster." At other times when the food was given hotter than usual a vague, undefined sensation was felt in the stomach and duodenum. This was not recognized as warmth.

On the day that the tube was to be withdrawn Dr. Einhorn kindly consented to allow the writer to perform a few experiments upon thermal sensitivity. These consisted in having an experimenter[3] very forcibly inject a syringe full of water at varying temperatures through the tube into the duodenum. The writer opening wide her throat, held the tube away from the mouth and throat surfaces, closed her eyes so as not to know what stimulus was being given, and observed and reported the sensations experienced. One syringe full of water at each of the following temperatures was used with the following results. At 40° and at 44° Fa. unmistakable cold was sensed. This was very widespread in the abdomen and according to the writer's best introspective ability was localized both in the stomach and in the duodenum. At 54° the cold did not seem so widespread and it was doubtful whether or not it was at all localized in the duodenum. At 104°, 106°, 108°, 112° Fa. warmth was felt in the stomach but it was not as widespread as the cold and the writer felt rather doubtful about its being in the duodenum. At 115° it felt considerably " hotter," more widespread, and was possibly sensed in the duodenum, although the writer was not sure of this last point.

The writer offers these few observations on thermal sensitivity not as conclusive evidence in any sense but merely as having suggestive value. The experiments, together with her observations during the whole treatment, do incline her to the view that the stomach certainly and the duodenum probably have thermal sensitivity. Both seem more sensitive to cold: although higher degrees of warmth might have given different results. The whole problem should be put to further experimentation.

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[3] The writer's brother, Dr. Malcolm S. Campbell, kindly performed these experiments.

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PLEASANTNESS AND UNPLEASANTNESS IN RELATION TO ORGANIC RESPONSE

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In a previous study[1] of mixed feelings we came to the conclusion that pleasantness and unpleasantness are not felt simultaneously but only in alternation. This incompatibility indicates the existence of two opposed physiological mechanisms which are involved in affective response.

We have thought that a comparative study of the organic-kinaesthetic processes of pleasant and unpleasant feeling might throw light upon the nature of affective opposition. Accordingly we have taken for an experimental problem the following: is there any characteristic difference between the organic-kinaesthetic factors of P and U feeling which may throw light on the physiological mechanism of affection?

We have evoked feelings by simple, single stimuli: odors, tastes, tactual impressions, and a few chords. The feelings aroused vary intensively between the mild aesthetic feelings and strong emotion. Following is a complete list of the stimuli used:

Olfactory: wintergreen; caproic acid; creosote; castor oil; white rose perfume; asafœtida; cod liver oil; nitro-benzole; peppermint odor; camphor; anise; violet perfume; heliotrope perfume. *Gustatory:* vinegar; castor oil; chocolate peppermint candy; sarsaparilla; salted almond. *Tactual:* stroke with velvet; tickle ear; slap with lamella; sandpaper chin and nose; snap on cheek with rubber band; pain from sharp nail; extension of arm for 60 sec. *Auditory:* chords and discords.

The subject was seated in a Morris chair, eyes closed, in a room specially prepared for the experiment. Every S worked two hours a week, at approximately the same time of day; a single session took from ½ to ¾ of an hour. The work was done during the winter of 1919-20, and was broken by the Christmas holidays.

The following instruction was finally adopted after several slight modifications:

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[1] Young, P. T., An Experimental Study of Mixed Feelings, *Am. Jour. of Psychol.,* 1918-19, 237-271.

"In this experiment be passive and receptive. Let the experimental situation have its full normal effect upon you.

"Report all muscular tendencies and organic sensations in any way related to the affective reaction.

"Report whether the experience was pleasant, unpleasant, or indifferent; and indicate the intensity of the feeling (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong')."

Our subjects were five men and two women, all connected with the psychology department at the University of Minnesota.[2] We shall designate these subjects by letter: A, B, C, D, E, F, G.

TABLE I

TOTAL NUMBER OF REPORTS

Subject	A	B	C	D	E	F	G	Total
Pleasantness....	25	17	12	6	27	26	9	122
Indifference*....	19	6	12	2	14	4	13	70
Unpleasantness..	34	29	8	8	41	17	11	148
Total.........	78	52	32	16	82	47	33	340

*"Indifference" includes all reports in which pleasantness and unpleasantness are not specifically mentioned, as well as those in which the experience is described as indifferent.

RESULTS

The total number of reports in the experiment is shown in Table I. It will be noticed that there is a good balance between P (122) and U (148).

Of the specific organic-kinaesthetic processes reported by our subjects the most frequent are:

Muscular strain, tension, shivers, shocks, jumps, moving waves of sensation; relaxation; incidental movements such as chewing, swallowing, tongue movements, jaw movements, eye movements, special associated movements, etc.; warmths, colds, pressures referred to the chest, heart and trunk; observed changes in respiration and circulation; perception of passive movement, of the flow of saliva, of tendency to vocalize; kinaesthesis of amusement, smiling, frowning; anger, expectation, surprise, nausea, sneezing, snarling, etc.

Correlations.—A preliminary survey of our data shows clearly that P and U are frequently reported without any mention whatsoever of organic and kinaesthetic accompaniments. Of course there may be subliminal physiological changes accompanying every P and U feeling which, conceivably, might be detected by an ultra-refined expressive method. However, peripheral changes were not observed by our subjects in a large number (38%) of reports. We conclude that there is no sensory *sine qua non* of P and U.

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[2] The subjects were: Dr. R. M. Elliott; Dr. (Miss) M. Fernald; Dr. (Mrs.) J. C. Foster; Dr. W. S. Foster; Dr. J. J. B. Morgan; Mr. O. P. Pearson; Mr. C. P. Stone.

Consequently we must limit our investigation to reports containing specific mention of organic-kinaesthetic processes. Apparently there are a few specific correlations generally recognized in daily life. Smiling, for example, appears to be correlated with P; frowning and snarling with U; nausea (3 cases) with U, etc. There is also a suggestion of correlations between P and U and changes in respiration. However, no conclusions can be drawn safely from our data, and the correlations in question are best studied by the physiological methods.

If, however, we examine the reports more closely, we find a general correlation-tendency of considerable interest. Muscular strain (tension) is correlated with U, while its opposite, relaxation, is correlated with P. In 28 reports strain is associated with U feeling, and in 31 muscular relaxation is associated with P. The distribution of these reports is shown in Table II. This result confirms previous work.[3]

Following are sample reports in which muscular strain (tension) is associated with U:

TABLE II

MUSCULAR STRAIN AND RELAXATION IN RELATION TO PLEASANTNESS AND UNPLEASANTNESS

Subject	A	B	C	D	E	F	G	Totals
Strain with Unpleasantness	7	8	0	5	0	7	1	28
Strain with pleasantness	0	2	0	0	0	1	0	3
Relaxation with Pleasantness	6	0	7	3	3	12	0	31
Relaxation with Unpleasantness	0	0	0	0	0	0	0	0
Total Reports	78	52	32	16	82	47	33	340

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[3] Hayes, S. P., A Study of the Affective Qualities, *Am. Jour. of Psychol.,* Vol. 17, 1906, 358-393. "For all observers alike, . . . judgments of strain were easy and, on the whole, direct. Strain was, however, described in 'muscular' terms throughout, and increasing strain meant, uniformly, increasing unpleasantness." Three of Hayes' subjects make relaxation the opposite of unpleasant strain and the curves are curves of pleasantness; one other subject considers relaxation a pleasurable muscular attitude or a pleasurable organic set.

"U. mildly so . . . I noticed that my hands were crossed quite tightly with a tension that increased slightly." A 29. "That was distinctly U. There was quite a foreperiod of apprehension—a straining." A 41. "It was U, meaning that there was some tension in the muscles around the nose, possibly tending to curl it up and withdraw it." A 65. "That is U but one's main interest is in balancing the strains all over the body." A 67. "That was U, rather unexpectedly so . . . Quite a degree of tension as compared with the muscle position assumed for P and relaxing stimuli." A 71. "There was the usual degree of tension that characterizes the foreperiod of expecting U as I did then. The tension there seemed to be lateral where the tension for the U odor seemed to be vertical." A 72. "That was U. I didn't notice any particular kinaesthesis except a slight tightening of the muscles of the lower part of the nose and the upper part of the lip." B 13. "Decidedly U . . . Tightening up of the eyes—a tense closing of them." B 23. "It was U. Quite a noticeable tightening of the muscles in the lower part of the face. It seemed to be the reverse of sniffing—the attempt to prevent air from coming in." B 25. "U . . . Only a very slight tendency to tighten the muscles about the nostrils." B 27. "I should say U, but very weak . . . Tightening of the muscles about the trunk." B 40. "U, strong. Tendency to shut off the breathing. Tendency to draw head away which was inhibited by the experimental situation. Tightening of muscles about the nose. Drawing together of muscles between the brows—a slight scowl." B 42. "Very faintly U. I didn't notice anything more than a slight tensing of the muscles concerned with smelling." B 46. "I should say very weakly U. I think there was some tenseness of the muscles about the nose and thorax." B 47. "That was U, rather moderately . . . First a little excitement, a little tension of the muscles." F 2. "A sudden tensing of the muscles as soon as you struck my cheek . . . It was U, though not so very U either, mostly surprise and tensing of the muscles in the right arm and also jerking away from where you struck." F 4. "Muscles had a tendency to contract. Face frowned . . . Very U." F 24. "A tendency to make muscles more tense. It was indifferent, or if anything weakly U." F 32. "Definite tensing of the muscles, pulling away to the side, turning of the head . . . Very U" F 44. "Quite a definite tensing of the muscles especially in the upper part of the body in the arms. Tendency to lift right arm and brush it away. Feeling was definitely U. Also during rubbing there was a tensing of the stomach muscles." F 49. "The feeling was U, very intense . . . A tendency to stiffen the muscles of the arm. That was a very slight feeling of stiffening of the arm—more like just tensing the arm." G 10. "At first there was tension and trifling surprise. During that time the feeling tone would be impossible to name; neither P nor U; exciting if you want to call it so; tensing if that is a feeling tone. Then the U which increased rather up to a maximum and then decreased a little. The U itself I can't say much about . . . A tension through my face." D 2. "That was U and exciting and perhaps somewhat tensing. The tension and excitement began before the first 'now' . . . The tension and excitement so far as I could tell were simply muscular feels in the cheek. The excitement was merely little contractions and quivers and that sort of thing. The U was many strains in a tendency to shake your head and get away from it—a frowning and possible tension through the body in the legs and arms as if to help you bear it." D 5. "U, moderately strong . . . Something of the same kind of a shiver you get with the first reaction to cold. At the same time there was a general increase in the kinaesthetic tension, particularly noticeable in my chest, in my face and in my arms. In my chest there was kinaesthetic holding of my breath for a bit after a rather sharp inspiration. In my face there was kinaesthesis frowning and tensing all my face muscles. In my arm simply a special tension . . ." D 10. "U. The reaction was a kinaesthetic jump of my whole body. A sudden tension and then it was all over. I should say there was tension in my legs and tension in my chest and face strongest. There was frowning. Perhaps I should also add that there is a pretty strong tension in my neck is if to draw my head away." D 12. "Mildly U. General tension through chest and in face at first. The tension kept up in my face although I kept up breathing naturally . . . I think the general tension and the U both were even greater towards the end than towards the beginning . . . An increase of tension—general tension—after that." D 14.

Below are sample reports in which relaxation is correlated with P:

"P. I should say quite a bit more relaxation than in any other of the stimulus periods. I should call the pleasure the absence of kinaesthesis, the relaxation, the lack of opposing tension in the muscles." A 40. "Moderately P. A singular absence of kinaesthesis . . . I was thoroughly relaxed." A 55. "That had a forperiod of U. I was apprehensive. I was alarmed. I could feel the tension relax, particularly in the upper part of the head. My consciousness was next P by way of anticipation . . . The P I could find little basis for except absence of disturbing kinaesthesis." A 63. "Tension followed by relaxation and deeper breathing. All initiated before I had anything P except by ideational anticipation . . . Attention that accompanies your approach was rapidly followed by relaxation of the muscles and deeper breathing—P." A 66. "I don't know whether the P caused the relaxation." A 69. "That was P . . . The result was ease and relaxation which spread back and surrounded the violet (odor) without diminishing the degree of P which I had from the start felt from the violet." A 70. "It was moderately P. A feeling of relaxation. It tended to increase my relaxation." F 1. "That was a P odor. It tended to make me more relaxed—to stretch." F 17. "It had a calming effect; tendency to relax. Very P." F 23. "At first a litle tension but the effect of the taste was calming, relaxing, soothing . . . It was moderately P." F 33. "It had a calming, soothing effect. A tendency to relax—was quite P." F 34. "Soothing, calming effect seemed to be localized in the front of the nose. A relaxing effect on the body. A tendency simply to let go. Very P." F 36. "A definite tendency to relax . . . It was very P." F 40. "Definite sensations from the central part of the chest. Produced a sort of diffused feeling. A slight tendency to relax. Moderately P." F 42. "Odor had a very soothing, calming effect. A tendency to relax—very P." F 45. "I believe a slight tendency to relax although it was approximately indifferent. A weak P feeling produced." F 47. "A slight tendency to relax. Weakly P." F 48. "Mildly P. Slight relaxation." C 8. "That is mildly P. It is relaxing, reminiscent." C 11. "P. Mildly P. A rather clean and clear and relaxing kind of a thing." C 15. "The feeling tone changed.

At first it was P and then tiresome . . . At first a relaxation and various fleeting images but that all went before the end." C 16. "There was conscious relaxation, a breathing slower and deeper in an effort to give myself up to the smell. A fleeting wonder if I would have time for a second intake of breath. My judgment now is that it was mildly P, that I would have had the same P if I had relaxed and breathed more slowly and deeply without any smell." C 21. "When I realized that it was a smell there was the usual relaxation—deep, slow breathing . . . I think the smells give me a kind of day-dream relaxation that is mildly P. I mean it is a P way to occupy your time till something more interesting comes up. You are simply passive." C 24. "That was P. There was a general relaxation, deeper breathing." C 30. "The P was a relaxing P rather than an exciting P." D 3. "I think of the experience in parts, and tend to report that the straining parts were U and the relaxing parts or moments were P." D 6. "Slightly P. I should say that the chief thing was a bit of relaxation very mildly felt through the chest and general muscular system, considerably more through my face." D 13. "P. Sort of a soothing effect. A tendency to relax." E 11. "That was P. I think there was a feeling of relaxation when I found it was the candy and not the vinegar." E 18. "That was P, fairly intense. I didn't observe any kinaesthesis. The soothing effect is a tendency to relax more." E 43.

The tendency for strain to occur with U and for relaxation to occur with P is by no means necessary and invariable. In the first place, we have several reports of strain and relaxation in which no mention is made of either P or U. In the second place, while the straining attitudes of anticipation, sensory preparation, attention, etc. are generally U in our data, there is no *a priori* reason why such strain should not occur simultaneously with P. The three reports (Table II) in which strain is associated with P are of this sort. In every case, however, the P is very weak, and there is no guarantee of strict simultaneity of strain and P.

"I should say it was very weakly P . . . Tendency to tenseness of some of the facial muscles (related to the questioning attitude)." B 43. "I should say it was very faintly P. I didn't notice anything in the way of response other than a slight tensing of the muscles about the nose (related to olfactory attention)." B 45. "A very little tension in the muscles. It was P, I think, though not very much—a low degree." F 3.

In the third place, we have six reports in which the subject was asked to hold out the arm for 60 sec. In three cases the strain produced was U. In the other three, however, it was indifferent or "interesting." A laboratory attitude may make a strain mildly agreeable! Hence the U is not intrinsic to the strain,[4] for strain is not necessarily U.

From the above considerations we conclude that the association between strain and U, and between relaxation and P, is not necessary and invariable. In this it resembles the correlation-tendencies found by the expressive methods.[5]

Intensity of response in relation to P and U. Our data make it possible to investigate the relationship between intensity (or extensity) of bodily response and P-U. The reports contain (1) organic-kinaesthetic processes related to the affective reaction, (2) the corresponding feeling with frequent mention of its intensity. What is the relationship between P and U, of different intensities, and the number or amount of organic-kinaesthetic processes reported?

To answer this question we have given every report an 'organic score' which shows the number of separate organic-kinaesthetic processes reported. As this rating was in no case greater than four we adopted a five-point scale: 0 1 2 3 4. A few reports contain processes which apparently have no relation to the affective response, such as the incidental and accessory movements of chewing and swallowing, and random eye-movements. These movements, when reported, have *not* been included in the count and are *not* shown in the organic scores. Whenever there was any doubt as to the significance of the association between organic-kinaesthetic process and affective reaction, as in the case of heart-sensations, chest-pressures, and the like, these processes *have been* included in the count. The organic score of a report, therefore, represents the number of probably significant organic-kinaesthetic processes reported. We have taken it as an index of the intensity or extensity of bodily response.

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[4] We have a large number of reports showing the dependence of P-U upon mental attitude or "brain set." "If I take the laboratory attitude, it is P no matter how U it may be. It is either P or U according to my point of view." C 9. "One difficulty is from the attitude of being in a laboratory experiment. There is a mild P all the time. Most of the experience is P from that point of view." C 19. "Neither I guess but I haven't the slightest objection to your calling it mildly either . . . I can't tell you anything about the two judgments except that evidently I have different points of view." C 21. "Potentially U but again rather interesting and interesting things are intrinsically P." C 22. "I imagine that if I didn't like doing laboratory experiments, I'd spit out the vinegar and the fact that I do swallow it means that I like it from one point of view." C 27. "That was U, of course, but it was so mixed with interest that I hate to call it U. I don't want to call it U because I was distinctly interested." 32. "I imagine you could give me pretty disgusting odors and I'd find them P. They are interesting. It is interesting to get down into an an odor." A 34. "In so far as it became interesting it wasn't U." A 49.

[5] Leschke, Erich (Die Ergebnisse und die Fehlerquellen der bisherigen Untersuchungen über die körperlichen Begleiterscheinungen seelischer Vorgänge, *Archiv. f. d. ges Psychol.*, 1914, 31, 30), finds an agreement of about 90% among previous investigators.

That there may be no misunderstanding as to the meaning of the organic score we give below a few examples of reports of every grade on our scale.

Reports with organic score of 0:

"Rather P. I couldn't detect any kinaesthesis there at all." E 20.

"Quite surprisingly U . . . I had expected something P because the same stimulus was P. I was surprised." A 20.

"That was P, quite P . . . The only kinaesthesis was that of the actual movements of chewing and swallowing." B 32.

Reports with organic score of 1:

"Very P and intense. A tendency to breathe deeply . . ." G 25.

"Rather U. A kinaesthetic tendency to pucker up my nose." E 22.

"U. . . . a slight tendency to scowl— a tightening of the muscles about the eyes." B 12.

Reports with organic score of 2:

"Quite P. A calming, soothing effect, a tendency to relax. I wanted to inhale as much as I could. Organic sensations from the heart and chest, moderately diffuse . . ." F 34.

"Not so very U . . . I was aware of the usual pinching, withdrawing response. The whole situation was sufficiently marked to produce reverberations that accompany most of the U stimuli . . ." A 45.

"That was very U. A tension of the muscles, especially those of the face and mouth. The muscles in the rest of the body were also made more tense and I seemed to get that feeling of tension toward the stomach." E 8.

Reports with organic score of 3:

"Decidedly U. A definite jerking back of the head. Tightening up of the eye muscles—a closing of them. I was aware of circulatory changes localized in the spot struck and I should guess that there was a rush of blood to the face for that particular spot felt hot." B 23.

"Moderately U. The first tendency was to refrain from breathing. The second was to move my head away. There was also a tendency to contract the muscles about the nose and upper lip." B 49.

"It was rather relaxing at the most P part. At other times I think it was mainly exciting but not relaxing . . . It is true that there were curious little quivers and trembles and reverberations through my fist and it is true that there was a tendency to smile but it does not seem correct to say that they *were* the P but rather that they were the expression of the P as well as of the relaxation and the excitement." D 8.

Reports with organic score of 4:

"That was distinctly U. There was a foreperiod of straining apprehension and a kinaesthetic tendency to look up . . . The rap was very sharp and I felt a twitch throughout the length of my body . . . The reflex was followed by one of those warm waves that goes over you—an unpleasant keying up of the body . . . There was distinctly a vasomotor change . . ." A 41.

"That was very U. A tension of the muscles especially those of the face and mouth. The muscles in the rest of the body were also made more tense. I also seemed to get that feeling of tension down towards the stomach. I seemed to get a taste of the liquid in the œsophagus. It seemed to be something going down—U." F 8.

TABLE III

Average Organic Scores

	Pleasantness			
	Weak	Average	Strong	No Report Intensity
Number of Reports	34	29	31	28
Their Total Organic Score	25	23	23	16
Average Organic Score	*0.7*	*0.8*	*0.7*	*0.6*

	Unpleasantness			
	Weak	Average	Strong	No Report Intensity
Number of Reports	47	29	27	45
Their Total Organic Score	45	45	61	43
Average Organic Score	*1.0*	*1.5*	*2.2*	*0.9*

	Indifference
Number of Reports	70
Their Total Organic Score	37
Average Organic Score	*0.5*

Table III shows the average distribution of the organic scores. From it several conclusions may be drawn.

First, the average organic scores for U are in every case greater than those for P. If we take the total P reports as a class, we find an average organic score of 0.7 ± 0.6; the same for U gives 1.3 ± 0.7; and for I, 0.5 ± 0.7. Although the

M.V.s are relatively large, indicating that the organic response has a considerable range of intensive variation, they are practically constant for all classes of report. The total number of reports upon which these averages are based is: P, 122; U, 148; I, 70 (Table I).

Secondly, the organic scores for U increase with the intensity of feeling. The more intense the feeling, the more widespread is the bodily reaction. For P, on the other hand, no such relationship appears. The average organic scores for P are practically constant for all intensities and classes of P. The 31 reports of intense P have exactly the same average score as the 34 reports of weak P.

Thirdly, the average organic score for the reports of indifference is less than any score for P or U.

When no intensity is reported the organic scores lie midway between weak feeling and indifference, and we hazard the guess that the feeling-intensity of this class of reports is, on the average, very weak. As the intensity of P and U decreases, the organic scores approach that of I.

We should note that relaxation is included in the organic scores of P as a significant process. Some of our subjects describe relaxation as the "letting up of strain," "the absence of kinaesthesis," *i.e.*, they tend to regard relaxation negatively as the release of strain.[6] Suppose that we assume that relaxation is negative, merely the release of strain, and on the basis of this assumption eliminate relaxation from the organic scores. If we do this, the organic scores for P become equal to that of indifference. Hence, with our data, P is differentiated from indifference primarily by relaxation, which itself is negative and represents a return to indifference (from one point of view) rather than a departure from it.

If, for the sake of comparison, we eliminate strain from the U reports, the average organic score is reduced from 1.3 to 1.1; and the latter is well above indifference (0.5).

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[6] Shepard, John F., Organic Changes and Feeling, *Am. Jour. of Psychol.*, 1906, 17, 522 ff. "Strain is described as composed of sensations from the muscles, the backflow from the acting muscles, particularly those of accommodation of a sense organ. . . . Relaxation seems to be a release from either strain or excitement." Cf. Titchener, E. B., Zur Kritik der Wundt'schen Gefühlslehre, *Zeits. f. Psychol.*, XIX, 1899, 321 f. Our result confirms the later polemical work against the Wundtian theory as regards the nature of strain and relaxation.

TABLE IV

AVERAGE ORGANIC SCORES WITH CUTANEOUS STIMULI ELIMINATED

Pleasantness	Weak	Average	Strong	No Report Intensity
Number of Reports	34	27	29	28
Their Total Organic Score	25	20	23	16
Average Organic Score	*0.7*	*0.7*	*0.8*	*0.6*

Unpleasantness	Weak	Average	Strong	No Report Intensity
Number of Reports	38	13	11	23
Their Total Organic Score	35	17	27	15
Average Organic Score	*0.9*	*1.3*	*2.4*	*0.65*

	Indifference
Number of Reports	55
Their Total Organic Score	27
Average Organic Score	*0.5*

It may be objected that the foregoing result is due to the selection and balance of stimuli. The cutaneous stimuli (slaps, pinches, etc.) were for the most part U while we had no stimuli capable of evoking correspondingly intense P. Had we used other stimuli and more intense P situations the differences of the curves might disappear.

To test this criticism we have eliminated all cutaneous reports and worked out average organic scores for the remaining odors, tastes, and chords (Table IV). We believe that simple odors, tastes, and chords are comparable from the affective standpoint. The elimination of cutaneous stimuli reverses the P-U balance (P 118; U 85. See Table I). However, the conclusions still stand; the differences are even clearer than before. The U curve is steeper. Consequently the presence of a relatively large number of U cutaneous stimuli does not account for the differences in the organic scores.

As regards intensity, we have no means of comparing P with U. We do not know whether the " intense P " of a perfume is equal to the " intense U " of caproic acid, and whether the " moderate P " of peppermint-candy is comparable with the " moderate U " of vinegar. It would be worth while to study these organic curves at higher levels of feeling-intensity. We may conjecture that the P curve would rise slightly and gradually, and that the U curve would rise suddenly and abruptly to a point much higher than the most intense P would reach.

We should note, however, that the relative numbers of " weak " " average " and " strong " Ps and Us are about the same (Table III). The intensive balance of our reports is very good.

Fundamental characteristics of the unpleasant response. An analysis of our data brings out several fundamental characteristics of the U response.

First, there is the well-known tendency to withdraw oneself from the stimulus-object, either reflexly or deliberately.

" Much the strongest avoidance impulse so far . . . It was all I could do to keep from brushing it away." A 38. " U. Quite a definite desire to withdraw and prevent one's face from being damaged." A 64. " A decided tendency to draw my head back which was not inhibited in this case." B 15. " I got away from it. The kinaesthesis was a definitely overt action." B 28. " Slightly U. I reduced the intensity of the sensation by drawing my head away." B 37. " U, strong. A tendency to shut off the breathing—to reduce it. Tendency to draw head away . . ." B 42. " Moderately U. The first tendency was to refrain from breathing; the second was to move my head away." B 49. " The head was jerked aside with the blow. It was moved to the left." G 32. " A very strong tendency to turn the head away from the odor." F 14. " First just a little start or shock. A tendency to draw the head up and back away from the spot rubbed." F 30. " Immediately there was a tendency to jerk my head away and to push away with my right hand." F 35. " A great deal of kinaesthesis particularly in my eye-lids as if protecting my eyes from a blow and a general tendency to shrink away." C 2. " A tendency to stop up my ears and to draw away from the stimulus." C 9. " The U was many strains in a tendency to shake your head and get away from it—and frowning and possibly a tension through the body in legs and arms as if to help you bear it." D 5.

Secondly, closely related to the above is the tendency to put the object away from oneself or to prevent its action. Biologically this type of response has the same purpose as the above.

"Provoked kinaesthesis of pushing the stimulus away—in the left arm in particular." A 19. "It was all I could do to keep from brushing it away." A 38. "The organics of an odor like that lead one to reflexes of repulsion." A 42. "U. Quite a noticeable tightening of the muscles in the lower part of the face. It seemed to be the reverse of sniffing—an attempt to prevent air from coming in. If I'd been wholly natural, I'd have closed the nostrils with my hand." B 25. "U, rather strong intensity. I got a movement of my intestines, a sort of nausea. It was an expulsive movement preliminary to vomiting." E 51. "I had a tendency to move my mouth to blow it away. It was disgusting. I thought I got a little nausea." E 56. "A tendency to lift up the right arm and brush it away. Tensing of neck and jaw muscles." F 49. "A tendency to stop up my ears and a tendency to draw away from the stimulus." C 9. "I tended to spit it out. Then the laboratory attitude came back . . . If you get too naïve and too relaxed you have a physical reaction and nothing else. I think that time I would simply spit it out." C 14. "I am perfectly sure that under other circumstances I should have actually reached up and knocked away the stimulus from my chin." D 10.

Thirdly, there is a tendency to inhibit or resist (frequently due to the experimental situation) some normal response to the stimulation.

"A tendency to keep it in my mouth rather than to swallow it." B 18. "U. There was a much weaker tendency to resist smelling than in the other case. Only a slight tendency to tighten the muscles about the nostrils to the extent that it interfered with breathing." B 27. "I tended to inhibit the movement of swallowing. I think there was some tenseness about the nose and throat which may have been connected with my inhibiting of the swallowing." B 47. "The first tendency was to refrain from breathing. The second was to move my head muscles and to hold the head in position against any increase in pressure." B 15. "There was some resistance to swallowing although it was swallowed." G 20. "A tendency to resist the stimulus." E 27. "Slight stiffening of the neck to hold the head rigid as the friction was increased." G 18. "A slight tendency to shut out the noise . . . to put my fingers in my ears." G 30. "There was a movement in my cheek muscles and my facial muscles producing a frown and a reaction against the stimulus in the opposite direction." E 48. "There was a rivalry between the tendency to move the head away and to hold it there." E 61. "Kinaesthesis was localized in my chin—a resistance to the rubbing. A tendency to pull away from the placed rubbed." F 6. "I had to force myself to hold the chin still. A tendency to withdraw my chin from the rubbing." F 9.

Muscular strain or tension is frequently present in the form of anticipation of a U stimulus. From one point of view such strains may be regarded as the resistance to an expected undesirable situation.

Fourthly, the unpleasant response is accompanied by reflex twitchings, convulsive contractions, and bodily reverberations of various sorts, especially when the stimulus-object is presented suddenly and there is an element of surprise.

> "At first a bit of suspense; then I jumped (marked reaction). Consciousness was empty there and the first thing I remember was the sensation from the jump . . ." C 10. "No feeling tone at first during the violent reaction, the jump. Then it began to tingle . . . There was a desire to slap back." C 25. "A little shiver of what one might call revulsion went through me. The shiver itself seemed to start in my chest and go even down into my legs. It was something the same kind of a shiver you get with the first reaction to cold." D 10. "The reaction was a kinaesthetic jump of the whole body. A sudden tension and then it was all over (S's face seen to twitch)." D 12. "At first a little shock. I didn't expect the sounds. It seemed to be spread down toward the fingers, the arms." F 3. "A slight jar or shock at the beginning of the playing. The shock was felt in the heart and chest." F 16. "A sudden start. A sudden tension of the muscles as soon as you struck me." F 4. "Quite a definite tensing of the diaphragm and a jump in the hand which tapered off rapidly." A 7. "Distinctly U. It starts reflex twitchings in every part of the body, particularly in your hands but also in your head and around your ear. Quite a mass of tendencies to react." A 22. "At first a start that is so reflex that it is done for you. Then after a slight pause the wave of sensation. It is a wave coming from my feet and rises higher and higher and then recedes." A 26. "Quite decidedly U. I very distinctly felt my heart give a thump. There was a wave-like twinge that goes up your back. This time I was conscious of it going down my right leg." A 36. "I felt a twitch throughout the length of my body. The reflex followed by one of those warm waves that goes over you—a keying up of the body." A 41. "The whole situation was sufficiently marked to produce reverberative reflexes that accompany most of your U stimuli." A 45. "I felt movement down my neck and chest as well as on my cheek (marked reaction)." E 33. "I had a movement in the pit of my stomach right beneath the floating ribs." E 39. "U, very intense. I got movement in my neck and facial muscles, twitchings (seen)." E 52. "There was a feeling as if perspiration came out. A muscular reaction—movement of the head." E 79.

Summing up the characteristics of the unpleasant response we may say that, first, there is a tendency to react away from the stimulus-object; secondly, a tendency to put the stimulus-object away from oneself or to prevent its action; thirdly, a tendency to inhibit or resist the normal repsonse (tension); fourthly, bodily twitches, shocks, waves of sensation, and other reverberative reflexes.

Analysis of the P response.—While U is associated with a large variety of reflex movements, P is passive and negative. Our data do not contain a single case of active reflex response to a P stimulus. With U there are withdrawing movements, frowning, straining, reflexes of expulsion, etc. while P is characterized by mere acceptance of the situation and the passive yielding to it.[7] Also the bodily reverberations which are associated with U (twitches, shocks, waves of sensation, etc.) are entirely absent from the P reports. U is organically positive and active while P is negative and passive. This general finding brings into relief the significance of relaxation. Muscular relaxation is the typical process associated with P and, so far as our data go, both qualitatively and quantitatively, differentiates P from indifference. Relaxation itself is negative and passive, a letting-up of strain.

The traditional relation between P and seeking movements finds little support in our reports, while that between U and withdrawal is abundantly confirmed. The entire evidence for seeking movements, as 'expressions' of P, is found in 11 reports, in which the subject "sniffed," "took a deeper breath in order to get more," "held the breath," etc. In every case these seeking movements are deliberate and never reflex like most of the withdrawing movements of U. It is a question to what extent and in what sense voluntary, deliberate behavior based upon the knowledge that P may be produced or prolonged, or U avoided, can be considered an 'expression' of feeling.[8]

CONCLUSIONS

Let us now ask the orginal question: is there any characteristic difference between the organic factors of P and U feeling which may throw a light on the physiological mechanism of affection?

I. A study of the processes reported by the subjects shows that there is no organic-kinaesthetic *sine qua non* of affection. P and U are reported in more than a third of the cases without any mention of organic-kinaesthetic processes; and when such processes are reported, they bear no fixed and invariable relationship to the affections. However, a number of tendencies toward correlation can be made out, and of these the most probable is that between muscular strain and U and muscular relaxation and P.

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[7] "I was willing to have the thing continued." A 4. "A relaxing effect on the whole body. A tendency simply to let go." F 36. "It is the kind of a thing you want continued." C 16.

[8] If we look at the question theoretically, it is clear that in many cases seeking movements are expressions of U: strong hunger, for example. The situation is complicated by the fact that the total seeking activity may become P, as when prey is sighted or food smelled. With the seeking movements of sex there is room for argument. Unsatisfied and unrestrained desire is probably U when associated with seeking. However, there may be vaso-dilatation, general relaxation, anticipatory imagery, etc., so that the total activity of seeking is P. Since positive seeking movements may be associated with either P or U, the traditional coupling of P with seeking is unsatisfactory.

II. A statistical study of the organic-kinaesthetic processes in relation to affection and an analysis of the reports themselves bring out the fact that: U is associated with a positive bodily response which becomes more intense and widespread as the feeling becomes stronger, while with P the bodily response is relatively slight. When one reacts away from the stimulus-object, or puts it away from oneself, or resists it (strain), or when bodily 'reverberations' are present, U is apt to be felt. P, on the other hand, so far as our data go, is organically-kinaesthetically negative. P is felt when one relaxes, or simply 'does nothing;' there are no reflex responses to the stimulus-object and no bodily 'reverberations.'

It should be remembered that any result is a function of conditions. Our subjects were seated quietly in a Morris chair, instructed to be "passive and receptive" and "to let the experimental situation have its full normal effect." In one of the earlier instructions they were asked to "make no resistance to the stimuli; to let them have their full effect." The *Aufgabe,* therefore, was to accept the stimuli calmly and quietly, or else as one does in ordinary life. With this set simple odors, tastes, tactual impressions, and a few chords and discords were presented. Those responses which are described as U involve a positive reaction which is absent with P.

AN EXPERIMENTAL STUDY OF KINAESTHETIC IMAGERY[1]

By Alice Helen Sullivan

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INTRODUCTION

In the first part of this paper we seek to determine the differences between sensory and imaginal kinaesthesis. It is often said that the kinaesthetic sensation and the kinaesthetic image are difficult or impossible to distinguish. We have attacked the problem by way of simple kinaesthetic complexes, experienced in their sensory reality and reproduced in imaginal terms. The results of this comparative study, attributive (sensational) and perceptive, are set forth on p. 67.

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[1] From the Psychological Laboratory of Cornell University.

In the second part of the paper we have compared resident kinaesthesis with the projected kinaesthesis of the empathic experience. The results appear on page 78.

I. Kinaesthetic Sensation and Kinaesthetic Image Compared

We wished to obtain, for the purpose of comparison, a psychological picture of sensory and imaginal kinaesthesis, first in attributive and then in perceptive terms. We made out 50 short commands to be read to the *O*'s. We paired these commands for sensation and image thus: (1) "Think of: nodding your head once." (2) "Nod your head once." (3) "Raise your index finger once." (4) "Think of: raising your index finger once." In order that the *O* should not know when to expect a sensory or an imaginal stimulus, we arranged the experiments by this schema: IS IS IS SI SI II SS IS IS IS.[2]

O sat in a dark room, facing a black wall. *E* sat at a table a few feet behind *O*, and near a window through which a streak of light, just sufficient for his needs, was allowed to shine.[3] After *O* had read the instructions, *E* asked him to close his eyes. After a few minutes of rest, to free the eyes of *O* from possible images, *E* said "Ready," waited an instant, and read the stimulus-sentence. As he spoke the last word of the sentence, which was always "once," *E* started a stop-watch. When *O* said "Yes," indicating that the command had been obeyed, the watch was stopped.[4] Then the complete report of *O* was taken down. An interval of 15 sec. then elapsed before the giving of the next stimulus-sentence.

We began the experiments in May, 1919, and carried them on during the following months of June (with the exception of a two weeks' vacation), July, August, and October. There were seven *O*'s: Dr. L. B. Hoisington, instructor in psychology; Drs. H. G. Bishop and M. J. Zigler, assistants in psychology; Mr. P. J. Cavanaugh, Misses C. Comstock, R. Stutsman, and A. H. Sullivan, all graduate students in psychology.[5]

Experiment I.

We performed preliminary experiments, in order to try out our 50 paired commands, and to give our *O*'s practice in describing their processes. We noticed very soon that, in the pairs designated in the schema as SI, all *O*'s showed a tendency to be influenced in the imaginal part of the experiment by the report made upon the preceding sensory complex. The finding of attributive characters in the sensory complex suggested that the same attributes would appear in the image. An effort on *O*'s part not to be thus biassed resulted often in a complete inhibition of imagery.

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[2] I = Image; S = Sensation.

[3] A small shaded light was used for evening experimentation.

[4] We have not included the times in the following results, because we found no typical times for sensation and image, the time for the carrying out of the sensory and imaginal commands being approximately the same.

[5] St observed only during the summer term and did not complete the perceptive experiments. When the writer (Su) acted as observer, the experimenter (Ca, and later Miss M. F. Martin) made out new sets of stimulus-sentences.

Experiment II

In the light of these preliminary experiments, we settled upon the 15 paired commands which seemed simplest and yet were adequate for our needs. We also separated and distributed the sensory and imaginal members of the pairs, so that the schema SI SI SI etc. still held good, except that the imaginal member of a given pair preceded the sensory by a number of sentences.

The instructions read: "You are to follow the instructions which I shall give you and to report the experiences thus set up with especial reference to kinaesthesis.

"Every kinaesthetic experience may show perceptive characters (locality, pattern, etc.) and attributive characters (quality, intensity, etc.). In the present series I wish you to report the experience in terms of quality, intensity, and extent.

"You may neglect every other character of the kinaesthetic experience, and you may neglect all non-kinaesthetic experiences aroused by the presented stimuli.

"If, however, you find yourself noting any further feature of the experimental consciousness, I shall be glad if you will report it."

It was necessary to fractionate the experiences for report. In Group I we asked for quality, intensity, and extent; in Group II, for intensity, extent and duration; in Group III, for extent, duration, and vividness; in Group IV, for duration, vividness, and locality; in Group V, for vividness, locality, and temporal course; in Group VI, for locality, pattern, and course; in Group VII, for quality, pattern, and course. In this way we covered the attributive and perceptive characters a number of times, using always the same 30 stimulus-sentences. We took one practice-series with every new group, in order that our *O*'s might attain a fair degree of stability of attitude.

Stability of attitude was not attained at once. All *O*'s with the exception of Su were more familiar with visual imagery than with kinaesthetic. Visual images came constantly at first, and acted as a distraction. After training in the observation of kinaesthetic processes, much of the visual imagery dropped into the background, and the kinaesthetic processes were sufficient to carry the whole meaning of an imagined movement. Then the *O*'s showed more certainty about their kinaesthetic imagery. After training, the *O*'s were able also to describe more accurately the kinaesthetic sensory processes.

Sources of error due to wrong attitudes had also to be discovered and eliminated. Some *O*'s fell into the habit of holding or building up the image, instead of letting it run its course naturally, as they did in the case of sensations. We had to make it clear to them that such "holding" was possible only by a shift of attitude.

Another source of error was that of using a different scale for sensation and image in reports upon the intensive attributes. We explained that the intensive characters mean distances from zero, and that the same scale could therefore be used in both cases.

Results

We found that, whereas the actual perception of movement aroused by our stimuli was very complex, having a number of different sensations whose attributes had to be reported upon separately, the image of the movement was in some cases for all *O*'s, and in most cases for a few *O*'s, actually a single image-process of one quality, intensity, etc., which carried the whole meaning of movement. An actual nod, *e.g.,* aroused several sensations; the thought of nodding aroused, in many instances, one simple image. In other cases, the attributive reports seemed to indicate a single image-process, but the perceptive reports which came later showed that the apparently simple process was in reality an imaginal complex, with a pattern and temporal course, which the attributive *Aufgabe* did not always bring to light.

We have tabulated our results in parallel columns for sensation and image.[6] There was no mention of the terms image and sensation in the instructions; and yet our stimuli were cut closely enough so that all *O*'s reported, with as much assurance as they would show in reporting upon visual sensations and images, "I observed a kinaesthetic image," or "I experienced kinaesthetic sensations." If the *O*'s indicated any doubt whether a slight movement had been made in the carrying out of an imaginal command, we threw out the results. The columns for sensation, then, represent reports of the experiences aroused by our sensory commands, but at the same time they represent the experiences which were introspectively recognized as sensory; while the columns for image represent reports of the experiences aroused by our imaginal commands, but only those experiences which were recognized introspectively as imaginal.

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[6] In the tables, the figures within the columns show the frequencies of report for the rubrics which head the horizontal columns. If, *e. g.,* the number after *O*'s initial is 3, this means that the 30 stimulus-sentences were repeated 3 times for report upon the character in question; 45 experiments for sensation and 45 for image were performed.

(1) *Quality*

TABLE I

QUALITY

	B (3)		Ca (3)		Co (3)		H (3)		St (1)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Du. P	49	40	26	29	18	33	17		19	11	17	9	53	34	199	157
Br. P							35						6		41	
Drag							6						2		8	
Sm. P	10	3	23	3	19	2	34		16	9	31		24	8	157	25
Strain	28		28		35		38		9		20		31		189	
Ache	15		9		5		11				9		1		50	
Lt. P	12	5	11	29	6	13	9	47			20	38	17		75	132
Total	114	48	97	62	83	48	150	47	44	20	97	47	134	42	719	314

Du. P. = Dull Pressure; Br. P. = Bright Pressure; Sm. P. = Smooth Pressure; Lt. P. = Light Pressure (or Neutral Pressure.)

Ca reported also strain in the image once in the early part of the experiment. Z reported 3 cases of strain and 3 of bright pressure in the images. The following were also reported in early observations: sharp pressure, Z, for image 2, for sensation 4; granular pressure, Z, for image 1, for sensation 1; coolness, Co, for sensation 2; St, for image 1; dizziness (which later was found to be nothing but dull pressure), for sensation, Ca 5; St, 1; Su, 3; for image Su, 2.

We see that the characteristic qualities in kinaesthetic sensations are dull pressure, light pressure, smooth pressure, strain, and ache. These were reported by all *O*'s except St, who reported no light pressure. Dull pressure averages the highest frequency, although the individual reports show that Ca, Co, and H reported the greatest number under strain, and Su the greatest number under smooth pressure. H and Z reported also bright pressure and drag. Bright pressure is experienced only when the *O* is in very good physical condition. Drag is very like dull pressure, and may have been considered qualitatively the same by the other five *O*'s.

The table shows very strikingly the fact that the image is less varied in quality than the corresponding sensations. The headings for quality run thus:

Observer	*Sensation*	*Image*
B	5	3
Ca	5	3
Co	5	3
H	7	1
St	3	2
Su	5	2
Z	7	3

In every case a greater number of qualities belongs to sensation.

Table I shows also the complexity of a perception of movement as compared with the imaginal reproduction of that movement. All the *O*'s except St performed 45 sensory experiments and 45 imaginal experiments on quality. The numbers in the total at the bottom of the table show that the different *O's* reported from 83 to 150 separate qualities in the 45 sensory experiments, while they reported from 42 to 62 separate qualities in the 45 imaginal experiments. When we remember that 45 is the lowest number possible if we have one image for every experiment, it becomes evident that in most cases one image carried the whole meaning of the movement, provided that we take one quality to mean a simple process.[7]

TABLE II

INTENSITY

	B (2)		Ca (4)		Co (4)		H (2)		St (2)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Strong	20		16		16		7		24		15		17		115	
M. St.	22		5		3		17		6				21	7	74	7
Mod.	11	7	63	40	58	27	22		31	4	28	10	20	18	233	106
M. Wk.	8	16					5	10	1			3	14	16	28	45
Weak	13	14	48	45	33	23	25	16	11	12	39	21	6	2	175	133
V. Wk.			3	6	3	22		5	3	18		11		3	9	65
Total	74	37	135	91	113	72	76	31	76	34	82	45	78	46	634	426

M. St. = Moderately Strong; Mod. = Moderate; M. Wk. = Moderately Weak V. Wk. = Very Weak.

The following were reported in the early part of the experiments, and were not reported in later observations: kinaesthetic image, strong in intensity, Ca, 1; Co, 7; Z, 2; moderately strong, St, 1; moderate, H, 1; kinaesthetic sensation, very weak in intensity, H, 2; Su, 1; Z, 1.

All the *O*'s but H reported dull pressure for the image; all but St and Z, light pressure; all but H and Su, smooth pressure. The smooth pressure has so low a frequency as compared with the other two qualities that we must doubt its validity as a separate quality for the image (cf. p. 73). We may be fairly certain, then, that the characteristic qualities in kinaesthetic imagery are dull pressure and light pressure; we may say without any doubt that all qualities in the image are "pressury" (a term used by *O*'s in describing the quality of images).

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[7] For some observers (H and generally Z) one quality was to all intents and purposes a simple process, but for others it was not. We must not be misled by quality, for even though there is but one quality present, we have no guarantee that the image is not complex. There are still possible variations of intensity and vividness, as well as changes in temporal course. We found, indeed, that quality oftentimes played a surprisingly minor rôle in kinaesthetic experiences. Quality is usually regarded as the individualizing attribute; but our *O*'s were often able to describe the intensive attributes of a sensation or image when they could not describe its quality. Furthermore, the quality of the kinaesthetic images was monotonous in its sameness, and yet the image was varied and interesting because of slight shadings of intensity, or slight changes during its brief temporal course.

The table shows only 42 imaginal qualities reported by Z in 45 experiments. In reality he reported 51 qualities, but the other 9 are included in a footnote to the table, because they are not qualities reported by other *O*'s and were reported by Z only in early experiments.

There is neither strain nor ache in the image, though the kinaesthetic image may mean a movement that would be strainful or achy in sensation.

There were indications that there is a difference in brightness in the qualities of sensation and image. All qualities in the sensations seem to have a certain "liveness" or brightness which the qualities in the images lack. H stated the difference thus: "The quality of the image is dead and static, while the quality of the sensation 'wells up,' has brightness or liveness or sparkle, and is dynamic." There is, of course, always the possibility that an obscure perceptual motive has colored the sensations.

(2) *Intensity*

We find that the intensity of kinaesthetic sensations ranges from strong to weak. The whole intensive scale is represented in all the reports, save that three *O*'s report a few sensations of very weak intensity, while four report none below weak.

The highest frequency of report for B and Z occurred under the rubric "moderately strong;" for Co, Ca, and St under "moderate;" and for H and Su under "weak." Thus, the middle portion of the scale was most favored.

The kinaesthetic image has less range over the intensive scale than the sensation, going only from "moderate" to "very weak." There is one exception to this statement: Z reported 7 images as "moderately strong." Z showed a tendency to estimate all the intensive attributes high. This may have been due to the fact that he "held" his images at first; for the tendency disappeared in the later experiments.

The highest frequency for B's images was in the "moderately weak" group; the highest frequency for Ca's, H's, and Su's images was in the "weak" group; the highest frequency for Co's and Z's was in the "moderate" group. For all observers except Z more than half of all the images reported belong to the lower half of the scale. If we consider the totals at the left of the table, we see that the highest frequency for sensation fell under the "moderate" group, and for image, under the "weak" group.[8]

We may not say, then, that kinaesthetic images are weaker in intensity than kinaesthetic sensations, for some kinaesthetic sensations are weak; but we may say that kinaesthetic images are never strong in intensity, as kinaesthetic sensations often are.[9]

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[8] We cannot expect the individual reports to agree absolutely, for our *O*'s were using subjective scales. The small divergences from the average for the group show, however, that these scales must have been very much alike in all cases. In general, it is not the absolute estimate that we care for, but the relative judgments which show the difference between sensation and image.

We do not have agreement of numbers in the grand totals in the different tables. The characters for report were fractionated, and were repeated a different number of times.

[9] The *O*'s, with further training, confined their reports on intensity of images more and more to the lower part of the intensive scale (cf. p. 73).

(3) *Extent*

TABLE III

EXTENT

	B (2)		Ca (5)		Co (4)		H (2)		St (2)		Su (5)		Z (6)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
V. Ex.	12		2		8		13				4				39	
Ext.	14		40	13	40		18		10		41		74	12	237	25
M. Ex.	10	2	19	11			7		11	2	29		15	4	91	19
Mod.		4			34	15	6		9	10		3	54	36	103	68
M. Lm.	6	7	8	5	6	8	5	2	1		17	22	1	2	44	46
Lim.	8	9	35	31	1	36		19	5	14	37	37	35	33	121	179
V. Lm.	6	13		2		5		12	6	5		15	6	15	18	67
Total	56	35	104	62	89	64	49	33	42	31	128	77	185	102	653	404

V. Ex. = Very Extended; Ext. = Extended; M. Ex. = Moderately Extended; Mod. = Moderate; M. Lm. = Moderately Limited; Lim. = Limited; V. Lm. = Very Limited.

The following were also reported; kinaesthetic image, very extended, Ca, 2; Co, 1; extended, Co, 2; St, 1; kinaesthetic sensation, very limited in extent, Ca, 2. All of these reports occurred in the early part of the experiments.

The extent of kinaesthetic sensations ranges from very extended to very limited. All *O*'s but one favored the rubric "extended" the exception being St who favored "moderately extended." Twice as many sensations are found in the upper portion of the scale as in the lower.

The extent of the kinaesthetic image varies within smaller range than that of kinaesthetic sensations. To be sure, the table shows that two *O*'s reported a number of their images as "extended." It was in this group of experiments, however, that we discovered the same *O*'s "holding" their images. After we had instructed them that there must be no voluntary holding or building up of the image, the reports of "extended" dropped out almost entirely. In spite of this source of error, we see that by far the greatest number of images fall in the lower part of the extensive scale, and that no images reach the maximum of extent attained by some sensations.

Most of the images reported by B were "very limited;" most of those reported by Ca, Co, H, St, and Su were "limited;" while most of those reported by Z were "moderate." When all the sensations reported by all the *O*'s are totalled, the highest frequency falls under the rubric "extended;" when all the images reported are totalled, the highest frequency falls under the rubric "limited."

We may say, then, that kinaesthetic sensations tend to be extended, while kinaesthetic images tend to be rather limited. We may say also that whereas kinaesthetic sensations are sometimes limited in extent, kinaesthetic images are never maximally extended.[10]

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[10] The *O*'s, with further training, agree more and more in their reports upon the extent of the image (cf. p. 74).

(4) *Duration*

TABLE IV
DURATION

	B (2)		Ca (5)		Co (5)		H (3)		St (2)		Su (5)		Z (5)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Long	15		25	35	30		32		15		31		74	14	222	49
M. Lg	11		39	27	71	20	12		4		47		8		192	47
Mod	12	10					2	2	15	6		3	24	5	53	26
M. Sh	14	6	58	8	6	19	2	2		3	17	13	16	12	113	63
Short	4	15		28	5	26	12	34	7	6	34	46	16	27	78	182
V. Sh			4	2		9		8	3	11		13	5	15	12	58
Total	56	31	126	100	112	74	60	46	44	26	129	75	143	73	670	429

M. Lg. = Moderately Long; Mod. = Moderate; M. Sh. = Moderately Short; V. Sh. = Very Short.

The following reports occurred in the earlier experiments, and dropped out entirely in later experiments: kinaesthetic image, long in duration, Co, 2; H, 1; St, 2; moderately long, H, 1; Z, 1.

The duration of kinaesthetic sensation may be long, medium, or short. The sensations, however, tend to group themselves about the upper half of the scale, more than 50 per cent of all the sensations reported being above moderate.

The highest frequency fell under the heading "long" for B, H, and Z; under "moderately long" for Co and Su.

The kinaesthetic image, according to the table, may be anywhere from "moderately long" to "very short" in duration. This statement excludes the reports of Ca and Z, who in the first 3 repetitions of this group of experiments, "held" their images. After the instruction to let the image run its course naturally, the reports of "long" dropped out. For all *O*'s but Ca, more than half of all the images reported are in the lower part of the scale.[11]

As to highest frequencies, we find the greatest frequency occurring under the rubric "short" for all *O*'s except Ca. In the totals, the highest frequency is under "long" for sensation, and under "short" for image.

(5) *Vividness*

TABLE V
VIVIDNESS

	B (2)		Ca (3)		Co (3)		H (3)		St (1)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
V. H					5	6	2	3			2	9			9	18
High	29	10	8	15	18	23	43	35	24	10	16	24	27	24	165	141
M. H	13	16	17	10	12	1	6	1	3	9	13	6	12	16	76	59
Mod	9	15	12	18	21	5	5	3	6	5	17	5	29	8	99	59
M. Low	14	2	23	4			9				2		24	3	72	9
Low	13	2	25	5	14	10	21	4	3	3	21		15	4	112	28
Total	78	45	85	52	70	45	86	46	36	27	71	44	107	55	533	315

V. H. = Very High; M. H. = Moderately High; Mod. = Moderate; M. Low = Moderately Low.

In an early experiment, Su reported one case in which an image was of a low degree of vividness.

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[11] With further training the *O*'s confined their reports entirely to the lower part of the durative scale (cf. p. 75).

We find that the vividness of kinæsthetic sensations ranges from high to low. The sensations generally share their vividness with other members of the sensory complex, and are, therefore, not of the highest degree of vividness. The sensations in a perception of movement reach the highest degree of vividness only when they demand attention because of their quality (ache or strain), or their high intensity.

Most of the sensations reported by B, H, and St were "high" in vividness; most of those reported by Co and Z were "moderate;" and most of those reported by Ca and Su were "low." For Ca, Co, Su, and Z there is a pretty even distribution between the three rubrics of "high," "moderate," and "low."

The kinaesthetic image makes up in vividness what it lacks in the other attributes. Comparatively few images were reported "low" in vividness. In nearly all these cases, the lack of vividness was due to the occurrence of a visual image along with the kinaesthetic image. Co had more images of a low degree of vividness than any other *O*; she always reported, in such instances, "The kinaesthetic image was complicated by a visual image." Ca and B had also visual accompaniments to their kinaesthetic imagery, in which cases the vividness of the kinaesthesis varied from "moderate" to "low."[12]

The highest frequency occurred under the heading "high" for Co, H, St, Z, and Su; under "moderately high" for B; under "moderate" for Ca. When all the sensations reported are totalled, the highest frequency comes under the rubric "high," just as the total for all images shows as highest frequency the rubric "high."

The kinaesthetic image, then, is of a high degree of vividness. It has the advantage over sensation in its simplicity and uniformity; it has a chance to stand alone in consciousness for a brief time. While kinaesthetic sensations may be as often low in vividness as high, kinaesthetic images, unmixed with imagery from other sense departments, are never low.

(6) *Locality*

TABLE VI

LOCALITY

	B (2)		Ca (3)		Co (3)		H (3)		Su (4)		Z (4)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Deep	25	10	50	7	9	3	56		77	30	83	18	300	68
M. Deep	7	5	18	10	57	12	21		11	2	41	33	155	62
M. Sup	12	3	8	10		2	46		9	4	2	1	77	20
Sup	16	14	6	28	16	29	5	45	30	2	55	25	128	143
Total	60	32	82	55	82	46	128	45	127	38	181	77	660	293
Def	23	16	41	34	10	29	13	35	54	13	61	8	202	135
M. Def	11	4	22	10	43	10	42	8	18	12	53	35	189	79
Indef	22	18	15	4	28	8	47		45	13	66	32	223	75
Total	56	38	78	48	81	47	102	43	117	38	180	75	614	289

M. Deep = Moderately Deep; M. Sup. = Moderately Superficial; Sup. = Superficial; Def. = Definite; M. Def. = Moderately Definite; Indef. = Indefinite. B reported 9 images which were localised "outside;" Su reported 19 images as "not localised."

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[12] Where the visual image acted as a distraction, it perhaps would have been better to count the experiment unsuccessful. We have not done this, for the disturbance seemed to affect only the reports on vividness. We have, therefore, included everything reported by all our *O*'s.

The depth of kinaesthetic sensation seems to be correlated with its quality. Dull pressure is deep or moderately deep; smooth pressure is deep or moderately deep; strain is more superficial; light pressure is superficial. The highest frequency of report for all *O*'s except Co was "deep;" the highest frequency for Co was "moderately deep."

The superficially localised sensations are, as a rule, definite in localisation; the deep sensations are sometimes quite definite, at other times indefinite. The highest frequency of report fell under the heading "definite" for B, Ca, and Su; under the heading "moderately definite" for Co; and under "indefinite" for H and Z: yet the highest frequency for depth, it will be remembered, came under "deep" or "moderately deep" for all these *O*'s, a fact which shows the difficulty of trying to correlate depth and definiteness. If we consider our total numbers, however, we find the highest frequencies coming under the rubrics "deep" and "indefinite."

The reports upon depth of the kinaesthetic image do not agree among the *O*'s. The highest frequency for B, Ca, Co, and H was "superficial;" for Z, "moderately deep;" for Su, "deep." Here no part of our scale for depth is favored by all *O*'s. B perceived some images as being outside the body, either just touching the skin or entirely separate from the body. Su reported 19 images which were so indefinite that she called them cases of "non-localization." They were unequivocal in reference; they *meant* that she was making the movement in imaginal terms, but they did not seem to be localized in the body.[13]

The images were most often definite in localization for Ca, Co, and H; indefinite for B; and moderately definite for Z. In the totals, we have our highest frequencies under "superficial"[14] and "definite."

Our study of locality reveals a greater diversity among *O*'s than we found in our study of the attributive characters. This is to be expected, for here we are dealing with perception. The image, perhaps because it was a surface-like feel, was perceived by most *O*'s as being at or near the surface. Yet the highest frequency of report for quality came under the heading of dull pressure for B, Ca, and Co, three of the four *O*'s whose highest frequency of report upon locality was "superficial." Again, Su's highest frequency for quality came under the heading of light pressure, while the locality of her images was most often deep. Again, we might be tempted to suppose that the image was localized at or near the surface because it was perceived, generally speaking, as flat and thin, like the cutaneous qualities. And yet Su and Z reported a great many images which were perceived as "flat," but were localized as deep or moderately deep. We can only say, then, that although quality and bidimensionality are probably conditions of the perception of depth in the kinaesthetic image, there are other important conditions which our reports did not reveal.

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[13] There is an analogy to this indefiniteness of localization in the visual field. D. Katz characterizes the film-color as "indefinite" in localization; "Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung," 1911, 73 ff.

[14] Superficial does not always mean "on the surface;" it may mean "skin-deep" or "just underneath the skin."

(7) *Pattern*

TABLE VII

PATTERN

	B (2)		Ca (2)		Co (3)		H (3)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Broad to narrow	8		3	3	1	2			14		2		28	5
Longer than broad	12		3	2	3	3	11		6		3		38	5
Spot to streak	2	15	3	2	21	19	8		6	23	18	7	58	66
Irreg. patch	3	3							12		7		22	3
Small spot				5	2	4		29	4	4	7	19	13	61
Qual. diff's			3		1				4		2		10	
Int. and viv. shadings	5	8	1		4	10			6	5	2		18	23
Total	30	26	13	12	32	38	19	29	52	32	41	26	187	163

Irreg. = Irregular; Qual. diff's = Qualitative differences; Int. & viv. shading = Intensive and vividness shading.

Su and Z reported also spots which became "rayed" on all sides, five times and once respectively; H reported a sharp, regular outline for the image 16 times; Z reported a "mosaic of spots" 7 times for sensation and twice for image (he could not decide whether it was qualititive or intensive.)

The totals in this table do not equal those in the former tables, because often there was no observable pattern in the kinaesthetic experience.

Kinaesthetic sensory complexes show great diversity of pattern. There were reported, by all observers but H, "masses" which ran out to points in pyramidal fashion; there were indefinitely outlined "patches," sometimes "bulky," sometimes "flat," and usually "longer than broad;" there were "spots or lumps" which stretched out to "bands" during their course; there were small spots which remained unchanged during their course, for all but B and Ca. Dimensionality did not come into all reports, but where it appeared, the kinaesthetic sensations are mostly tridimensional. The *O*'s who did not report upon it were questioned at the end of the experiment, and stated that so far as they could remember most of their kinaesthetic sensations had been in three dimensions.

The qualities showed characteristic patterns. Strain was perceived as "stringy" or pointed; dull pressure as "massive" and "bulky," and often "elongated;" light pressure as "patchy," "sheet-like," or "flat." The patterns themselves had attributive differences within them: "nuclei" of dull pressure, strain, or smooth pressure, with "fringes" of light or bright pressure (for H and Z); "bands" of dull pressure with pointed ends of strain (for Ca, Co, and Su). Intensive and vivid variation, such as more intense or vivid center, point, or end, or "strips" or "cores" or "mosaics" of greater intensity or vividness, were reported by all.

The kinaesthetic images show more uniformity in pattern than the kinæsthetic sensations. Their pattern consisted almost entirely of small spots, which sometimes remained unchanged during their brief temporal course, and at other times ran out temporally to small streaks. Images, too, were bidimensional, or of very slight thickness.

For all *O*'s the qualitative patterning of the images was uniform. For some *O*'s (Ca and H) the images were uniform in all the attributes; for others there were at times intensive and vivid variations within the pattern: a less intense border which was perceived as "fringed" if the quality was dull pressure; as "fuzzy," "feathery," or "like a bit of down, only smoother," if the quality was light pressure; a greater intensity or vividness at one end or side; or a wave of vividness which travelled over the image (for Ca and Su).

(8) *Temporal Course*

TABLE VIII

TEMPORAL COURSE

	B (4)		Ca (2)		Co (3)		H (4)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Increase in intensity	22	20	29	7	24	15	80		29	3	39		223	45
Increase in extent	12	8	10	7	35	29	47		29	23	35	5	168	72
Increase in vividness	18	8	13	8	6	1	5		10	11	3		55	28
Decrease in intensity	31	17	18	2		2	14		8				71	21
Decrease in extent	6		5	2	2		3						16	2
Decrease in vividness	28			2	3				4		11		46	2
Addition of quality	42		6		20		75		29		42		214	
Subtraction of quality	37		7		6		31		7		16		104	
No change		22		13				52		17		40		144
Total	196	75	88	41	96	47	255	52	116	54	146	45	897	314

Kinaesthetic sensory complexes undergo attributive as well as processual changes in temporal course. All the *O*'s reported increases in intensity, extent, and vividness, while some reported decreases in intensity, extent, and vividness. Processual changes were reported by all. A perception of movement might begin, *e. g.*, with dull pressure, to which would be added strain, and perhaps ache. The ache might be very brief, leaving the strain and dull pressure. Then the strain would drop out, perhaps, leaving the dull pressure. Addition of quality was more frequent than subtraction of quality for all *O*'s except Ca.

Kinaesthetic images or imaginal complexes undergo very little attributive change and no processual change during their course. Oftentimes the image remained absolutely unchanged (for all *O*'s but Co). Increase in some one of the intensive attributes was common with B, Ca, Co, and Su; most often in intensity for B and Ca, most often in extent for Co and Su. All of H's images were without pattern or temporal course; only 5 of Z's images had temporal course. Decreases in the intensive attributes were rarely reported by anyone but B. No processual changes were reported. An image never adds to itself a new process. It simply flashes in and out, with now and then a second image following it, but entirely separate from it.

(9) *'Body' or Collocation of Attributes*

Kinaesthetic sensations have more "body" than images. The intensive attributes in the sensory integrations are of varying degree. If a sensation is of very weak intensity, and of very limited extent, it will be, in all probability, somewhat long in duration. A haphazard search through reports on sensation shows the following combinations:

Observer	Intensity	Extent	Duration
Co	weak	very extended	long
	strong	mod. limited	long
H	weak	mod. limited	mod. long
	mod. strong	extended	short
Su	weak	extended	mod. short
	weak	mod. limited	long
Z	weak	mod. extended	mod. short
	moderate	extended	long

The majority of reports on image for these 4 observers show integrations of this sort:

Intensity	Extent	Duration
weak	limited	short
mod. weak	very limited	very short
very weak	very limited	very short

Although there were reports of sensations which were weak, limited, and short, there were never reports of sensations as very weak, very limited, very short. Yet all *O*'s gave many reports of images which were very weak, very limited, and very short.[15]

SUMMARY

Kinaesthetic sensation and kinaesthetic image compare thus:

(1) The qualities in kinaesthetic sensation are dull pressure, light pressure, bright pressure, smooth pressure, strain, and ache. The qualities in kinaesthetic image are all 'pressury,' dull pressure, light pressure, and possibly smooth pressure (cf. p. 73).

(2) The intensity of kinaesthetic sensation may be anywhere on the intensive scale from strong to weak. The intensity of the kinaesthetic image is always comparatively weak.

(3) Kinaesthetic sensations range all over the extensive scale from near zero to maximum, with a tendency however to group themselves about the upper part of the scale. The extent of the kinaesthetic image is for the most part limited, and it never reaches the maximum of extent attained by some sensations.

(4) The duration of kinaesthetic sensation is, in most cases, relatively long. The duration of the kinaesthetic image is

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[15] Cf J. Lindworsky, "Wahrnehmung und Vorstellung," *Zeits. f. Psych.*, LXXX, 1918, 215 ff.; C. Stumpf, "Empfindung und Vorstellung *Abh. d. kgl. preus. Akad. d. Wiss.*, 1918. These articles reached us, unfortunately, too late for more than a mention at this place. See also E. B. Titchener, Thought-Processes, 1909, 20 f., 214; and for evidence of 'holding' in the sphere of sensation, R. T. Holland, *Journ. Exp. Psych.*, iii., 1920, 312 ff.

relatively short; it never reaches the highest part of the durative scale.

(5) The vividness of kinaesthetic sensations ranges from high to low. The vividness of the kinaesthetic image, on the other hand, is always high, unless the image is accompanied by imagery from other sense-departments.

(6) The locality of kinaesthetic sensations ranges from deep and indefinite to superficial and definite, with a tendency to be deep and indefinite. Dull pressure and smooth pressure are deep; strain and ache, more superficial; and light pressure, superficial. Kinaesthetic images are localized definitely by most *O*'s on or near the surface. They show no correlation between quality and depth or definiteness.

(7) Kinaesthetic sensory complexes show various types of pattern: 'pointed' or 'stringy' for strain; 'bulky' and 'roundish' or 'elongated' for dull pressure; and 'flat' and 'patchy' for light pressure. There are qualitative differences and intensive and vivid shadings within the sensory pattern. The sensations are for the most part tridimensional. Kinaesthetic imagery shows always the same pattern, a small spot, which sometimes increases temporally to a streak. It is uniform in all attributes for some *O*'s; it has intensive shadings for others. The images are most often bidimensional.

(8) The kinaesthetic sensory complexes show attributive as well as processual changes in temporal course, increasing and decreasing in all the intensive attributes, and changing in quality by addition and subtraction. Kinaesthetic imagery has very little, if any, temporal course, being generally too brief for change. Cases in which the image undergoes a slight increase in extent or intensity are the only exceptions to this statement.

(9) Kinaesthetic sensations have more 'body' than kinaesthetic images. What the sensation lacks in any one intensive attribute it makes up for in some other intensive attribute; the image may be poor in all the intensive attributes save vividness.

II. RESIDENT AND PROJECTED KINAESTHETIC IMAGERY

We have now compared kinaesthetic memory-image with simple kinaesthetic perception, and have been able in this way to compare kinaesthetic sensation with the kinaesthetic simple image aroused under certain (*i.e.*, memory) conditions. We have, however, no reason to suppose that we have exhaustively described 'the' kinaesthetic simple image. Under other conditions of arousal, the image may show in other phenomenal modes. In particular, there may (we thought) be various attributive differences between the resident kinaesthetic image of memory and the projected kinaesthetic image of empathy, if indeed we have any psychological right to speak of a 'projection' of kinaesthesis at all. This, then, is the point upon which our further experimentation is directed.

We made out a new list of paired commands for the arousal of kinaesthetic imagery, half of them representing situations which called for usual, ordinary, familiar movements, similar to those of the preceding experiments, the other half representing situations in which there were unusual movements.

The experimental procedure was the same as before. The instructions, too, were the same. The work began at the end of October, and continued throughout the next four months. Two of our *O*'s, Ca and St, left the university; the remaining five served in this second period of our work.

Experiment III

We performed about 50 preliminary experiments, in order that our *O*'s might attain a fair degree of stability of attitude. We found that now, when the situations were more complex, the visually minded *O*'s realized the situations for the most part in visual terms, in spite of the instructions. We decided, therefore, to change the wording of our commands, making them read, "Feel: Tantalus standing in water, trying in vain to drink," instead of "Think of: Tantalus standing in water, etc." We explained that by "feel" we meant "realize kinaesthetically." This slight change in the stimuli effected wonders in the results. Visual imagery became unimportant, while the kinæsthetic imagery, in many cases, carried the whole meaning of the situation.

Experiment IV

We were now ready to begin the main part of the experiment. We made out fifteen paired commands, one member of every pair referring to a usual situation, the other member referring to an unusual situation.[16] Again we fractionated the characters to be reported upon, asking in Group I for quality, intensity, and extent; in Group II. for duration, vividness, and locality; in Group III. for pattern and temporal course. We repeated every group, so that we had 60 experiments for every group.

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[16] Examples of the stimuli are: "Feel: yourself running downstairs." "Feel: an acrobat walking a tight rope." "Feel: Laocoon struggling in the coils of the serpent." "Feel: yourself stooping to pick up a pencil." The colon after the word "feel" indicates that a short pause was made after this word in reading the stimuli.

Results

We found that the kinaesthetic images aroused by our stimuli may be classified under two main headings: (1) resident kinaesthesis, which is like that studied in the first part of the paper, the kinaesthetic imagery which ordinarily is referred to oneself; and (2) projected kinaesthesis, the kinaesthetic imagery which is projected outward, and is referred ordinarily to someone else or something else.

The *O*'s came upon the projection gradually. H, *e. g.*, said: "That kinaesthesis was not in me; it belonged to the visual image." "The kinaesthesis did not relate to myself; it related to someone else." And later (of an image aroused by the stimulus, "Feel: a farmerette pitching hay"), "The kinaesthetic image belongs to the farmerette, it is a kind of empathy."

Z, after reporting for some time the fact that some of his kinæsthetic images did not belong to himself, but to the person referred to in the stimulus, said: "It is a kind of projection."

Su reported at first thus: "The kinaesthetic image belonged to myself; then it was projected into an imaginary figure." And for another situation, relating to Tantalus trying in vain to drink: "The imagery was in me and then in Tantalus, but somehow it was transferred from the right side of my neck to the left side of Tantalus, who was out in front and facing me." Later, Su reported: "It seems as if the kinæsthesis is lifted bodily out of me and put into someone else; then it changes attributively. Sometimes, it sinks back into me at the end of the report." Still later Su reported projection without having first noted the beginning in resident kinaesthesis.

Co reported for the stimuli referring to the self: "The kinaesthetic image was in me," but for the stimuli referring to someone else: "The kinaesthetic image was not in me, but went with the visual image to make up the situation."[17]

Finally, B said of a projected kinaesthetic image: "It is as if it were fully formed out in space, and happened to catch my attention." And again: "It is fully formed like an object out there to be looked at."

Thus, all the *O*'s came, rather slowly but certainly, to a realization that some of their images were resident, while others were projected out into a foreign object.

We found, however, that before the resident and projected kinæsthesis could be differentiated clearly, we had to break up an attitude which some of the *O*'s had carried over from the work on sensation and image. There, it will be remembered, because of the mistakes of several *O*'s who "held" or "built up" their images, we had warned all the *O*'s against a shift from the passive or receptive attitude towards sensation to an active or constructive attitude toward their imagery. When their attention was thus called to the change, they realized that the images which were taken passively, as the sensations were, did not persist, but were sudden and brief. It was then natural to keep the passive attitude, and to be content to report upon the brief "flash" of kinaesthesis as it came and passed.

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[17] Two of the *O*'s always had visual accompaniments to their projected kinaesthesis; three had nothing but kinaesthetic imagery, the whole imaginal figure and the meaning of the situation being carried in terms of dull pressures, light pressures, bright pressures, strains and aches.

But here where we are working upon a differentiation within the realm of imagery itself, the *O*'s *must be able to shift attitudes.* The projected kinaesthesis begins as a brief flash of resident kinaesthesis, which is then projected out into a foreign object, and becomes stabilized as the attitude shifts from passive to active. After an *O* has become habituated to the active attitude of projected kinaesthesis, the beginning bit of resident kinaesthesis may fail to be observed. Before such habituation, however, the resident kinaesthesis and the projected kinaesthesis may alternate, making the experience mixed. If, now, the *O* is accustomed to the passive attitude, he will seize upon the fleeting resident image, and will begin his report immediately after this 'flash' of experience, instead of waiting for 'stabilization.' He will thus report upon resident kinaesthesis partially changed, perhaps, by admixture of a bit of projected kinaesthesis.[18]

As soon as the *O*'s became familiar with the two attitudes, we asked them to alternate the two. When we instructed them to take an active attitude toward the stimuli which normally gave resident kinaesthesis, they reported that the image could not be made to persist, but slipped away, repeated itself, and produced a quick succession of images of like patterns. This shows that true 'stabilization' was not possible. When we instructed the *O*'s to take a passive attitude toward the stimuli which normally gave projected kinaesthesis, they reported different patterns with each succeeding 'flash' of experience, until in the end all the separate patterns returned, became stabilized, and persisted as a complete situational image. This shows that stability was necessary here for a complete realization of the experience. In these cases, neither the 'flash' nor the 'stabilization' was quite natural: the 'flash' was not the totally involuntary thing that normally occurs in resident kinaesthesis, and was longer in time; and the 'stabilization' was not complete as in projected kinaesthesis; there were gaps in time, in which there was no process. The *O*'s thus discovered that, although either attitude is possible with any stimulus, the active attitude is natural for projected kinaesthesis, as the passive attitude is natural for resident kinaesthesis. They reported also that the active attitude seems to grow naturally out of the passive, as stability grows naturally out of a beginning 'flash.'[19]

From the statements of the *O*'s in characterizing the two attitudes, we have gleaned the following differences between the two. The attitude in projected kinaesthesis is the perceptive attitude; the situation is objectified, and the *O* contemplates it as he would a landscape; the report upon the imagery is more like an *Aussage*-report than a scientific description; the attitude is correlated with imagery which is so full that it is not all clear at once, imagery which is persistent and unchanging as the different parts become vivid in turn for report; the attitude is active, exploratory, detached. The attitude in resident kinaesthesis is the sensory attitude, the same as that of the earlier part of our work; the *O* experiences the imagery as he would a sensation or an after-image; the report is the ordinary laboratory description; the attitude is correlated with imagery which is all clear at once, and is fleeting and changing; the attitude is passive, receptive, realizing.

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[18] The two terms 'flash' and 'stabilization' were used by an *O* in distinguishing the experiences under the two attitudes.

[19] It may be that some of Perky's critics have been misled by failure to recognize the different attitudes. Their *O*'s may not have waited for stability, but may have reported upon the first brief 'flash' of experience; and may then have called this an 'image of imagination,' when in reality it was a 'memory image' from which an 'image of imagination' might (with shift of attitude) have developed.

A. Resident and Projected Kinesthetic Imagery Compared

The following tables, arranged upon the same plan as those for the comparison of sensation and image, show the difference in the psychological pictures of resident (R) and projected (P) kinaesthesis.

(1) *Quality*

TABLE IX

Quality

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Du. P	30	29	30	26	85	1	22	9	41	18	208	83
Brt. P	11				34		18		1		64	
Drag					2				2		4	
Sm. P					13		5		1		19	
Strain	2		16		45		3		10		76	
Ache	1		4		21		2		6		34	
Lt. P	1	26	4	7	16	43	32	32	26	11	66	119
Total	45	55	54	33	216	44	82	41	87	29	471	202

P = projected kinaesthetic imagery; R = resident kinaesthetic imagery.
Du. P. = Dull Pressure; Brt. P. = Bright Pressure; Sm. P. = Smooth Pressure;.
Lt. P. = Light Pressure (or Neutral Pressure.)

Projected kinaesthetic imagery is qualitatively very much like sensory kinaesthesis. Dull pressure, strain, ache, and light pressure are common. Bright pressure occurs much more often in the projected imagery than in the sensory complexes; only one *O* failed to report it here, while only two *O*'s found it in actual sensations.[20] The existence of smooth pressure in projected kinaesthesis is doubtful; only three *O*'s reported it, and that in a comparatively small number of cases. Drag is not common in projected kinaesthesis.

From 4 to 7 different qualities were reported in projected kinaesthetic imagery. Dull pressure has the highest frequency in the average, and for the individual *O*'s with the exception of Su.

Resident kinaesthetic imagery has but two qualities, dull pressure and light pressure. In the preceding part of our work we were in doubt whether to include smooth pressure in the list of qualities for imagery. Since none of the *O*'s, after further training, reported it, we may conclude that it is not characteristic of resident kinaesthetic imagery (cf. p. 59). Light pressure has the highest frequency in the average, although individual *O*'s gave the highest frequency to dull pressure.

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[20] A 'textural' difference is reported between the bright pressure of sensation and the bright pressure of projected kinaesthesis. In sensation, it is 'sparkling' like a liquid upon which a light is shining; in projected imagery, it is misty, like a halo or a sun-lighted cloud.

Here, where our situations are more complex, a single image-process did not always stand for the whole situation. Often the reports showed an imaginal complex made up of a number of simple processes following one another in quick succession, and differing from one another very slightly. There were never two such processes present at once, as there were in the projected kinaesthesis and in sensory kinaesthesis.

(2) *Intensity*

TABLE X

INTENSITY

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Strong	14		17		33		8				72	
M. St.					23		5		5		33	
Mod.	5	19	25	12	33		21	3	32	16	116	50
M. Wk	5	7			5	2	11	12	37	15	58	36
Weak	10	16	8	17	26	29	27	11	17	3	88	76
V. Wk	8	13	4	4	14	10	14	14	5		45	41
Total	42	55	54	33	134	41	86	40	96	44	412	203

M. St. = Moderately Strong; Mod. = Moderate; M. Wk. = Moderately Weak; V. Wk. = Very Weak.

The intensity of projected kinaesthetic imagery ranges from strong to weak, with the highest frequency of the average coming under 'moderate' as in the sensory complexes. The highest frequency for B falls under the rubric 'strong;' for Co, under 'moderate;' for Su, under 'moderately weak.'

The intensity of resident kinaesthetic imagery ranges from moderate to very weak, with the highest frequency averaging 'weak.' The highest frequency for B and Z occurs under 'moderate;' for Co and H, under 'weak;' for Su, under 'very weak.'[21]

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[21] We see that the *O*'s have ceased to report any resident kinaesthetic images in the upper half of the intensive scale (cf. p. 60). Training undoubtedly had an effect here; but there is a more important factor to be considered, namely, the fact that the *O*'s had no absolute criterion. It was natural, then, that the resident imagery should be comparatively weaker when compared with projected kinaesthesis, which represented large and complicated movements, than it was when compared with simple sensory movements, such as we used for stimuli in the earlier experiments.

(3) *Extent*

TABLE XI

EXTENT

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
V. Ex.	2				33		6				41	
Ext.	3		7		22		14		1		47	
M. Ex.	20				29		25		3		77	
Mod.	2	16	31	7			11	3	39	6	83	32
M. Lm.	4	11			13		12	7	33	11	62	29
Lim.	5	10	16	30	6	9	15	16	12	12	54	77
V. Lm.	6	16	1		4	21	3	14	10	5	24	56
Total	42	53	55	37	107	30	86	40	98	34	388	194

V. Ex. = Very Extended; Ext. = Extended; M. Ex. = Moderately Extended; Mod. = Moderate; M. Lm. = Moderately Limited; Lim. = Limited; V. Lm. = Very Limited.

Projected kinaesthesis, like sensory kinaesthesis, varies in extent from very extended to very limited. The highest frequency comes under the heading 'moderately extended' for B and Su; under the rubric 'moderate' for Co and Z; under the rubric 'very extended' for H.

According to the table, the resident kinaesthetic image never goes beyond the middle part of the extensive scale.[22]

The highest frequency of report is 'limited' for Co, Su, and Z; and 'very limited' for H. The highest frequency in the totals comes under the heading 'moderate' for projected kinaesthetic imagery; the highest frequency in the totals comes under the heading 'limited' for resident kinaesthesis. Thus all the resident images are found in the lower half of the scale, while over 50 per cent of the projected images are found in the upper half of the scale of extent.

(4) *Duration*

TABLE XII

DURATION

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Long	32		48		175		92		117		420	
M. Lg.												
Mod				1								1
M. Sh.		13		4				8		6		28
Short		6		24		1		11		5		42
V. Sh.		17		9		49		14		19		99
Total	32	36	48	38	175	50	92	33	117	30	420	170

M. Lg. = Moderately Long; Mod. = Moderate; M. Sh. = Moderately Short; V Sh. = Very Short.

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[22] Training *plus* a larger relative scale for comparison has again levelled out the discrepancies which we found in the earlier reports (cf. p. 61).

Projected kinaesthetic images are always long in duration. The whole image comes fully formed, and there is no addition or subtraction of process.

Resident kinaesthetic images are always short in duration. The highest frequency for all *O*'s falls under the rubrics 'short' and 'very short.' The highest frequency in the totals comes under the rubric 'very short.'[23]

(5) *Vividness*

TABLE XIII

VIVIDNESS

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
V. H.	6	4	1	7	20	43	2	4	1	15	30	73
High	9	9	19	14	56	6	25	16	2	10	111	55
M. H.	13	19	12	11	7		33	8	20	3	85	41
Mod	5	5	8	1	6		19	5	33	2	71	13
M. Low	2				30				30		62	
Low			6	1	60		13		33		112	1
Total	35	37	46	34	179	49	92	33	119	30	471	183

V. H. = Very High; M. H. = Moderately High; Mod. = Moderate; M. Low = Moderately Low.

The vividness of projected kinaesthesis is divided evenly between high and low. Not all of the projected experience can be in the focus of attention at one time. The highest frequency of report falls under the heading 'moderately high' for B and Su; under 'high' for Co; and under 'low' for H.

The vividness of resident kinaesthesis ranges from very high to moderate. Most of B's images were 'moderately high;' most of Co's and Su's were 'high;' most of H's and Z's were 'very high.' The greatest number of images in the total falls under the heading 'very high.'

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[23] The table for duration shows markedly the relativity of the subjective scale. In the earlier experiments, most of the images were 'short' in duration, but here by far the greatest number of images are 'very short' in duration (cf. p. 62). We can account for the change in report by the fact that the projected imagery is (objectively) very long in duration.

(6) *Locality*

TABLE XIV

LOCALITY

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Deep	17	10	13		85		21	8	37	1	173	19
M. Deep	8	9	31		53		16	4	56	3	164	16
M. Sup	3	3	1	15	38	5	21	6	18	10	81	39
Sup	7	8	2	19		44	32	5	9	16	50	92
Total	35	30	47	34	176	49	90	23	120	30	468	166
Def	8	23	9	29	65	46	23	5	3	19	108	122
M. Def	10	8	21	4	41	6	24	7	41	9	137	34
M. Ind		1			23		4	11	38	1	65	13
Indef	15	3	18	1	49		39		38	1	159	5
Total	33	35	48	34	178	52	90	23	120	30	469	174

M. Deep = Moderately Deep; M. Sup. = Moderately Superficial; Sup. = Superficial; Def. = Definite; M. Def. = Moderately Definite; M. Indef. = Moderately Indefinite; Indef. = Indefinite.

B reported 2 sensations and 5 images localised "outside;" Su reported 9 images as ' not localised."

Generally speaking, projected imaginal kinaesthesis, like sensory kinaesthesis, is localized as deep and indefinite. There is, however, much individual variation. As to depth, the highest frequency for B and H is under the rubric 'deep,' for Co and Z, under 'moderately deep;' for Su, under 'superficial.' As to definiteness, the highest frequency for B and Su comes under the rubric 'indefinite;' for Co and Z, under 'moderately definite;' for H, under 'definite.'

Resident kinaesthetic images are most often localized as superficial and definite. Most of Co's, H's, and Z's images were 'superficial' and 'definite;' most of B's were 'deep' and 'definite;' most of Su's were 'deep' and 'moderately indefinite.'

We find very few images in resident kinaesthesis which are indefinite in locality, although earlier (p. 63) we had a fair proportion of images reported under this rubric. We know of no way to account for the change except by the effect of training. One *O* said of resident kinaesthetic images: "They are so definitely localized that I could pin a ribbon on them."

We find, as before, cases of non-localization of resident images.

(7) *Pattern*

The pattern of projected kinaesthesis is even more varied than that of sensory kinaesthesis. All *O*'s reported 'long strips' of bright pressure or dull pressure; 'strings' of strain; masses, longer than broad, of dull pressure; irregular patches of light pressure or bright pressure; small spots of dull pressure or light pressure. The bright pressure was sometimes characterized as 'hollow,' like a short tube

TABLE XV

Pattern

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Strip	5	12	3	24	7		4	16	24	16	43	68
Longer than broad	27		18		92		31	6			168	6
Irreg. Patch	3		5		5		43		10		66	
Spot		9		7	50	52	28	16	51	11	129	95
Qual. diff's			13		104						117	
Int. and Viv. shadings	4	11	22	16	121		49	16	96	7	292	50
Def. outline				4		52	9				9	56
Indef. outline	1		14				6				21	
Total	40	32	75	51	379	104	170	54	181	34	845	275

Irreg. patch = Irregular patch; Qual. Diff's = Qualitative differences; Int. and Viv. shadings = Intensive and vivid shadings; Def. outline = Definite outline; Indef outline = Indefinite outline.

The following were also reported: " hollow pattern " for Projected Kinaesthesis, 10 (Su), 1 (B); " mosaic of spots " for Projected Kinaesthesis, 20 (Z).

or 'band-box.' This is a pattern which did not occur in sensory kinaesthesis. As in sensory kinaesthesis, so in projected kinaesthesis there were qualitative differences within the patterns, as well as intensive and vivid shadings, characterized as 'striped,' 'mosaics,' 'wrinkled,' 'spotted,' or 'fringed.' The projected patterns were often very indefinite in outline. They were both tridimensional and bidimensional. It is noteworthy that the frequency of report under every heading is much larger for the projected kinaesthesis than it was for the sensory kinaesthesis (p. 65). It is obvious that this difference is to be explained by the fact that the patterns in projected kinaesthesis persist unchanged while the *O* explores them, whereas the patterns in sensory kinaesthesis are in constant flux, and must be caught 'on the wing' for report.

The resident kinaesthetic images, like the kinaesthetic images of the earlier experiments, are characterized generally as small spots, which somtimes remain unchanged and at other times run out temporarily to streaks. Again we had no qualitative differences reported, but quite often intensive and vivid differences, such as " fringes " or " fuzzy " borders. As opposed to the projected kinaesthesis, the resident kinaesthesis is definitely outlined and, for the most part, bidimensional.

(8) *Temporal Course*

Projected kinaesthetic imagery shows no change in time. The imagined situation, made up entirely of kinaesthetic imagery, or supplemented by visual imagery, remains fully formed out in space, like an object at which we are looking, so that the *O* reports from the actual (imaginal) perception.

Resident kinaesthetic imagery comes more suddenly than projected imagery, and remains only a brief time, a time too brief to allow of any change for some *O*'s, but long enough for others to permit a slight increase in intensity or extent. The resident kinaesthetic images pass so quickly that the *O* must report them from memory.

SUMMARY

Projected and resident kinaesthetic imagery compare thus:

(1) The qualities in projected kinaesthesis are dull pressure, light pressure, bright pressure, strain, and ache. The qualities in resident kinaesthetic imagery are dull pressure and light pressure.

(2) The intensity of projected kinaesthesis ranges from strong to weak. The intensity of resident kinaesthesis ranges from moderate to weak.

(3) The extent of projected kinaesthesis ranges from maximum to near zero. The extent of resident kinaesthesis ranges from moderate to near zero.

(4) The duration of projected kinaesthesis is always long. The duration of resident kinaesthesis is always short.

(5) The vividness of projected kinaesthesis ranges from high to low. The vividness of resident kinaesthesis ranges from very high to moderate.

(6) The locality of projected kinaesthesis varies from deep and indefinite to superficial and definite, with a tendency toward depth and indefiniteness. The locality of resident kinaesthesis tends to be superficial and definite.

(7) Projected kinaesthesis shows various types of pattern: 'strips,' 'masses,' 'patches' and 'spots.' These patterns have qualitative differences and shadings of intensity and vividness. Both two and three dimensions are common. The resident kinaesthesis is unvaried in pattern, being ordinarily a spot which may run out temporally to a streak. It sometimes has slight shadings of intensity or vividness. It is most often bidimensional.

(8) Projected kinaesthesis shows no change throughout its long temporal course. Resident kinaesthesis may show slight attributive changes during its brief temporal course; it never shows processual changes.[24]

B. Some Conditions of Resident and Projected Kinaesthetic Imagery

In the foregoing experiment, we obtained resident kinaesthetic imagery when the stimuli represented ordinary situations, such as stooping to pick up a pencil. But these stimuli always referred to the self: "Feel: yourself stooping to pick up a pencil." Further, we obtained projected kinaesthetic imagery when the stimuli represented unusual situations and movements not likely to have been made by the *O*'s, such as walking a tight rope. But these stimuli referred always to someone else: "Feel: an acrobat walking a tight rope."

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[24] Cutaneous projections, which are usually referred to association, as when we say a thing looks cold or hard, may very well be, in the light of these results, projections at the level of perception. But we know hardly anything of cutaneous imagery.

We asked ourselves, then, which is the stronger influence upon resident and projected kinaesthesis, the influence of the usual and unusual, or the influence of the self and another? The natural thing to do was to reverse the situations, so that the usual should refer to another person, the unusual to oneself. We made out new paired commands which fulfilled this requirement.

We found that one of three things happened. (1) As a rule, the *O*'s reported resident kinaesthesis with the stimuli referring to the self, and projected kinaesthesis with the stimuli referring to the other person, in spite of the usualness or unusualness of the situation. (2) Sometimes the report was mixed, resident kinaesthetic imagery occurring for part and projected for part of the same situation. This mixture occurred with stimuli referring to the self making some strange movement. The thought of the movement would begin in resident kinaesthetic imagery, but the imagery would quite suddenly project itself without changing the meaning in any way except to make the self-reference uncertain. The *O* would say: "I think that was myself in the projected imagery, but I am not at all sure; it was a detached self." Every *O* who experienced this projected kinaesthesis in the midst of resident said: "I wonder if the projection happened there because I have never performed such a movement." Thus H, under the instruction "Feel: yourself making a hundred yard leap on a pair of skis," experienced the beginning of the leap in resident kinaesthesis, the movement through the air in projected kinaesthesis, and the landing in resident imagery. He said: "I have never leaped on a pair of skis, but I have made leaps, and that may be the reason that I had resident for the beginning and end of the leap." (3) Sometimes pure projected kinaesthesis resulted when the stimulus referred to the self performing a very strange feat.

We now had the reversal from resident to projected kinaesthesis for certain situations referring to the self in a strange situation; but we had not the corresponding reversal from projected to resident for usual situations referring to someone else. We thought that the failure to obtain the second reversal might be due to the fact that our stimuli for these experiments did not equal in familiarity the strangeness of the stimuli for the first reversal; *i. e.*, the *O*'s were *shocked* into projection by the strangeness of the one, and they must be *shocked* into resident imagery by the familiarity of the other. We therefore changed our situations, making them refer to someone else figuring in situations in which we had seen our *O*'s figure only a few days before. Immediately, and to the immense surprise of the *O*'s themselves, who had come to expect projection for all situations referring to other persons, resident kinaesthesis resulted.

We found, then, that the *O*'s could be shocked into projection in spite of the self-determination, and could be shocked into resident kinaesthesis in spite of the other-determination. The conditions thus cut across each other; ordinary usualness-unusualness is less strong a determinant than selfness-otherness, but a salient familiarity-strangeness, coming to the *O* with a shock of surprise, may reverse the normal conditioning of the two modes of imagery. It is plain that we are here upon the threshold of a highly complex problem, whose resolution will require methods more refined than those which we had at our disposal in the present study.

CONCLUSION

(1) Kinaesthetic images of memory may be distinguished from kinaesthetic sensations by uniformity, simplicity, and lack of 'body.' Kinaesthetic memory-images are uniform: always tiny bits of pressure, absolutely lacking in brightness. They are simple: they approximate single processes, and take on few or no perceptive characters. They lack 'body:' they are low in all the intensive attributes save vividness. Kinaesthetic sensations in a perception of movement are varied, complex, and have 'body;' they are now of one quality, now of another; they are sometimes high in all intensive attributes, sometimes high in only one, but always have more 'body' than the image; they constitute a sensory complex rich in all the perceptive characters.

(2) Kinaesthetic imagery may be classified under two main headings, resident and projected. The resident kinaesthetic images are referred ordinarily to oneself; the projected kinaesthetic images are referred ordinarily to someone else or something else. Resident kinaesthetic images may approximate simple processes, co-ordinate with sensation; projected kinaesthetic images constitute imaginal complexes, co-ordinate with perception. Resident kinaesthetic images show a psychological picture very different from the picture of kinaesthetic sensations in sensory complexes; projected kinaesthetic images in an imaginal complex show a psychological picture very like the picture of kinaesthetic sensations in a sensory complex. Resident kinaesthetic images are changing, fleeting, and involve an attitude which is passive, receptive, realizing; projected kinaesthetic images are invariable and persistent, and involve an attitude which is active, exploratory, detached, scrutinising.

(3) The difference between resident and projected kinaesthesis does not reflect merely the functional distinction of self and other; it is correlated with a specific difference of attitude on the part of the observer.

AFFECTIVE TENDENCY AS CONDITIONED BY COLOR AND FORM[1]

By Matsusaburo Yokoyama

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Introduction

This investigation studies the simultaneous effect of two aspects of a simple sensory material in conditioning affective tendency, and the nature of the affective resultant. The problem in its broader phase—in its bearing upon the question of summation of affections—is not new. Wundt,[2] in his analysis of affective fusion, declares that the characteristic of feeling lies in the fact that all feelings present in consciousness at a given moment tend to fuse into a unitary manifold, a *Totalgefühl,* which, nevertheless, is not a mere sum of its components, but has a unique property of its own. Külpe,[3] MacDougall[4] and Titchener,[5] on the other hand, hold that the affection of any given moment is the algebraical sum of the affections attaching to all the various sensory processes that constitute the mind at that moment.

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[1] From the Psychological Laboratory of Clark University. The writer wishes to acknowledge his indebtedness to Dr. L. R. Geissler for suggestions in the initiation of this research.

The present article has been condensed for purposes of publication by the omission of numerous tables that present special details or represent intermediate phases of the quantitative argument and by the abbreviation or elimination of much of the discussion. A complete bound manuscript, which gives the original data, the full numerical account and a complete discussion is available under this title in the Clark University Library and can be borrowed under the usual courtesies of library exchange.

Unfortunately, however, there seems, with a single exception,[6] to have been no systematic experimentation in support of either of these views. The question occurs, it is true, in various studies of color-harmony and of mixed feelings;[7] but these are too few in number and of too fragmentary a nature to warrant general psychological conclusions. The problem, as it stands to-day, like other problems in affective psychology, is still in controversy, pending solution by experiment.

PROCEDURE

The present work was conducted in the psychological laboratory of Clark University during the academic year 1919-1920.

Observers.—The observers in the experiment were B, Dr. E. G. Boring, who worked with knowledge of the problem; D, Dr. L. D. Boring; F, Dr. S. W. Fernberger; M, Miss M. Bates; P, Mr. C. C. Pratt. All but M were highly trained observers and had had experience in observation under the conditions of the experiment. M soon acquired introspective facility.

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[2] W. Wundt, *Physiologische Psychologie,* 6 Aufl., 1910, II, p. 351 ff.

[3] O. Külpe, *Outlines of Psychology,* trans., 3 ed., 1909, p. 264.

[4] W. MacDougall, *Physiological Psychology,* 1905, p. 80; *Body and Mind,* 3 ed., 1915, p. 313.

[5] E. B. Titchener, *Text-book of Psychology,* 1910, p. 258.

[6] L. R. Geissler, The Affective Tone of Color-Combinations, *Titchener Commemorative Volume,* 1917, 150-174. Geissler found that the pleasantness of color-pairs increases directly with the pleasantness of the colors taken individually.

[7] Jonas Cohn: Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten und ihrer Combinationen, *Philos. Stud.* 10, 1894, 562-603. C. H. Johnston: The Combination of Feelings, *Harvard Psychol. Stud.,* 2, 1906, 159-191. C. E. Kellogg: Alternation and Interference of Feelings, *Psychol. Mon.,* No. 79, 1905, 95 pp. A. Wohgemuth: Pleasure-Unpleasure, *Brit. J. Psychol. Mon. Supp.,* No. 6, 1919, 252 pp.

Stimuli.—The stimuli, except in Group II, were colored forms cut from the Milton-Bradley pigment papers and pasted on squares of neutral-gray cardboard, 14.5 x 17.5 cm. The list of stimuli and their designations follows:

Group I.
Rs: red square, 5 x 5 cm.
Os: orange square, 5 x 5 cm.
Ys: yellow square, 5 x 5 cm.
Gs: green square, 5 x 5 cm.
Cs: cyan (bluegreen) square, 5 x 5 cm.
Bs: blue square, 5 x 5 cm.
Ps: purple (redviolet) square, 5 x 5 cm.

Group II. Each of the following forms is drawn in black ink on a neutral-gray cardboard, 14.5 x 17.5 cm. The thickness of the line is about 0.5 mm.
Na: equilateral triangle, 7 cm. to a side, resting on its base.
Nb: circle, radius 2.8 cm.
Nc: annular sector, radius of larger arc, 7.3 cm.; of smaller arc, 2.5 cm.; placed convex upward; the two sides subtend an angle of 60 degrees.
Nd: regular pentagon, 3.8 cm. to a side, resting on one of its angles.
Ne: regular hexagon, 3.2 cm. to a side, resting on one of its angles.
Nf: rectangle, 6 x 4.15 cm., resting on its shorter side.
Ng: ellipse, major axis 3.5 cm.; minor axis 2.35 cm.; major axis horizontal.

Group III. 49 colored forms, *i. e.*, every color of Group I, combined with every form of Group II. Capitals designate the color (cf. Group II). Thus,
Ra, red triangle
Rb, red circle
Oc, orange sector
Yd, yellow pentagon
Cf, cyan rectangle
Pf, purple rectangle

The brightness of the colors and the gray cardboard (N) was determined by flicker photometry under the artificial daylight of a frosted 75-watt type C-2 Mazda lamp, 15 cm. distant. The average determinations for B and the writer, reading in both directions, and expressed in per cent of baryta white mixed with velvet black paper, are:

Colored paper	R	O	Y	G	C	B	P	N
White	11.7	32.3	91.2	46.5	39.5	12.9	19.5	38.1

Method and apparatus.—The method of paired comparisons was used throughout the entire course of experimentation.

The inapplicability of the method of choice for investigating the affective value of color has been shown by Cohn.[8] The value of the method of single exposures (*Reizmethode* after M. Brahn) has been questioned by Nakashima.[9] While the serial method (method of isolated exposures) has been highly recommended by Major,[10] it is not practicable in our research because of the non-serial nature of our stimuli. The method of paired comparisons was chosen, in spite of the objections of Gordon,[11] and Bullough[12] since it is the method which has been applied by Titchener,[13] Hayes,[14] Cohn,[15] Geissler,[16] and others with some degree of success in the investigation of affective problems.

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[8] Cohn, *op. cit.*, 564.

[9] T. Nakashima, *Amer. J. Psychol.*, 20, 1909, 180.

[10] D. R. Major, *Amer. J. Psychol.*, 7, 1895, 59.

Recently Barrett,[17] having tested experimentally the relative value of the order of merit method and the method of paired comparisons, came to the conclusion that the former is " vastly to be preferred to " the latter " from the standpoint of their relative demand upon the time and energy both of the experimenter and of the subjects." This conclusion, to be sure, is legitimate as far as it goes, but the use of the order of merit method is less desirable where scientific thoroughness is required. We tried this method for testing the preference of colors and forms on over 200 students of Wellesley and Clark Colleges, and found it quite unsatisfactory because of the observers' inability to comprehend the large number of stimuli, which inevitably led to various memorial confusions, as well as to neglect of areas beyond the visual range, and because of the tendency to make mediate judgments based upon reflection or upon previously established choices, *i. e.*, the tendency to be " logical." In addition to these defects, there were spatial errors, contrast effects, extraneous associations, *etc.*

A neutral gray cardboard screen, 120 x 57 cm. on a wooden framework was erected on a table 120 x 60 cm., 76 cm. high. In the center of the cardboard were two square openings, each 10.5 x 10.5 cm., placed side by side at a distance of 9.5 cm. The table on the side towards O was covered with neutral gray cloth which hung to the floor. On E's side of the screen an oblong board, 43 x 16 cm., was hinged to the wooden frame, 19 cm. above the table, in such a manner that it could be let down towards E and two stimulus cards arranged on it side by side with each card held in place by a triangular piece of tin into which one corner slipped. When this board was snapped up by a rubber band into the upright position the stimuli came directly behind the square openings. Hinged at the top of the screen and falling toward O was a neutral gray shutter, 45 x 27 cm., which completely covered the square openings. A string was attached to the shutter from behind so that it could be lifted by E, disclosing the stimulus without letting shadow fall upon it. A vernier chronoscope[18] was used for taking reaction times. It was connected with the exposure-apparatus in such a way that one of the pendulums was automatically released by an electromagnet when the shutter was raised. The other pendulum was similarly released when the reaction key was pressed by O.

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[11] K. Gordon, *Psychol. Rev.*, 19, 1912. 354.

[12] L. E. Bullough, *Brit. J. Psychol.*, 2, 1908, 411ff.

[13] Titchener, *Philos. Stud.*, 20, 1902, 382ff.

[14] S. P. Hayes, *Amer. J. Psychol.*, 17, 1906, 358-393.

[15] Cohn, *op. cit.*

[16] Geissler, *op. cit.*

[17] M. Barrett, *Psychol. Rev.*, 21, 1914, 278-294.

[18] *Cf.* E. C. Sanford, The Vernier Chronoscope, *Amer. J. Psychol.*, 9, 1897-1898, 191-197.

O sat at a distance of 1.5 m. from the screen. The experiment took place in a dark-room. Light was furnished by two 75-watt frosted type C-2 Mazda daylight lamps, about 75 cm. behind O and a little to the right and left.

The actual procedure was as follows. At the beginning of each experiment, the ready signal was given by E who sat in the rear of the screen and manipulated the apparatus. About two seconds later, E raised the shutter and presented a pair of stimuli for three seconds. O, seated comfortably in his chair, faced the screen and announced his judgment by reacting upon a key with his right hand and stating subsequently whether his judgment has been " left " or " right." Immediately after recording O's judgment and reaction time, E shifted the stimuli and set the chronoscope for the next exposure. An interval from twenty to thirty seconds was necessary.

The experiment was continued in this fashion for the first thirty-five minutes of each session, after which a short intermission was given. The remaining time was chiefly devoted to taking introspections. B, D and F wrote their reports; M and P dictated them. It was possible to obtain about fifty comparisons and one or more introspective reports in an hour.

Instructions.—The instructions finally adopted were typewritten and read by O at the beginning of each session.

Instruction I. (For the comparison of colors.)

" In each experiment after the ready signal two colors will be exposed for a short time in the square openings of the screen. You are to give yourself up entirely to color-quality. You are to assume the attitude of passively living the color. You are not to be critical toward the stimulus or to concern yourself with extrinsic references, but to take the color simply in its own right. *Immediately* after the colors are perceived, indicate the one which you prefer as the more pleasant by pressing the key and by saying the word ' left ' or the word ' right,' according as the left or the right color is preferred.

" Whenever you think you have failed to live up to this instruction, indicate that fact to the experimenter."

Instruction II. (For the comparison of forms.)

Same as Instruction I with " form " substituted for " color," and " form-aspect " substituted for " color-quality."

Instruction III. (For the comparison of color-forms.)

Same as Instruction I with "color-form" substituted for "color" and "color-quality."

When introspection was desired, the following instruction was given orally:

"In the next experiment, describe in detail your affective experience, with particular reference to the nature and mechanism of your judgment."

The observers were further instructed to indicate unambiguously cases of doubt, indecision, or of equality. D, F, M, and P were frequently unable to report upon the more pleasant stimulus, since each member of the pair was actually unpleasant; they were then allowed to base their preference upon the less unpleasant stimulus.

Order of experiment.—The experiment was divided into five parts. Parts I and V were planned to test the permanence of O's judgments upon colors and forms throughout the experimental period. The main experiment consisted of Parts II, III, and IV. Part II dealt with colors; Part III, with forms; and Part IV, with color-forms. The order for all O's but M is outlined below. M observed in the reversed order and did not participate in Parts I and V.

Part I.

Series S. The comparison of colors.

Instruction I. Stimuli, Group I (*q. v.*)
21 comparisons of colors in one spatial order, *i. e.*, each of the 7 colors compared with each of the 6 other colors, 7 x 6/2 = 21; and the repetiton of the same in reversed spatial order, 42 comparisons in all.

Series N. The comparison of forms.

Instruction II. Stimuli, Group II (*q. v.*)

21 comparisons of forms in one spatial order, *i. e.*, each of the 7 forms compared with each of the 6 other forms, 7 x 6/2 = 21; and the repetiton of the same in reversed spatial order, 42 comparisons in all.

Part II. The comparison of colors. Identical with Part I, Series S, except as to stimuli.
Instruction I. Stimuli, Group III (*q. v.*)

Series				**Stimuli**			
a	**Ra**	**Oa**	**Ya**	**Ga**	**Ca**	**Ba**	**Pa**
b	**Rb**	**Ob**	**Yb**	**Gb**	**Cb**	**Bb**	**Pb**
c	**Rc**	**Oc**	**Yc**	**Gc**	**Cc**	**Bc**	**Pc**
d	**Rd**	**Od**	**Yd**	**Gd**	**Cd**	**Bd**	**Pd**
e	**Re**	**Oe**	**Ye**	**Ge**	**Ce**	**Be**	**Pe**
f	**Rf**	**Of**	**Yf**	**Gf**	**Cf**	**Bf**	**Pf**
g	**Rg**	**Og**	**Yg**	**Gg**	**Cg**	**Bg**	**Pg**

21 comparisons in each of 2 spatial orders for each series; 2 x 21 = 42 comparisons in each series, 7 x 42 = 294 comparisons in all

Part III. The comparison of forms. Identical with Part I, Series N, except as to stimuli.

Instruction II. Stimuli, Group III.

Series	Stimuli						
R	Ra	Rb	Rc	Rd	Re	Rf	Rg
O	Oa	Ob	Oc	Od	Oe	Of	Og
Y	Ya	Yb	Yc	Yd	Ye	Yf	Yg
G	Ga	Gb	Gc	Gd	Ge	Gf	Gg
C	Ca	Cb	Cc	Cd	Ce	Cf	Cg
B	Ba	Bb	Bc	Bd	Be	Bf	Bg
P	Pa	Pb	Pc	Pd	Pe	Pf	Pg

21 comparisons in each of 2 spatial orders for each series; 2 x 21 = 42 comparisons in each series, 7 x 42 = 294 comparisons in all.

Part IV. The comparison of color-forms.

Instruction III. Stimuli, Group III. Series 1 to 24 inclusive.

1176 comparisons of color forms in one spatial order only, *i. e.*, each of the 49 color-forms compared with each of the other color-forms, 49 x 48/2 = 1176. Series 1 and 2 consisted of 42 comparisons each; series 3, of 63; and each of the rest, of 49.

Part V.

Series S′ = Repetition of Part I, Series S.
Series N′ = Repetition of Part I, Series N.

The stimuli were presented in haphazard order, with the precaution that no stimulus should appear twice in succession. The same stimuli, differently arranged, were used in Parts II, III, and IV. Thus, the isolation of color (Part II) and of form (Part III) was accomplished solely by the direction of attention. It would, of course, be ideal if we were able to present formless colors and colorless forms for the experiments on colors and forms. The use of the same stimuli in all parts of the main experiment did not, however, introduce any measurable disadvantage in the investigation, for the observers acquired skill in isolating under attention the color or form from the color-form and in attending to the one or the other separately.

Affective Judgment under the Method of Paired Comparisons

At the outset of this investigation, we had no definite knowledge regarding the practicability of the method of paired comparisons in dealing with our particular problem, nor did we have any exact conception of the nature of affections induced under this method. The choice of the method was quite arbitrary; it was, however, the only purely psychological method available, the only method that experimentalists might agree upon as furnishing the conditions or definition of the generally conceded affective process. We found, as have our predecessors, that the results obtained with this method are of a positive character as far as their quantitative aspect is concerned, and that pleasantness and unpleasantness are not only reportable, but that judgments upon them may be nearly as immediate as judgments upon sensory impressions. Nevertheless the observers worked under very different attitudes and, so far as their introspective reports are admissible as evidence, we found their judgments based upon different sorts of psychological processes. This divergence in type of judgment led to less inconsistency in the nature of the data than might be expected, although the introduction of a critical introspective attitude undoubtedly accounts for the instability of some of the results. A characterization of these differences is in place here, since it reflects the limitations of the method; a critical study of the introspections which show these differences must, however, wait for a later article.

In a preliminary survey of the introspective data of our four practiced subjects, it appears that the affective judgments may be of two distinct types which depend upon two different attitudes,—a sensorial attitude and an objective attitude. The sensorial attitude presumably grew out of an instruction for introspection. Either the observers understood the instruction that they were sometimes to report introspectively upon the judging process to mean that they were to judge processes, or else the general introspective habits of the laboratory operated toward a similar self-instruction. The fact seems to be that the observers who sought to base their judgments upon introspectable material tended actually to base them upon sensory process, and that this introspective attitude is actually a sensorial attitude. Observer B's judgments were of this type. He approached the experiment with a firm conviction that if the affection is an existential mental process, it must be directly observable by introspection and his attitude throughout the experiment was characteristically introspective. He found, however, that pleasantness and unpleasantness were for him nonexistential in this psychological sense, that they were always 'meanings' or conscious attitudes based definitely under these particular conditions upon some particular sensory pattern. In order to make an affective judgment, however, it was always necessary for him to have some essential sensory cue, and his reaction-times, which were comparatively long, reflect this intervention of a sensory mediator between the perception of the stimulus and the arousal of the judgment. D also worked in the sensorial attitude, characterizing affections as meanings carried by organic sensations. P reported them as observable psychological elements, but concomitant with organic and kinaesthetic processes. F, on the other hand, approximated the objective attitude, under which the stimulus is focal and introspected process is of secondary account. This attitude is in the spirit of the major instructions and F followed these instructions uncritically and was very receptive. His judgments after a little practice became mechanized and quick, as might have been expected, since there appeared to be no process-intermediary to retard the course. Although he became, during the course of the experiment, convinced that judgments of preference might be made without the presence of either of the affective qualities, he believed, nevertheless, that pleasantness and unpleasantness do exist as mental elements.

These results are especially suggestive in so far as they throw light upon the question of the psychological status of affection and the manner in which the problem of meaning enters into the problem of feeling. It is nothing new for observers in the same experiment to disagree over the nature of affection,[19] and the applicability of the introspective method is seriously open to question. Nevertheless the problem needs to be faced in the light of protocols obtained from observers trained in the introspective method and with their wits keen to note the incursions of meaning and *Kundgabe* upon the field of existential process. This issue remains; it is not immediately involved in the present problem, and it seems preferable to reserve it for separate treatment later.

It has seemed advisable tentatively to adopt the term "affective tendency" in this study because the entities involved (number of preferences) are statistical data and not immediately given and because the conceptual bases of the affective judgment must necessarily be as little directly observable as are the associative and determining tendencies. The difficulties and divergences of our observers emphasize again the elusiveness of the immediately observable affective datum, and the experiment as a whole brings to the fore what seems to be the available material at the bottom of the experimentalist's belief in affection, *viz.*, the statistical relative frequencies of preferences. And the term tendency, so it seems, is used by psychologists to represent those mental entities that appear thus in statistical settings.

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[19] Nakashima, Contributions to the Study of the Affective Processes, *Amer. J. Psychol.*, 20, 1909, 180ff.

TABLE I

PERMANENCY OF AFFECTIVE JUDGMENT

Percents of agreement between early series (Part I; S, N) and late series (Part V; S^1, N^1). S and S^1 are for colored squares; N and N^1 for "neutral" forms. Obs.: B, D, F, P. Colors O and P omitted in "B (revised.)"

	B	D	F	P	B (Revised)
S & S^1	64.3	90.5	100.0	92.9	90.0
N & N^1	90.5	95.2	100.0	88.1	90.5

Degree of Permanency of Affective Judgment (Results of Parts I and V; Instructions I and II.)

Table I indicates in terms of percentages of agreement,[20] the degree of permanence of O's affective judgments on colors and on forms just before (Part I) and after (Part V) the main experiment. Inspection will show clearly that, except in case of B, the influence of time,—as might be expected from the result of Bradford,[21]—is too small to change the preferential order of colors and forms. The low degree of consistency of B's judgments on colors is due apparently to a change in his preference for purple and orange during the progress of experiment. The omission of orange and purple from calculation of B's results gives a much higher percentage of agreement for colors without causing any change for forms (Table I, last column).

Of this change B remarked: "Purple became more pleasant than it used to be. I liked it better than red." Since the objective conditons of experiment have been kept fairly constant throughout and since no other salient cause is discoverable, we are justified in attributing B's change to the relative inconstancy of his attitude during the long interval of time. On the other hand, having shown that the degree of consistency of our observers' affective judgments in general is high and that therefore the preferential order for colors and for forms is not the result of transient conditions, we may safely conclude that the affective tendency of colors and forms can be determined psychophysically in experimental series extending over as long a period as that of our main experiment (Parts II, III, IV).

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[20] See W. S. Foster and K. Roese, Tridimensional Theory of Feeling from the Standpoint of Typical Experiences, *Amer. J. Psychol.*, 27, 1916, 161, for the calculation of these. Our "per cent of agreement" is obtained by multiplying their 'degree of similarity' by 100.

[21] E. J. C. Bradford, A Note on the Relation and Aesthetic Value of the Perceptive Types in Color Appreciation, *ibid.*, 24, 1913, 545-554.

TABLE II

Mutual Independence of Color and Form: Obs. B.

Upper right half of the table shows percents of agreement for colors between every pair of forms; attention on color (Instruction I.) Lower left half of the table shows percents of agreement for forms between every pair of colors; attention on form (Instruction II.)

Av. for Color = 80.3; M.V. = 5.0
COLOR

		b	c	d	e	f	g	Form
		88.1	81.0	78.6	69.0	88.1	78.6	a
O	90.5		88.1	85.7	71.4	88.1	78.6	b
Y	90.5	90.5		81.0	69.0	81.0	73.8	c
G	88.1	90.5	92.9		71.4	90.5	81.0	d
C	88.1	90.5	92.9	100.0		76.2	88.1	e
B	81.0	85.7	90.5	95.2	95.2		85.7	f
P	88.1	95.2	88.1	90.5	90.5	85.7		
Color	R	O	Y	G	C	B		

FORM
Av. for form = 90.5; M. V. = 2.7

TABLE III

Mutual Independence of Color and Form: Obs. D

See Legend of Table II

Av. for color = 90.7; M. V. = 3.2
COLOR

		b	c	d	e	f	g	Form
		95.2	95.2	92.9	85.7	95.2	85.7	a
O	85.7		95.2	95.2	88.1	90.5	88.1	b
Y	95.2	83.3		92.9	90.5	95.2	90.5	c
G	90.5	81.0	85.7		83.3	90.5	88.1	d
C	95.2	85.7	95.2	85.7		85.7	92.9	e
B	90.5	81.0	85.7	90.5	85.7		88.1	f
P	90.5	90.5	85.7	90.5	85.7	90.5		
Color	R	O	Y	G	C	B		

FORM
Av. for form = 88.1; M. V. = 3.6

TABLE IV

Mutual Independence of Color and Form: Obs. F.

See Legend of Table II

Av. for color = 94.8; M. V. = 2.2
COLOR

		b	c	d	e	f	g	Form
		95.2	90.5	95.2	90.5	100.0	100.0	a
O	90.5		95.2	95.2	95.2	95.2	95.2	b
Y	95.2	95.2		95.2	100.0	90.5	90.5	c
G	90.5	90.5	95.2		95.2	95.2	95.2	d
C	95.2	85.7	90.5	85.7		90.5	90.5	e
B	95.2	95.2	100.0	95.2	90.5		100.0	f
P	95.2	95.2	100.0	95.2	90.5	100.0		
Color	R	O	Y	G	C	B		

FORM
Av. for form = 93.7; M. V. = 3.3

TABLE V

Mutual Independence of Color and Form: Obs. M.

See Legend of Table II

Av. for color = 82.0; M. V. = 5.5
COLOR

		b	c	d	e	f	g	Form
		76.2	71.4	81.0	76.2	76.2	81.0	a
O	90.5		85.7	95.2	81.0	88.1	90.5	b
Y	71.5	76.2		85.7	76.2	76.2	76.2	c
G	76.2	76.2	66.7		85.7	85.7	90.5	d
C	66.7	76.2	90.5	76.2		90.5	76.2	e
B	76.2	85.7	71.4	85.7	76.2		81.0	f
P	81.0	88.1	76.2	81.0	76.2	90.5		
Color	R	O	Y	G	C	B		

FORM
Av. for form = 78.8; M. V. = 6.0

TABLE VI

MUTUAL INDEPENDENCE OF COLOR AND FORM: OBS. P.

See Legend of Table II

Av. for color = 91.8; M. V. = 3.4
COLOR

		b	c	d	e	f	g	Form
		97.6	81.0	90.5	92.9	97.6	92.9	a
O	85.7		85.7	92.9	95.2	100.0	92.9	b
Y	95.2	90.5		92.9	90.5	85.7	88.1	c
G	90.5	92.9	88.1		92.9	92.9	85.7	d
C	95.2	90.5	90.5	95.2		95.2	92.9	e
B	90.5	95.2	95.2	92.9	95.2		92.9	f
P	90.5	90.5	85.7	95.2	95.2	90.5		
Color	R	O	Y	G	C	B		

FORM
Av. for form = 91.9; M. V. = 2.7

TABLE VII

MUTUAL INDEPENDENCE OF COLOR AND FORM: REVISED RESULT OF OBS. B.

See Legend of Table II. Colors O and P omitted because of inconstancy.

Av. for color = 86.7; M. V. = 5.6
COLOR

		b	c	d	e	f	g	Form
		95.0	85.0	90.0	80.0	95.0	95.0	a
Y	90.5		85.0	90.0	85.0	90.0	100.0	b
G	88.1	92.9		85.0	70.0	85.0	85.0	c
C	88.1	92.9	100.0		75.0	90.0	90.0	d
B	81.0	90.5	95.2	95.2		75.0	85.0	e
							90.0	f
Color	R	Y	G	C				

FORM
Av. for form = 91.4; M. V. = 3.8

Mutual Independence of Color and Form in Conditioning Affective Tendency of Color and Form respectively under Attentive Isolation (Results of Parts II and III, Instructions I and II.)

The results of Parts II and III are summarized in Tables II-VII. Each number in the upper right half of these tables is a percentage of agreement between the preferential order of colors of one form and the preferential order of the same colors with another form. The numbers in the lower left half are percentages of agreement of the preferential order of forms for pairs of colors. For example, in Table II, the figure 88.1 below 'f' and opposite 'a' is the percentage of agreement between the order of colors of series f (colors of form f) and the order of colors of series d (colors of form d); 90.5 above "R" and opposite "O" in the same table is the agreement between the preferential order of forms colored R and the order of forms colored O.

The percentages of agreement between the various series for both color and form are very high, ranging from 61.9 to 100.0 in case of the former, and from 66.7 to 100.0 in the latter. The averages of all the observers are 87.9 for color and 88.1 for form with the M. V's 3.9 and 3.7 respectively. These agreements are high enough to warrant a conclusion that when color-forms are presented for affective comparison of colors only, the forms have practically no influence upon the preferential order of these colors, provided the observer's attitude remains constant throughout the task, and that color similarly has practically no effect upon form. Color and form, then, may be said to be each independent of the other in conditioning affective tendency where each is isolated from the other by attentive abstraction.

This conclusion follows even in the face of relatively low degree of consistency of M's affective judgments on color and form and of B's on color. In B's case, as we have noted, purple and orange proved always the disturbing factors in his otherwise regular preferences. The M. V's of the scores of these colors were each greater than one-fourth of their averages. The inconsistency is plainly conditioned upon the nature of these two colors and not in any way upon form. (See Table VII, 'revised result' for B.) The case of M is of more significance, as these experiments were performed after she had completed 1176 comparisons of color-forms, and she was therefore expected to have a fixed set of preferential orders. However, there is no reason to assume that her failure to attain a higher degree of consistency is to be attributed to the mutual influence of color and form, exhibited by no other observer; rather it is reasonable to believe that, being untrained in psychological experimentation, she misunderstood the instructions, attempting to secure speed rather than accuracy. Moreover, it may be that she worked with various criteria of pleasantness, without sufficient discrimination, and thus gave inconstant results. These indications are borne out by her extraordinarily short reaction times and her frequent premature judgments, as well as by her own introspections, in which not infrequently are found such sentences as: "I chose the right in spite of myself," "the left was so impressive that I judged it as more pleasant, though I knew I liked the right better." It is also pertinent to observe that she had no decided preference among the forms as she did among the colors, and that the degree of consistency for forms is therefore even lower than for colors.

TABLE VIII

Affective Values of Color-Forms: Obs. B.

Number of preferences under method of paired comparisons for every colored form with attention on color-form (Instruction III). Rows show comparisons of forms for every color; columns show comparisons of colors for every form.

	a	b	c	d	e	f	g	Av.	M.V.
R	32.5	48.0	29.5	35.5	38.0	37.5	42.0	37.6	4.37
O	10.0	24.5	1.5	11.5	25.5	10.5	19.5	14.7	7.24
Y	17.0	41.0	17.0	25.5	27.0	15.5	29.5	24.6	6.98
G	24.5	36.0	25.0	28.0	34.5	28.5	36.5	30.4	4.50
C	2.5	25.0	3.5	7.0	11.5	3.0	10.0	8.9	5.68
B	14.5	39.0	18.5	24.5	30.0	17.5	34.0	25.4	7.64
P	20.5	38.0	13.0	25.0	31.0	21.0	35.5	26.3	7.33
Av.	17.3	35.8	15.4	22.4	28.2	19.1	29.6		6.25
M. V.	7.26	6.45	8.08	7.53	5.90	8.51	8.49	7.46	

TABLE IX

Affective Values of Color-Forms: Obs. D.

See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	35.0	35.0	41.0	41.0	38.0	33.0	37.5	37.2	2.47
O	8.0	1.0	18.0	16.0	11.5	0.0	2.0	8.1	6.08
Y	15.5	7.0	30.0	28.0	22.0	4.0	7.0	16.2	8.96
G	12.0	8.0	28.0	25.0	18.0	6.5	11.0	15.5	7.00
C	25.0	21.0	34.0	33.5	27.0	16.0	23.0	25.6	5.02
B	45.0	42.0	48.0	46.0	45.0	42.0	43.0	44.5	1.79
P	25.0	15.5	32.0	31.0	20.0	7.0	16.0	20.9	7.20
Av.	23.6	18.5	33.0	31.5	25.9	15.5	19.9		5.50
M.V.	10.12	12.14	6.86	7.43	9.20	12.71	12.49	10.14	

TABLE X

Affective Values of Color-Forms: Obs. F.

See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	38	37	47	35	31	19	43	35.7	6.33
O	32	27	44	27	22	14	42	30.0	8.29
Y	21	19	37	14	11	7	31	19.7	8.24
G	6	2	13	4	1	0	7	4.7	3.39
C	16	14	27	12	8	7	25	15.7	6.10
B	41	40	48	37	33	23	46	38.3	6.24
P	26	24	38	21	15	10	33	23.9	7.31
Av.	25.7	23.4	36.1	21.6	17.3	11.4	32.4		6.56
M.V.	9.76	9.94	9.37	11.37	9.76	6.20	9.80	9.46	

TABLE XI

Affective Values of Color-Forms: Obs. M.

See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	29.0	45.0	36.0	41.0	41.0	27.0	43.0	37.4	5.80
O	13.0	24.0	19.0	18.0	27.5	17.5	24.5	20.5	4.14
Y	10.0	35.0	28.0	24.0	26.0	17.0	32.0	24.6	6.49
G	10.0	12.0	6.5	3.5	11.0	5.0	20.0	9.7	4.04
C	14.5	25.0	14.5	13.0	11.5	7.5	15.0	14.4	3.22
B	34.0	47.0	44.0	40.0	42.0	32.0	44.0	40.4	4.37
P	12.5	30.0	26.0	24.0	22.5	10.5	21.0	20.9	5.39
Av.	17.6	31.1	24.9	23.4	25.9	16.6	28.5		4.78
M.V.	7.96	9.59	9.88	10.16	9.37	7.69	9.57	9.17	

TABLE XII

Affective Values of Color-Forms: Obs. P.

See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	27.5	34.0	35.0	35.0	45.0	16.5	47.0	34.3	7.10
O	24.0	28.0	29.0	36.0	39.0	17.0	44.0	31.0	7.43
Y	21.0	23.0	23.0	32.0	36.0	17.0	40.0	27.4	7.35
G	6.5	5.0	6.5	8.5	10.0	2.5	13.5	7.5	2.79
C	7.5	8.5	8.5	8.5	11.0	1.0	17.5	8.9	3.04
B	33.0	38.0	37.0	40.0	46.0	27.0	47.0	38.3	5.18
P	19.0	15.0	16.5	23.0	30.0	10.5	30.0	20.6	6.08
Av.	19.8	21.6	22.2	26.7	31.0	13.1	34.1		5.57
M.V.	7.53	10.41	10.04	10.89	12.00	7.20	11.84	9.99	

TABLE XIII

AFFECTIVE VALUES OF COLOR-FORMS: REVISED RESULT OF OBS. B

See legend of Table VIII. Colors O and P omitted because of inconstancy.

	a	b	c	d	e	f	g	Av.	M.V.
R	22.5	34.0	19.0	23.5	26.0	24.5	29.0	25.5	3.21
Y	11.5	28.5	9.0	16.5	17.0	10.5	19.5	16.1	4.92
G	15.5	25.5	17.0	19.0	25.0	20.5	25.0	21.1	3.51
C	1.5	16.5	1.0	4.0	7.0	2.5	6.5	5.6	3.80
B	9.5	27.5	11.0	17.5	20.0	8.5	23.5	16.8	6.10
Av.	12.1	26.4	11.4	16.1	19.0	13.3	20.7		4.31
M.V.	5.52	4.32	5.48	4.84	5.60	7.36	6.16	5.61	

Concurrent Operation of Color and Form upon Affective Tendency (Parts II, III and IV).

Affective values of color-forms. (Part IV; Instruction III.) —Tables VIII-XIII inclusive give the result of Part IV, the experiment upon the forty-nine stimuli with attention upon "color-form." In each table the figures in the first seven rows and columns are the absolute frequencies of choice made upon these color-forms by each observer. Further, these tables are so arranged that each row contains the scores of seven color-forms, different from one another in form and identical in color, and thus show implicitly the preferential order of seven forms, all of the same color; while, conversely, each column exhibits the preferential order of seven colors, all in the same form. The averages and M.V's are respectively placed in the rows and columns so named. For example, let us take Table VIII (Obs. B). The figures 32.5, 48, 29.5, *etc.,* respectively under a, b, c, *etc.,* in the row R indicate the scores of the color-forms, Ra, Rb, Rc, *etc.,* establishing the preferential order of the forms, a, b, c, *etc.,* for the color red. Likewise, the figures, 48, 24.5, 41, *etc.,* respectively opposite R, O, Y, *etc.,* in the column b may be taken either as the number of choices of the color-forms, Rb, Ob, Yb, *etc.,* or as indicative of the preferential order of the colors, R, O, Y, *etc.,* in the form b, the circle.

Effect of instruction upon preference. (Comparison of Parts II and III with IV.)—In order to measure the influence of the direction of attention upon affective tendency, the preferential orders of colors with attention directed upon color only (Part II, Instruction I) and of forms with atten-

TABLE XIV

Effect of Direction of Attention

Correlation for colors between ranks with attention on color-quality (Instruction I) and ranks with attention on color-form (Instruction III). " Av." = av. correlation and av. P.E. " M. V." = M.V. of " Av." " Correl. Av." = correlation of av. of all forms for every obs. " B (revised)" = B's results with colors O and P omitted.

Obs.	B		D		F		M		P		Av. for All obs.		B (revised)	
Form	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.
a	0.86	0.07	0.92	0.04	1.00	0.00	0.43	0.22	0.96	0.02	0.83	0.07	0.88	0.07
b	0.56	0.17	0.88	0.06	1.00	0.00	0.87	0.07	0.99	0.01	0.86	0.06	0.55	0.22
c	0.90	0.05	0.99	0.01	0.96	0.02	0.93	0.04	0.98	0.01	0.95	0.02	0.90	0.06
d	0.46	0.21	0.94	0.03	0.99	0.01	0.90	0.05	0.94	0.03	0.85	0.07	0.88	0.07
e	0.64	0.16	0.94	0.03	0.96	0.02	0.74	0.12	1.00	0.00	0.86	0.07	0.90	0.06
f	0.63	0.16	0.85	0.08	0.99	0.01	0.81	0.09	0.83	0.08	0.82	0.08	0.90	0.06
g	0.67	0.15	1.00	0.00	1.00	0.00	0.88	0.06	0.93	0.04	0.90	0.05	0.95	0.03
Av.	0.68	0.14	0.93	0.03	0.99	0.01	0.89	0.09	0.95	0.03	0.87	0.06	0.85	0.09
M. V.	0.12		0.04		0.01		0.12		0.04		0.03		0.09	
Correl. Av. . . .	0.74	0.12	1.00	0.00	1.00	0.00	0.96	0.02	1.00	0.00	0.94	0.03	0.90	0.06

TABLE XV

Effect of Direction of Attention

Correlation for forms between ranks with attention on form-aspect (Instruction II) and ranks with attention on color-form (Instruction III). " Av." = av. correlation and av. P.E. " M. V." = M.V. of " Av." " Correl. Av." = correlation of av. of all colors for every obs. " B (revised)" = B's results with colors O and P omitted.

Obs.	B		D		F		M		P		Av. for All obs.		B (revised)	
Color:	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.	ρ	P.E.
R	0.89	0.05	0.94	0.03	1.00	0.00	0.26	0.25	0.92	0.04	0.80	0.04	0.90	0.05
O	0.86	0.07	0.96	0.02	0.94	0.03	0.56	0.18	0.89	0.05	0.84	0.07	. . . .	. . . .
Y	0.88	0.06	0.93	0.04	0.99	0.01	0.39	0.23	0.96	0.02	0.83	0.07	0.96	0.02
G	0.89	0.05	0.96	0.02	0.89	0.05	0.21	0.26	0.88	0.06	0.77	0.09	0.92	0.04
C	0.86	0.07	0.96	0.02	1.00	0.00	0.19	0.26	0.96	0.02	0.79	0.07	0.96	0.02
B	0.82	0.09	0.98	0.01	0.99	0.01	0.62	0.17	0.99	0.01	0.88	0.06	0.75	0.12
P	0.96	0.02	0.96	0.02	0.99	0.01	0.77	0.11	0.80	0.10	0.90	0.05	. . . .	. . . .
Av.	0.88	0.05	0.95	0.02	0.97	0.02	0.43	0.21	0.92	0.03	0.83	0.07	0.90	0.03
M. V.	0.03		0.02		0.03		0.19		0.05		0.04		0.06	
Correl. Av. . . .	1.00	0.00	1.00	0.00	1.00	0.00	0.46	0.21	0.96	0.02	0.89	0.05	1.00	0.00

tion directed upon form only (Part III, Instruction II) were compared by the method of rank-differences with the results for color and form respectively with attention upon 'color-form' (Part IV, Instruction III). The correlations are indicated in Tables XIV (color) and XV (form).[22] In table XIV, 0.90 under " ρ " and opposite " c," for instance, is the coefficient of correlation between the preferential orders of colors of the form c obtained in Part II and in Part IV; the figure 0.05 adjacent to it is the probable error of this ρ. Similarly, in Table XV, the figure 0.89 under “ ” and opposite “R” is the coefficient of correlation between the preferential orders of forms of the color red obtained in Part II and in Part IV, *etc.*

Examination of these tables reveals the fact that the coefficients of correlation, whether they be between the preferential orders of clors or of forms, are high for the observers D, F, and P, never falling below 0.80 and never with the P.E. exceeding one tenth of ρ. Indeed, for these observers, the correlation is perfect or nearly so when computed for the average ranks of colors or of forms. (Table XIV, row ‘Correl. Av.’, and Table XV, row ‘Correl. Av.’.) With B (color) and M (both color and form), the correlation is less significant than with the other observers.

It might be argued *a priori* that the poor result of M is due mainly to the fact that she had worked with the color-forms before she worked with the separate colors and forms, and that thus her judgments of the color-forms were relatively independent of the affective tendencies intrinsic to the colors and forms. Since Geissler, however, has already proved in his investigation of the influence of the affective tone of single colors on the affective tone of their combinations that there is no difference in the actual result whether the individual colors are presented before or after the color-pairs,[23] the writer is inclined to question the validity of any such claim. The more salient causes for the instability of her judgments seem to lie in the precarious nature of her attitude and her apparent lack of decided preference for any particular forms (see above). B's low correlation for the colors (0.77) results from the inconstancy of his judgments with the orange and purple stimuli. The recalculation of his data after eliminating the judgments on all the orange and purple stimuli raises the coefficient of correlation (Tables XIV and XV, last column) greatly between Part II and IV and between Part III and IV.

Taken all in all, there is a high degree of correspondence between the results of Parts II and III, and Part IV for all the observers, thus indicating that color and form operate as affective conditions independently of the direction of attention.

Degree of independence of color and form in conditioning affective tendency of color-form.—The failure to attain per-

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[22] The formulae used were: $\rho = 1 - \frac{6 \Sigma D^2}{n(n^2-1)}$, and

P.E.$_\rho = 0.706 \frac{1-\rho^2}{n}$.

[23] Geissler, *op. cit.*

fect correlations in the foregoing analysis may be attributed either to fluctuations of the observers' attitude under a given *Aufgabe,* or to the mutual effect of color and form upon the preferential order for each when the two are combined and judged as color-forms. Accordingly, if for the sake of simplicity we ignore, for a moment, the first of these factors (although there is every reason to believe that it was an important cause in effecting the inconstant results) and consider the data as indicative of mutual effects only, then the coefficients of correlation may be looked upon as the indices of the degrees of independence of color and form, one from another, as they condition the affective tendencies of the color-forms; and, conversely, the differences of these coefficients from unity may be taken as the measure of the degree of their mutual effect. It may be readily seen in Tables XIV and XV that, except in the cases of B and M, the effect is not very marked (ave. effects of forms on colors = 1.00 — 0.87 = 0.13, of colors on forms = 1.00 — 0.83 = 0.17). The variations (M.V's) between the effects of different colors and forms are comparatively large; yet no definite relation can be found between the relative effects of these colors and forms, and the relative intensities of their own affective tendencies. It is doubtful whether these variations are at all the result of interaction of color and form. That they might easily be due to vacillations in the observer's attitude appears from the fact that they vary from observer to observer according to the order of the inconstancy of attitude. (See analysis of Parts I, II and III). At any rate we may conclude that as far as relative pleasantness is concerned, color and form are, in the main, mutually independent in conditioning affective tendency of color-form.

TABLE XVI

Relative Effectiveness of Separate Colors in Conditioning Affective Tendency of Color-Form

Inversely measured by M. V. of different forms for every color. Values taken from last columns, Tables VIII-XIII. Rank orders of effectiveness in parentheses.

Obs.:		B	D	F	M	P	Av. for All obs.	B (revised)
For the 7 forms in:	R	4.37 (1)	2.47 (2)	6.33 (4)	5.80 (6)	7.10 (5)	5.21 (4)	3.21 (1)
	O	7.24 (5)	6.08 (4)	8.29 (7)	4.14 (3)	7.43 (7)	6.64 (5)	....
	Y	6.98 (4)	8.96 (7)	8.24 (6)	6.49 (7)	7.35 (6)	7.59 (7)	4.92 (4)
	G	4.50 (2)	7.00 (5)	3.39 (1)	4.04 (2)	2.79 (1)	4.34 (1)	3.51 (2)
	C	5.68 (3)	5.02 (3)	6.10 (2)	3.22 (1)	3.04 (2)	4.61 (2)	3.80 (3)
	B	7.64 (7)	1.79 (1)	6.24 (3)	4.37 (4)	5.18 (3)	5.04 (3)	6.10 (5)
	P	7.33 (6)	7.20 (6)	7.31 (5)	5.39 (5)	6.08 (4)	6.66 (6)	....
	Av.	6.25	5.50	6.56	4.78	5.57	5.73	4.31

TABLE XVII

Relative Effectiveness of Separate Forms in Conditioning Affective Tendency of Color-Form

Inversely measured by M.V. of different colors for every form. Values taken from last rows, Tables VIII-XIII. Rank orders of effectiveness in parentheses.

Obs.:		B	D	F	M	P	Av. for All Obs.	B (revised)
For the 7 colors in:	a	7.26 (3)	10.12 (4)	9.76 (4)	7.96 (2)	7.53 (2)	8.53 (2)	5.52 (4)
	b	6.45 (2)	12.14 (5)	9.94 (6)	9.59 (5)	10.41 (4)	9.51 (6)	4.32 (1)
	c	8.08 (5)	6.86 (1)	9.37 (2)	9.88 (6)	10.04 (3)	8.85 (3)	5.48 (3)
	d	7.53 (4)	7.43 (2)	11.37 (7)	10.16 (7)	10.89 (5)	9.48 (5)	4.84 (2)
	e	5.90 (1)	9.20 (3)	9.76 (3)	9.37 (3)	12.00 (7)	9.25 (4)	5.60 (5)
	f	8.51 (7)	12.71 (7)	6.20 (1)	7.69 (1)	7.20 (1)	8.46 (1)	7.36 (7)
	g	8.49 (6)	12.49 (6)	9.80 (5)	9.57 (4)	11.84 (6)	10.44 (7)	6.16 (6)
Av.		7.47	10.14	9.46	9.17	9.99	9.24	5.61

Dominance of color or form as independently conditioning affective tendency of color-form.—Let us inquire next whether color or form is more dominant in independently conditioning the affective tendency of color-form, and further, as to the relative effectiveness of separate colors and forms in such an operation.

It is evident that if the colors and forms were equally effective, the scatter of the scores of the forms in each color would tend to be equal to that of the colors in each form; and, if colors and forms were unequally effective, the scatter would be different. Again, if any color or form were more effective than the others, the scores of the forms of that color or of the colors of that form would cluster more closely together than the scores of the forms of any other colors or of the colors of any other forms. Obviously, therefore, the effectiveness of each of the seven colors is measured by the M.V. of the scores of the seven forms used in that color. A scale of the relative effectivness of these colors may be based upon the comparison of these M.V's, with effectiveness inversely proportional to the M.V's. In the same way, the effectiveness of each of the seven forms can be measured in terms of the M.V. of the scores of the seven colors used in that form, and a scale similarly constructed for them.

These M.V's have been computed and are indicated in the right-hand columns and in the bottom rows of Tables VIII-XIII. They are reproduced in Tables XVI and XVII with the ranks (in parentheses) of the colors and forms according to their relative effectiveness. On the whole, the colors thus appear more effective than the forms in determining the affective tendencies of the color-forms.

The relative effectiveness of every color and form (Tables XVI and XVII) has been compared with its relative pleasantness (Part II, Instruction I, and Part III, Instruction II) with the following result:

The first three most effective colors are for the observer:
- B, the 2 most pleasant and the 1 least pleasant colors.
- D, the 2 most pleasant and the 1 moderately pleasant colors.
- F, the 1 most pleasant and the 2 least pleasant colors.
- M, the 1 most pleasant and the 2 least pleasant colors.
- P, the 1 most pleasant and the 2 least pleasant colors.

The first three most effective forms are for the observer:
- B, the forms which rank 3, 1, and 6.
- D, the 3 most pleasant forms.
- F, the 1 most pleasant and the 2 least pleasant forms.
- M, the 1 most pleasant and the 1 least pleasant and 1 moderately pleasant forms.
- P, the 3 least pleasant forms.

In other words the extremes, *i.e.* the most pleasant and the least pleasant colors and forms, were the most effective. These extremes must represent the most intensive affective degrees, since all observers except B testified that the least pleasant stimuli were actually intensively unpleasant. It appears, therefore, that the dominance of color or form as independently determining the affective tendency of color-form depends directly upon the intensity of its pleasantness and unpleasantness.

Summation of affective tendencies.—We have seen that the high degree of correspondence between the results of Part II and III (attention on color or form alone) and of Part IV (attention on color-form) indicates that both color and form were simultaneously effective in conditioning the pleasantness of a colored form when the attention was directed by instruction upon both the color and form aspects; and that color and form operate simultaneously in conditioning affective judgment in the same manner that each operates when attended to separately. We have seen further that the relative effectiveness of color and form in simultaneous operation is dependent upon the effectiveness of each when operating in attentive isolation. Now to say that color and form thus work independently and simultaneously to establish affective tendency in the way in which each works separately is to state that the two summate. Since we are dealing here with ranks and lack exact measurements of the amount of the affective tendencies involved, we can not, of course, state that summation occurs in any exact arithmetical sense, but the general law that their effects are algebraically additive can not be denied. Remembering that four of the five observers made judgments at times upon the relative unpleasantness of the stimuli, we may lay down the rule: *Within the dimension of pleasantness and unpleasantness, the affective tendency of color-form varies approximately with the algebraical sum of the affective tendencies of its constituent color and form.*

TABLE XVIII

Effect of Direction of Attention: Preliminary Experiment

Preferential order of colors: correlation between ranks for colored squares, and ranks for colors of various forms. (12 obs.; 1st 4 women.)

Obs.:	1	2	3	4	5	6	7	8	9	10	11	12
ρ:	0.97	0.94	0.93	0.97	0.93	0.97	0.89	0.90	0.94	0.99	0.99	0.97

TABLE XIX

Effect of Direction of Attention: Preliminary Experiment

Preferential order of forms: correlation between ranks for "neutral" forms, and ranks for forms of various colors. (12 obs.; 1st 4 women)

Obs.:	1	2	3	4	5	6	7	8	9	10	11	12
ρ:	0.86	0.94	0.86	0.96	1.00	1.00	1.00	0.86	0.86	1.00	0.94	0.88

Confirmation by an Experiment under Diffused Daylight.—A comparable result was obtained in a preliminary experiment, which was conducted under uncontrolled diffused daylight but with laboratory conditions otherwise similar to those described above. There were 12 observers, 8 men and 4 women, all of whom except one had had only a little previous training in psychological observation. Color preferences were determined for these observers with a series of colored squares; form preferences were determined by the use of a series of forms outlined in black ink on a grey ground. The results were examined to see whether the judgments given for the 49 colored forms occurred as if dependent upon the concurrent independent summative operation of the tendencies effective in the first two series. As indicated in Tables XVIII and XIX, the co-efficients of correlation between the results on colors and forms and the results on color-forms are, without a single exception, very high for all the observers and support convincingly the conclusion reached in the foregoing analyses.

TABLE XX

AV. REACTION-TIMES (SECS.) FOR ALL COLORS OF EVERY FORM

Form	B	D	F	P	Av. 4 obs.	M
S	2.18	1.05	1.15	0.89	1.32	0.45
a	1.88	1.23	1.05	0.91	1.27	0.49
b	2.15	1.09	0.80	0.91	1.24	0.49
c	1.94	1.17	0.67	0.59	1.09	0.50
d	1.76	1.01	0.76	0.71	1.06	0.44
e	1.71	0.69	0.70	0.66	0.94	0.45
f	1.83	0.92	0.57	0.84	1.04	0.48
g	1.53	0.78	0.53	0.77	0.90	0.47
Av.	1.87	0.99	0.78	0.79	1.11	0.47
M.V.	0.17	0.15	0.16	0.11	0.15	0.02

TABLE XXI

AV. REACTION-TIMES (SECS.) FOR ALL FORMS OF EVERY COLOR

Color	B	D	F	P	Av. 4 obs.	M
N	2.19	1.05	0.68	1.25	1.29	0.64
R	2.08	0.92	0.73	0.78	1.13	0.60
O	1.19	0.80	0.63	1.05	0.92	0.65
Y	1.00	0.69	0.78	0.95	0.86	0.51
G	1.26	0.90	0.79	0.92	0.97	0.65
C	1.33	0.76	0.60	1.01	0.93	0.57
B	1.21	0.75	0.70	1.11	0.94	0.51
P	1.17	0.79	0.50	0.95	0.85	0.57
Av.	1.43	0.83	0.68	1.00	0.99	0.59
M.V.	0.35	0.10	0.09	0.10	0.16	0.04

TIME-RELATIONS OF AFFECTIVE JUDGMENTS

Speed.—The data indicating the speed of affective judgments are summarized in Tables XX-XXII. They are fairly comparable with the results of Washburn[24] and Nakashima.[25] Miss Washburn and her collaborators found, with a group of thirty observers, that the average of the average reaction times for all the judgments of pleasantness on colors taken together was 1.4 secs. The individual averages ranged from 1 to 2.5. For unpleasantness the average was 1.4, ranging from 1 to 2.5. With another group of twenty-five observers, the average of the average reaction times for judgments of pleasantness, exclusive of extreme judgments, was 1.6; the longest individual average for pleasantness was 2.7 secs., and the shortest

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[24] H. Potter, R. Tuttle, and M. Washburn, The speed of affective judgments, *Amer. J. Psychol.,* 25, 1914, 288-290.

[25] Nakashima, Time relations of affective processes, *Psychol. Rev.,* 16, 1909, 303-339.

TABLE XXII

AV. REACTION-TIMES (SECS.) FOR COLOR-FORMS FRACTIONATED TO SHOW PRACTICE-EFFECTS

Series	B	D	F	P	Av. 4 obs.	M
1	1.95	1.17	0.94	1.27	1.33	1.14
2	1.86	0.83	0.74	1.22	1.16	0.79
3	1.74	1.02	0.68	1.40	1.21	0.59
4	1.01	0.97	0.82	1.09	0.97	0.69
5	1.69	0.91	0.66	0.87	1.03	0.60
6	1.61	0.84	0.66	0.88	1.00	0.60
7	1.35	0.84	0.67	1.07	0.98	0.58
8	1.09	1.03	0.57	1.05	0.94	0.59
Av. 1-8	1.54	0.95	0.72	1.11	1.08	0.70
9	1.12	1.63	0.56	1.02	1.08	0.64
10	1.40	0.78	0.57	1.00	0.94	0.71
11	1.38	0.83	0.63	0.82	0.92	0.75
12	1.69	0.67	0.60	0.87	0.96	0.50
13	1.56	0.92	0.54	0.99	1.00	0.57
14	2.01	0.85	0.48	0.84	1.05	0.49
15	1.64	0.87	0.61	0.92	1.01	0.39
16	1.54	0.96	0.58	0.82	0.98	0.40
Av. 9-16	1.54	0.94	0.57	0.91	0.99	0.56
17	1.42	0.82	0.50	0.74	0.87	0.34
18	1.66	0.79	0.50	0.87	0.96	0.38
19	1.56	0.75	0.57	0.70	0.90	0.41
20	1.40	0.84	0.56	0.73	0.88	0.46
21	1.58	0.74	0.52	0.82	0.92	0.45
22	1.27	0.90	0.52	0.72	0.85	0.43
23	1.48	0.76	0.60	0.90	0.94	0.52
24	1.45	0.79	0.48	0.85	0.89	0.50
Av. 17-24	1.48	0.80	0.53	0.79	0.90	0.43
Av. of all	1.52	0.90	0.61	0.93	0.99	0.56
M.V.	0.20	0.12	0.08	0.14	0.14	0.13

0.9 secs. The average for unpleasantness, exclusive of extreme judgments, was 1.6 secs., the longest individual average for unpleasantness was 2.4 and the shortest 0.9. In both cases the speed varied with the intensity of affection. Nakashima by employing the direct reaction method found that the shortest time required for an affection to arise varied from 0.84 to 0.98 secs. for colors; and from 0.72 to 1.08 for geometrical figures. With our observers, the average of all the reaction times for colors is 1.11 secs., for forms, 0.99, and for color-forms, 0.99.[26] There are great individual differences due probably to the difference in attitude and in " affective sensitiveness " of the observers.

We find that the average of the reaction time for the comparisons of the most pleasant color-form with the 24 color-forms that come next in the order of preference (B = 0.80; D = 0.72; F = 0.50; P = 0.61 secs.) is greater than the average of the reaction time for the comparisons of the most pleasant color-form with the 24 least pleasant color-forms (B = 0.74; D = 0.63; F = 0.45; P = 0.45 secs.). It appears, therefore, that the speed of affective judgments under the method of paired comparisons varies with the degree of difference in the intensity or quality of the affective tendencies of the two stimuli compared. A similar relation would doubtless hold with sensory judgments.[27]

Effect of Practice.—The effect of practice is not striking except in the first two or three series. It is overshadowed by the presence of the daily fluctuations of reaction times, which are doubtless caused by the variations in psychophysical conditions determining the general efficiency of the observers. A practice-effect may, however, be made apparent by dividing the 24 series of experiments on color-forms into three groups according to the order in which they were conducted, and by comparing the averages of these reaction times. It will be seen (Table XXII) that these averages become shorter and shorter as they advance from the first to the last groups.

Distribution of reaction times.—The frequencies of reaction times in the experiments on colors, forms and color-forms are much skewed. Starting from the lower end, the distribution curve rises quickly to its maximum and falls slowly with the increase in the length of time. This skew is consistent with the fact that the reaction times are limited in negative deviation since they can scarcely be less than a considerable portion of a second, whereas there is no limitation to the delay that may occur in giving judgment.

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[26] M's results are excluded from the calculation of these averages, since with her the experiments were performed in the reversed temporal order.

[27] *Cf.* S. S. George, Attitude in Relation to the Psychophysical Judgment, *Amer. J. Psychol.,* 28, 1917, p. 33.

Conclusions

I. The preferential orders of colors and forms are relatively permanent during a period extending over five months.

II. When color-forms are presented in some particular form for affective comparisons of colors only, the form has practically no influence upon the preferential orders of these colors.

III. When color-forms are presented in some particular color for affective comparisons of forms only, the color has practically no influence upon the preferential order of forms.

IV. As far as relative pleasantness is concerned, color and form are, in the main, mutually independent in conditioning affective tendency of color-form, even though simultaneously operative.

V. The dominance of color or form as independently determining the affective tendency of color-form probably depends directly upon the intensity of its pleasantness or unpleasantness.

VI. (Corollary of IV and V.) Within the dimension of pleasantness and unpleasantness, the affective tendency of color-form varies approximately with the algebraic sum of the affective tendencies of its constituent color and form.

VII. Two types of affective judgments, characterized by sensorial and objective attitudes, can be made under the method of paired comparisons.

VIII. The reaction times for affective judgments are more rapid in the immediate judgment of the objective attitude than in the mediate judgment of the sensorial attitude, and more rapid when the members compared differ widely in affective degree.

BRENTANO AND WUNDT: EMPIRICAL AND EXPERIMENTAL PSYCHOLOGY[1]

By E. B. Titchener

§ 1. The year 1874 saw the publication of two books which, as the event has shown, were of first-rate importance for the development of modern psychology. Their authors, already in the full maturity of life, were men of settled reputation, fired as investigators with the zeal of research, endowed as teachers with a quite exceptional power to influence younger minds, ready as polemists to cross swords with a Zeller or a Helmholtz. Yet one would look in vain for any sign of closer intellectual kinship between them; hardly, indeed, could one find a greater divergence either of tendency or of training. Psychology, seeing how much their work and example have done to assure her place among the sciences, may gladly confess her debt to both. The student of psychology, though his personal indebtedness be also twofold, must still make his choice for the one or the other. There is no middle way between Brentano and Wundt.[2]

Franz Brentano began his career as a catholic theologian. In 1867 he published an outline of the history of philosophy within the mediaeval church which sets forth, as clearly and sharply as the essay of thirty years later, his famous doctrine of the four phases.[3] Early and late, however, his intellectual interest has centered in the philosophy of Aristotle. He came to psychology by way of an intensive study of the *De Anima,* and he has made the Aristotelian method his pattern of scientific procedure. We possess, unfortunately, only the first volume of his *Psychologie:* Brentano seems always to have preferred the spoken to the written word: but this volume, like everything else that he has given to the press, is complete in itself, the finished expression of his mature thought.

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[1] The following paragraphs form the introduction to the first volume of my long-projected and long-delayed work upon Systematic Psychology. When I wrote them, Brentano and Wundt were still living. Brentano died at Zurich, March 17, 1917; Wundt died at Leipsic, Aug. 31, 1920.

[2] F. Brentano, *Psychologie vom empirischen Standpunkte* (henceforth cited as *PES*), i, 1874. Cf. the Biographical Note in F. Brentano, *The Origin of the Knowledge of Right and Wrong,* trs. C. Hague, 1902, 119 ff.; M. Heinze, *F. Ueberwegs Grundriss der Geschichte der Philosophie*, iv, 1906, 332 ff. W. Wundt, *Grundzüge der physiologischen Psychologie* (henceforth cited as *PP*), 1874. The first ten chapters of Wundt's work were issued in 1873 and are utilised by Brentano. For a bibliography of Wundt's scientific writings see *Amer. Journ. Psych.,* xix (1908) ff.;cf. Heinze, *op. cit.,* 322 ff.

Wilhelm Wundt started out as a physiologist, interested in the special phenomena of nerve and muscle. In 1862 he had sought to lay the foundations of an 'experimental psychology' (the phrase now appears in print for the first time)[4] in a theory of sense-perception. Here he fell into the mistake to which every student of natural science is liable who turns, without due preparation, to the things of mind: the mistake, namely, of supposing that psychology is nothing more than an applied logic; and the mistake was repeated in a popular work upon human and animal psychology which followed on the heels of the technical volume. By 1874 he had definitely discarded this earlier view for the conception of psychology as an independent science. He still maintained, however, that the path to it leads through the anatomy and physiology of the nervous system.

Such, in briefest outline, were the conditions under which the two psychologies acquired their form and substance. We see, on the one hand, a man who has devoted his 'hours of solitary reflection' to ancient and mediaeval philosophy; we see, on the other hand, a man who has wrought out in the laboratory his contributions to the latest-born of the experimental sciences. They are both professors of philosophy, and they are both to range widely, in the future, over the varied fields of philosophical enquiry. Yet it would be wrong to suppose that the psychology to which they have now attained, and which, by a happy chance, they give to the world in the same year, represents merely an incident, even if it were the central incident, of their philosophical history. Psychology, on the contrary, has laid strong hands upon them, and is to dominate all their further thinking. Wundt, a generation later, will round off the manifold list of his books with the encyclopaedic folk-psychology, and Brentano never gives up the hope of a descriptive—to be followed, perhaps, at long last by a genetic—psychology as the ripe fruit of his studious old age.

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[3] J. A. Möhler, *Kirchengeschichte,* ii, 1867, 539 f.; F. Brentano, *Die vier Phasen der Philosophie und ihr augenblicklicher Stand,* 1895. The four phases, repeated in the three great philosophical periods, are those of scientific construction, failure or perversion of the scientific interest, scepticism and mysticism.

[4] W. Wundt, *Beiträge zur Theorie der Sinneswahrnehmung,* 1862, vi.

§ 2. We shall better understand the nature of this choice which lies before us if we first note the points of resemblance between the two systems. For even in 1874 psychology was not in such bad case that Brentano and Wundt are always at variance. They agree that psychology holds a place of high importance in the fellowship of the sciences, and that it is logically prior to natural science.[5] They agree that it may dispense with the concept of substance and confine itself to an account of phenomena.[6] They reject the unconscious as a principle of psychological explanation.[7] They define the unity of consciousness in substantially the same terms.[8] So far there is agreement: and though the agreement is largely of a formal kind, and though a good deal of it has a negative ground in the reaction against Herbart, it serves nevertheless to mark out a common universe of discourse.

On the material side there is also agreement, with such difference of emphasis as the difference of authorship would lead us to expect. We find, for instance, that Brentano deals at length with the general method of psychology, and is at pains to distinguish inner perception from inner observation, while Wundt takes inner observation for granted and describes in detail only those special procedures which raise it to the rank of experiment.[9]. We find that Wundt devotes much space to Fechnerian psychophysics, and interprets the psychophysical law as a general psychological law of relativity, while Brentano makes only incidental and critical mention of Fechner's work.[10] The differences are striking enough, but behind them lies agreement regarding the subject-matter of psychology. Even in the extreme case, where the one book emphasises what the other omits, difference does not of necessity mean disagreement. We find, again, that Wundt says nothing of a question which for Brentano is the essential problem of psychology as it was the first problem of psychophysics, the question of 'immortality,' of the continuance of our mental life after death, and conversely that Brentano fails to discuss Wundt's cardinal problem of attention. Yet Wundt had touched upon the question of immortality in his earlier writing, and Brentano plainly recognises that there is a problem of attention, although (as we may suppose) he has put off its discussion to his second volume.[11]

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[5] *PES*, 24 ff., 119; *PP*, 4, 863.

[6] *PES*, 10 ff.; *PP*, 9, 12, 20.

[7] *PES*, 133 ff.; *PP*, 644 f., 664, 708 f., 712, 790 ff.

[8] *PES*, 204 ff.; *PP*, 715 ff., 860 ff.

[9] *PES*, 34 ff., 184; *PP*, 1 ff.

[10] *PP*, 421; *PES*, 9 f., 87 ff.

So the student of psychology who read these two books in their year of issue might, if he had made due allowance for the training and natural tendencies of the authors, have entertained a reasonable hope for the future of his science; and we ourselves, who see their differences far more plainly than was possible for him, may still hope that the main issue can be taken on common ground and fought out at close quarters.

§ 3. Brentano entitles his book 'psychology from the empirical standpoint,' and Wundt writes 'physiological psychology' on his title-page and suggests 'experimental psychology' in his text.[12] The adjectives do not greatly help us. For all experimental psychology is in the broad sense empirical, and a psychology which is in the narrow sense empirical may still have recourse to experiment. To show the real difference between the books, the difference that runs through their whole texture and composition, we need at this stage terms that are both familiar and clear; the time has not yet come for technicalities and definitions. We may say, as a first approximation, that Brentano's psychology is essentially a matter of argument, and that Wundt's is essentially a matter of description.

At the end of his discussion of method Brentano refers with approval to Aristotle's use of *aporiae*, of difficulties and objections, wherein a subject is viewed from various sides, and opinion is weighed against opinion and argument against argument, until by comparison of pros and cons a reasonable conclusion is reached.[13] This is, in the large, his own way of working. He appeals but rarely, and then only in general terms, to facts of observation. His rule is to find out what other psychologists have said, to submit their statements to a close logical scrutiny, and so by a process of sifting to prepare the reader's mind for a positive determination. When the ground has thus been cleared Brentano's doctrine, novel though it may be, has the appearance (so to say) of a necessary truth; we feel that we have duly considered the possibilities in the case and have come to the one rational decision; and if for conscience' sake we go on to deduce and to verify, we still are assured beforehand that everything will fit together within the system. Minor points may need to be expanded; even, perhaps, in the light of further *aporiae,* to be corrected; but the whole exposition gives the impression of finality.[14] It is no wonder, then, that many students have judged the author successful in his aim of writing, not Brentano's psychology, nor yet a national psychology, but—psychology.[15]

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[11] *PES*, 17 ff., 32 f., 95 f.; Wundt takes up the question of immortality (indirectly, it is true) in *Vorlesungen*, etc., ii, 1863, 436, 442; cf. the direct treatment in the later edition, 1892, 476 ff. Brentano recognises the problem of attention in *PES*, 91, 155; cf. 263, and C. Stumpf, *Tonpsychologie*, i, 1883, 68; ii, 1890, 279 f.

[12] *PP*, 3.

[13] *PES*, 96 f.; cf. J. S. Mill, Grote's Aristotle, *Fortnightly Rev.*, N. S. xiii, 1873, 48 ff. Brentano had earlier noted, with the same approval, the use of *aporiae* by Thomas Aquinas: see J. A. Möhler, *Kirchengeschichte*, ii, 1867, 555.

Wundt's book, on the contrary, abounds in facts of observation: anatomical facts, physiological facts, results of psychophysical and psychological experiment. Its introductory chapter is brief to the point of perfunctoriness, and criticism of psychological theories is packed away into fine-print paragraphs that, to all intents and purposes, are a series of appendices. There is, to be sure, a great deal of argument. Where the facts are scanty, they must not only be generously interpreted but must also be eked out by hypothesis; if a leading physiologist has mistaken the problem of sense-perception, he must be argued into a better way of thinking; in any case, the new science of experimental psychology must offer a bold front to her elder sisters.[16] The argument, none the less, is always secondary and oftentimes plainly tentative; so that the book as a whole gives the impression of incompleteness, of a first essay which can be improved when more work (and a great many suggestions of further work are thrown out[17]) has been accomplished. Hence it is no accident, but rather a direct reflex of the spirit in which the authors approached their task, that Brentano's volume still bears the date 1874 while Wundt's book, grown to nearly triple its original size, has come to a sixth edition.[18]

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[14] I know of only three corrections that Brentano has made to his psychology. (1) In *PES* 292 degree of conviction, as intensity of judgment, is declared analogous to degree of intensity of love and hate (cf. 203); in the notes to *The Origin of the Knowledge of Right and Wrong* (1889), 1902, 52 f., this analogy is denied. (2) In *PES* 202 f. feeling is said to be always present along with ideation; the belief to the contrary is due to the mistaken preference of memory over inner perception (44); but in *Untersuchungen zur Sinnespsychologie,* 1907, 119, 124, the acts of the two higher senses are not intrinsically emotive. (3) In *PES* 115 the object upon which a psychical phenomenon is directed is not to be understood as *eine Realität*; but the notes appended to the reprinted section *Von der Klassifikation der psychischen Phänomene* (1911, 149) lay it down that "nie etwas anderes als Dinge, welche sämtlich unter denselben Begriff des Realen fallen, für psychische Beziehungen ein Objekt abgibt."—There would, no doubt, if the book were rewritten, be many other modifications of detail, and yet others if the second volume were undertaken; the discussion of the modi of ideation in the *Klassifikation* shows that Brentano had not in 1874 thought out the doctrine of his Bk. iii. In the main, nevertheless, the doctrine of 1874 has stood the test of Brentano's own continued reflection and of the attacks of critics.

Such an achievement is worthy of all admiration. Only we must add—those of us who challenge Brentano's premises—that even isolated changes are disconcerting. The first statement is so serenely confident, and the changes are again so confidently made!

[15] *PES,* vi.

This thorough-going difference of argument and description means, of course, a radical difference of attitude toward psychology itself. It means that Brentano and Wundt, in spite of formal and material agreement, psychologise in different ways. Our next step, therefore, is to place ourselves inside the systems and to realise, so far as we may without too much detail, what manner of discipline they intend psychology to be. We have to choose: and the illustrations that follow will show the alternatives of choice in concrete and tangible form.

§ 4. Brentano defines psychology as the science of psychical phenomena. The term may easily be misleading: for the phenomena in question are very far from being static appearances. Generically they are activities; in the individual case they are acts. Hence they can properly be named only by an active verb. They fall into three fundamental classes: those, namely, of Ideating (I see, I hear, I imagine), of Judging (I acknowledge, I reject, I perceive, I recall), and of Loving-Hating (I feel, I wish, I resolve, I intend, I desire). We may use substantives if we will, and may speak of sensation and idea, memory and imagination, opinion, doubt, judgment, joy and sorrow, desire and aversion, intention and resolution; but we must always bear in mind that the psychical phenomenon is active, is a sensing or a doubting or a recalling or a willing.

It is true that we never have act without content. When we ideate, we sense or imagine something; when we judge, we perceive something, acknowledge the truth of something, recall something; when we love or hate, we take interest in something, desire or repudiate something. This, however, is precisely the difference between psychical and physical phenomena. The latter are blank and inert: the color or figure or landscape that I see, the chord that I hear, the warmth or cold or odor that I sense, the like objects that I imagine, all these things are described when their given appearance is described; their appearance sums them up and exhausts them; they have no reference, and do not carry us beyond themselves. Psychical phenomena, on the other hand, are precisely characterised by relation to a content, by reference to an object; they contain an object intentionally within them; and this character of immanent objectivity, in virtue of which they are active, marks them off uniquely from the physical phenomena upon which they are directed or toward which they point. Even in cases where the content of a psychical phenomenon is not physical, but is another psychical phenomenon, the distinction holds good. For the act which becomes content or object of another act is not thereby deprived of its essential character; it is still active in its own right; and it is therefore by no means confusable with bare physical appearance.[19]

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[16] *PP*, Vorwort.

[17] *PP*, 284, 293, 314, 317, 373, 394, 399, etc., etc.

[18] See the prefaces to the successive editions of the *PP*. Even the sixth edition, as I have shown elsewhere (*Psych. Rev.*, xxiv, 1917, 52 f.), has not attained to systematic completion, and only in the fifth (*PP*, i, 1902, ix) did Wundt set himself definitely to the task of system-making.

These are Brentano's views of the subject-matter of psychology. He begins by considering the alleged differences between physical and psychical, finds an adequate *differentia* of the psychical, and is therefore able to define psychology in terms of the matter with which it deals. He then reviews the principal classifications hitherto made of psychical phenomena, and arrives at a classification of his own, in which judgment is accorded independent rank, and feeling and will are bracketed under a single heading. Throughout the discussion his chief reliance is upon argument. To be sure, he takes the testimony of inner perception; but inner perception is not observation; it is rather a self-evident cognition or judgment; and as such it is, if we may use the phrase, of the same stuff as argument.[20] Psychological observation is possible for Brentano only when past acts are recalled in memory; then indeed, as he admits, even a sort of experimentation becomes possible. Not only, however, is memory subject to gross illusion, but the act of memory, once more, falls under the category of judgment, so that experiment itself takes place in the world of argument.[21] The empirical psychology thus employs the same psychical activities to establish the nature of its subject-matter and to discuss the variety of psychological opinion.

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[19] *PES*, 23 f, 35, 101 ff, 161, 167, 256 ff. On the problem of natural science as an explanatory discipline, see 127 ff.

[20] *PES*, 35 ff, 181 ff (summary 202 f), 262. Cf. *Klassifikation*, 1911, 129.

§ 5. For Wundt, psychology is a part of the science of life. Vital processes may be viewed from the outside, and then we have the subject-matter of physiology, or they may be viewed from within, and then we have the subject-matter of psychology.[22] The data, the items of this subject-matter, are always complex, and the task of experimental psychology is to analyse them into " the elementary psychical processes." If we know the elements, and can compare them with the resulting complexes, we may hope to understand the nature of integration, which according to Wundt is the distinguishing character of consciousness.[23]

Analysis of the processes of the inner life brings us, in the last resort, to pure sensations, constituted originally of intensity and quality. Sensations carry no reference; they look neither before nor after; they tell us nothing of their stimuli, whether external or organic, and nothing of their point of excitation, whether peripheral or central, nor do they forecast the ideas in which we find them synthetised. They simply run their course, qualitatively and intensively, and may be observed and described as they proceed.[24] Ideas, in their turn, are originally constituted of these sensations; there is nothing within or upon them to show whether they are ideas of imagination or perceptions.[25] Individual ideas differ psychologically from general ideas solely in the nature of their sensory constituents: in the former the complex of sensations is constant, in the latter it is variable.[26] Concepts are not " psychical formations " at all; if we psychologise them, we discover only their substitutes in consciousness, spoken or written words, accompanied by a vague and indeterminate feeling.[27] Judgments, in the same way, belong to logic, and not primarily to psychology; logic and psychology approximate only as a result of the parallel growth, long continued, of conceptual thinking and its expression in language; our " conscious psychological processes" consist originally of nothing more than ideas and their connections.[28]

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[21] *PES,* 42 ff, 162, 169, 262; *Klassifikation,* 130.

[22] *PP,* 1 ff.

[23] *PP,* 5, 20, 717.

[24] *PP,* 273 ff., 484 f. When sensations enter into connection with one another, the third attribute of affective tone or sensory feeling is added. Intensity and quality are, however, the " more original " constituents.

[25] *PP,* 464 f.

[26] *PP,* 468.

[27] *PP,* 672.

The trend of all this analysis is clear: Wundt is trying to describe mind, to show the stuff of which it is made, to reduce it to its lowest terms. When, however, he turns from analysis to synthesis, the exposition is less easy to follow. Sensations are integrated into ideas by a "psychical synthesis" which Wundt himself compares to a chemical synthesis and which critics have assimilated to Mill's "mental chemistry."[29] Ideas gain their objective reference by a "secondary act" which seems to consist, psychologically, in the simple addition of further ideas;[30] yet the objective reference is itself put, later on, to psychological purposes. Concepts and forms of intuition are made 'postulates' of advancing thought,[31] as if the logical and practical aspects of mind were necessarily implied in its given or phenomenal aspect, and as if the psychologist might shift from one aspect to another without breach of scientific continuity. But though we may puzzle over details, there is nothing obscure in the general situation. Wundt, like many others of his generation, is dazzled by the vast promise of the evolutionary principle;[32] 'original' is for him more or less what 'nascent' is for Spencer; the later must derive from the earlier, because that is the way of things, and the later has no other basis. Let us remember, all the same, that Wundt's primary effort is to describe, and that he falls back upon 'genetic explanation' only when some phase of the traditional subject-matter of psychology proves to be indescribable.

That, then, is one of the threads of Wundt's system. Even a descriptive psychology cannot, however, be written simply in terms of sensations and their modes and levels of psychological integration. For the field of consciousness, Wundt reminds us, is not uniformly illuminated; it shows a small bright area at its centre and a darker region round about; the ideas which occupy it differ in their conscious status. So arises the problem of attention. Descriptively—Wundt takes up the task of description piecemeal, in different contexts, as if it were 'on his conscience'—attention reduces to clearness of ideas and characteristic feelings of effort or strain.[33] It has two concrete manifestations, apperception and voluntary action; we speak of apperception when we are considering the internal course of ideas, and of voluntary action when we are considering the issue of an emotion in external movement.[34] Both forms of the attentional process are subject to conditions, and both are strictly correlated with physiological processes in the cerebral cortex; they therefore fall within the limits of a scientific psychology.[35] Yet psychologists have neglected them, and have paid the penalty of this neglect in inadequate psychology and untenable philosophy.[36]

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[28] *PP,* 709 ff.

[29] *PP,* 484 f; J. S. Mill, *A System of Logic,* 1843, bk. vi, ch. iv (ii, 1856, 429); *An Examination of Sir William Hamilton's Philosophy,* 1865, 286 f; note in J. Mill, *Analysis of the Phenomena of the Human Mind,* i, 1869, 106 ff. The original source is D. Hartley, *Observations on Man,* 1749, pt. i, ch. i, sect. 2, prop. 12, cor. 1 (i, 1810, 77 f.).

[30] *PP,* 465.

[31] *PP,* 672, 680.

[32] *PP,* vi.

We need not here trace the doctrine of attention further; we need not either debate whether the problem of attention is included in Wundt's formal statement of the task of experimental psychology. We may, however, as an illustration of the interweaving of the two systematic threads, glance at his treatment of the association of ideas. He begins, as we might expect, with mode of integration; and under this heading declares that the recognised laws, of similarity and of frequency of connection in space and time, are imperfect even as empirical generalisations. We find, it is true, two forms of association, distinguishable in the free play of fancy and in reflective thought. But the one is wider than association by similarity, in that the effective resemblance may reside in any and every sensory constituent of the ideas concerned, and especially in their affective tone, while the other reveals itself simply as an affair of habit. Wundt therefore proposes to term them, respectively, 'association by relationship' and 'association by habituation.' The new names, he maintains, are not indifferent; for they do fuller justice than the old to the facts of self-observation, and they also point us to the conditions of association in the central nervous substance.[37]

Here then is an improvement on the side of analysis and synthesis; but that is not enough. For ideas do not associate automatically, as it were of their own motion; the laws of association are, on the contrary, under the universal dominance of attention. And now there opens up, for experimental attack, a whole series of special problems which an empirical psychology, following only the single line of enquiry, must naturally miss. In their light we pass beyond associationism to a more faithful transcript of the 'course and connection of ideas';[38] and in like manner we avoid, in our psychology of will, the philosophical *impasse* of indeterminism.[39]—

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[33] *PP,* 717 ff., esp. 724.

[34] *PP,* 831, 835.

[35] *PP,* 720 f., 723 f., 834 f.

[36] *PP,* 792 f., 831 ff.

[37] *PP,* 788 ff.

These paragraphs express, in rough summary, the teaching of the Wundt of 1874. He does not give psychology a distinct and peculiar subject-matter; the difference between physiology and psychology lies simply in our point of view. Wundt had already published a comprehensive work upon physiology, and now that he has turned to psychology he carries his knowledge and method with him; he is convinced that the processes of the inner life are best set forth in close connection with those of the outer life, and that the results of inner observation are surest when the appliances of external observation, the procedures of physiology, are pressed into psychological service. He spends little time upon preliminaries, but gets as quickly as may be to the exposition of facts. Where facts are few or lacking, he seeks to supplement or to supply them by observations of his own. His primary aim in all cases is to describe the phenomena of mind as the physiologist describes the phenomena of the living body, to write down what is there, going on observably before him: witness his treatment of idea, of concept, of attention, of association. There is still great space for argument, and the argument, we must admit, is often influenced by previous habits of thought, by psychological tradition, by a certain tendency to round things off to a logical completeness, by a somewhat naïve trust in the principle of evolution. The argument, however, does not impress the reader as anything but secondary: Wundt is at once too dogmatic and too ready to change his views. The recurring need of further facts and the patchwork character of the argument suggest, both alike, that psychology, under his guidance, has still a long systematic road to travel.

§ 6. We have now viewed our two psychologies from within. Brentano, we have found, looks back over the past, weeds out its errors with a sympathetic hand, accepts from it whatever will stand the test of his criticism, and organises old truth and new into a system meant, in all essentials, to last as long as psychology shall be studied; Wundt, after he has acknowledged his debt to the past, turns away from it and plunges into the multifarious and detailed work of the laboratories, producing a psychology that is as much encyclopaedia as system, and that bears on its face the need for continual revision. Which of the two books holds the key to a science of psychology?

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[38] *PP*, 793; cf. the earlier sections of ch. xix.

[39] *PP*, 837 f.

Brentano has all the advantage that comes with historical continuity. His doctrine of immanent objectivity goes back to Aristotle and the Schoolmen, and the classification of psychical acts into ideas, judgments, and phenomena of love and hate goes back to Descartes.[40] More than this: he can claim kinship with every psychologist, of whatever school, who has approached his subject from the technically 'empirical' standpoint. For the 'empirical' psychologist means to take mind as he finds it; and like the rest of the world, who are not psychologists, he finds it in use; he finds it actively at work in man's intercourse with nature and with his fellow-man, as well as in his discourse with himself. Terms may change and classifications may vary, but the items of classification are always activities, and the terms employed—faculties, capacities, powers, operations, functions, acts, states—all belong to the same logical universe. Brentano, innovator though he is, takes his place as of right in a great psychological community.[41]

To offset this advantage, and to justify his own break with tradition, Wundt holds out the promise of an experimental method. He should have been more explicit: for technology as well as science—medicine as well as physiology, engineering as well as physics—makes use of experiment. His actual purpose, as we trace it in the chapters of his book, is to transform psychology into an experimental science of the strict type, a science that shall run parallel with experimental physiology.[42] He failed, no doubt, to see all that this purpose implied, and his earlier readers may be excused if they looked upon his work as an empirical psychology prefaced by anatomy and physiology and interspersed with psychophysical experiments. There is plenty of empirical psychology in the volume. If, however, we go behind the letter to the informing spirit; if we search out the common motive in Wundt's treatment of the familiar topics; if we carry ourselves back in thought to the scientific atmosphere of the seventies, and try in that atmosphere to formulate the purpose that stands out sharp and clear to our modern vision; then the real significance of the *Physiological Psychology* cannot be mistaken. It speaks the language of science, in the rigorous sense of the word, and it promises us in this sense a science of psychology.

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[40] *PES,* 115 f.; *The Origin of the Knowledge of Right and Wrong,* 47.

[41] In spite of the remarks in §3 and in §6 below it may seem unjust to Brentano if, even in this preliminary sketch of the psychological issue, his interest in experiment is left without record. We note, then, that as early as 1874 he urged the establishment at Vienna of a psychological laboratory (*Ueber die Zunkunft der Philosophie,* 1893, 47 f.); that he has published *Untersuchungen zur Sinnespsychologie* (1907) and in particular that he brought the Müller-Lyer illusion to the attention of psychologists (*Zeits. f. Psych. u. Phys. d. Sinnesorgane,* iii, 1892, 349); and that Stumpf, who was his pupil (Ueberweg-Heinze, iv, 1906, 334 f.), has given us the experimental *Tonpsychologie.* All this, however, does not prevent his being, in the narrow sense, an 'empirical' psychologist. Stumpf tells us that his own work is to "describe the psychical functions that are set in action by tones" (*Tonpsych.,* i, 1883, v) and declares later that "there cannot be a psychology of tones; only a psychology of tonal perceptions, tonal judgments, tonal feelings" (*Zur Einteilung der Wissenschaften,* 1907, 30). Brentano, even with a laboratory, would not have been, in Wundt's sense, an 'experimental' psychologist. We know, besides, something of Brentano's systematic programme. The empirical psychology is not to be concluded; it is to be supplemented and replaced by a 'descriptive' psychology (*The Origin,* etc., vii, 51 f.), fragments of which have appeared in *The Origin of the Knowledge of Right and Wrong* (dealing with the phenomena of love and hate and, in the Notes, with judgment) and in the *Untersuchungen* sense-perception). This in turn is to be followed by an explanatory' or 'genetic' psychology, a sample of which is given in *Das Genie,* 1892 (see *The Origin,* etc., 123).

But Brentano also speaks of a 'science' of psychology. Which of the two authors is in the right?

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[42] The substitution of folk-psychology for experiment in the study of the more complicated mental processes appears in the fourth edition (*PP,* i, 1893, 5); the reservation in regard to psychophysical parallelism in the fifth edition (*PP,* iii, 1903, 775 ff.).

A PRELIMINARY STUDY OF THE RANGE OF VISUAL APPREHENSION

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Writers of psychological textbooks usually state that from four to six objects may be apprehended in a single grasp of attention.[1] It is not often that the range of attention is supposed to be greater than six, although a recent text states: "Under ordinary conditions from 6 to 8 objects are clearly distinguished simultaneously. The number may be increased with practice to about 15."[2] The emphasis upon the number six is derived from the pre-experimental work of Sir William Hamilton and his predecessors, but the later experiments seem not to have altered it.[3]

When one examines these measures of the so-called range of attention, however, one finds that the concepts are neither clear nor exact. The usual textbook statement would lead one to believe that all groups up to, say, 4, 5 or 6 objects are invariably capable of being apprehended correctly and that there is no way of assuring correct apprehension of a greater number. Nothing could be further from the case. Take Jevons' early results.[4] He threw at random a few beans into a box and sought at a glance to estimate their number. His results, thrown into the form of relative frequencies, are as follows:

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[1] E. B. Titchener, *A Beginner's Psychology,* 1915, 103; J. R. Angell, *Psychology,* 1904, 81; C. S. Myers, *A Text-book of Experimental Psychology,* 1911, 321; W. B. Pillsbury, *The Fundamentals of Psychology,* 1916, 247; R. M. Yerkes, *Introduction to Psychology,* 1911, 297.

[2] H. C. Warren, *Human Psychology,* 1919, 251.

[3] Sir W. Hamilton, *Lectures on Logic and Metaphysics,* 1859-60, I, 253 f. For a survey of the experimental work cf. J. McK. Cattell, *The Inertia of the Eye and Brain, Brain,* VIII, 1886, 248-404; B. Erdmann & R. Dodge, *Psychologische Untersuchungen über das Lesen,* 1898; G. M. Whipple, The Effect of Practice Upon the Range of Visual Attention and of Visual Apprehension, *Jour. of Ed. Psychol.,* I, 1910, 249-262; F. N. Freeman, Untersuchungen über den Aufmerksamkeitsumfang und die Zahlauffassung bei Kindern und Erwachsenen, *Päd. Psychol. Arbeit.,* 1910, 88-168; J. Zeitler, Tachistoskopis. Versuche über das Lesen, *Philos. Stud.,* XVI, 1900, 380-463.

[4] W. S. Jevons, The Power of Numerical Discrimination, *Nature,* III, 1871, 281 f. It will be noted that Jevons' results are very incompletely quoted by W. James, *The Principles of Psychology,* 1890, I, 406. James quotes only the results for the first four stimuli, namely, 3, 4, 5, and 6, while Jevons used stimuli up to 15 beans. The results as quoted by James are misleading.

Number of Beans	Per Cent. Correct Judgment
3	100
4	100
5	96
6	82
7	73
8	56
9	62
10	43
11	38
12	42
13	23
14	28
15	18

Jevons uses as the measure of the discrimination 100 *per cent* correct judgments and concludes that his limit is between 4 and 5 beans.

Cattell's experiments in the Wundtian laboratory yield the following results.

	PER CENT. CORRECT JUDGMENT			
Number of Objects Exposed	Lines	Digits	Letters	Short Words
2				60
3		90	87	23
4	90	74	63	
5	75	60	36	
6	75	34	23	

But here one cannot accurately use 100 *per cent* correct judgments as the measure of apprehension, because the experiments do not include those small values of the stimulus that would give the highest frequencies. One might extrapolate the function to 100 *per cent* if the frequencies indicated the exact nature of the curve.

In view of these facts, it appears that the statistical limen or threshold, used in the work on sensation and perception, is a more reliable and more readily determined measure of the so-called range of attention. The threshold is defined as that value of stimulus the sensing of which has a probability of 0.5; or that value of stimulus which is as likely to be sensed as not. The problem of the range of apprehension presents, statistically, the same problem as does the terminal limen for sensation. In the case of sensation, one is interested in determining, let us say, the value in wave-frequency of the tone which is sensed just half of the time and which is so high that it is just not sensed half of the time. Similarly, in the range of apprehension, one is interested in determining the number of objects, the correct apprehension of which has a probability of 0.5. The use of the psychophysical procedure in this case not only yields a measure of the range of apprehension that has a maximum degree of precision, but also ordinarily provides a measure of the degree of precision. It is curious that investigators, working outside the field of sensation, have not oftener used the tools of psychophysics that have been so highly developed for sensation. Williams[5] made such an application when he calculated an associative limen by means of the method of constant stimuli, but the extension is unusual.

Jevons' results, although they involve three inversions, can nevertheless, for purposes of rough comparison, be treated by the method of constant stimuli. For his data the limen is 10.28 beans and the coefficient of precision is 0.143,—a statement that is much more definite than Jevons' conclusion: "The limit of complete accuracy, if there were one, would be neither 4 nor 5, but half-way between them; but this is a result as puzzling as one of the uninterpretable symbols in mathematics, just, for instance, like the factorial of a factorial number. But I give it for what it may be worth."[6]

It must be remembered that this threshold will ordinarily be expressed in fractions of an object, even though it is not possible to prepare a fractional number of objects, of which the correct apprehension has a probability of 0.5. The limen is a *statistical limen,* a calculated quantity which summarizes observed results for purposes of scientific comparison. When one finds the upper threshold of tone to be, say, 18,264 *vs.,* one does not seek to construct a stimulus for this frequency in order to see whether it will give 50 *per cent* positive judgments. One is satisfied with the computed limen as part of a statistical account of observations already made.

The present paper presents some preliminary determinations of a limen of visual apprehension.[7] The stimuli were circular dots, 5 mm. in diameter. Four to twelve dots were arranged on cardboards in a haphazard order. Each group fell entirely within the range of foveal vision. These cards were exposed by means of a Whipple tachistoscope.[8] A pre-exposure fixation point was used. The subject was seated at a distance of 2.5 m. from the apparatus.

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[5] H. D. Williams, On the Calculation of an Associative Limen, *Amer. Jour. of Psychol.*, XXIX, 1918, 219-226.

[6] Jevons, *op. cit.*

[7] These experiments were performed in the Laboratory of Experimental Psychology at Clark University during the spring of 1920.

Three series of stimuli were employed. They were:

Series 1. Black dots on white background with an exposure time of 100 sigma.

Series II. Black dots on a medium-grey background with an exposure time of 100 sigma.

Series III. Black dots on a white background with an exposure time of 60 sigma.

By comparing Series I and III we hoped to study the effects of the speed of exposure. By comparing the results of Series I and II we hoped to study the effect of the brightness of the stimulus.

Five subjects were employed. They were L. D. Boring, Ph. D. (Ob. B), C. C. Pratt, A. M. (Ob. P), S. Yokoyama, A. M. (Ob. Y), M. Bates, A. M. (Ob. Ba) and J. H. Alston, A. M. (Ob. A). The instructions were: "You will be shown successively, for a very short exposure, cards which will contain a varying number of black dots on a white (or grey) background. Immediately after the exposure you will report verbally the number of dots which you have apprehended. Be sure of your judgment and do not guess. The report 'I do not know' is admitted."

The judgments were recorded either as 'correct' or 'not correct.' Seven cards were shown in each series, four to ten dots to Obs. P, Ba and A; six to twelve dots to Obs. B and Y. Nine series of cards were prepared (63 cards in all); each series was shown an equal number of times; and the cards of each series were shuffled after the series had been presented. In Series I (white background, 100 sigma exposure) 250 judgments for each number of dots were obtained from each subject. In Series II (grey background, 100 sigma exposure) 150 judgments were obtained, and in Series III (white background, 60 sigma exposure) 150 judgments were obtained from each subject. The results were fractionated into groups of 50 judgments on each stimulus and each fraction was treated separately by Urban's procedure for the method of constant stimuli.[9]

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[8] For a description of this apparatus cf. G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 264 f.

[9] For a description of this form of mathematical treatment cf. F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, XXIV, 1912, 236-243.

TABLE I

Ob. B.

	Fractions	Number of Dots						
		6	7	8	9	10	11	12
Series I	I	0.96	0.88	0.72	0.58	0.44		
	II	1.00	0.96	0.88	0.52	0.46	0.28	0.12
	III	1.00	0.96	0.84	0.64	0.46	0.28	0.20
	IV	1.00	0.96	0.74	0.58	0.44	0.22	0.22
	V	1.00	0.98	0.84	0.56	0.36	0.18	0.10
	Ave.	0.99	0.95	0.80	0.58	0.43	0.24	0.16
Series II	VI	1.00	0.98	0.86	0.56	0.34	0.22	0.08
	VII	1.00	0.98	0.90	0.60	0.38	0.20	0.10
	VIII	1.00	0.98	0.90	0.74	0.54	0.30	0.12
	Ave.	1.00	0.98	0.89	0.63	0.42	0.24	0.10
Series III	IX	1.00	0.98	0.92	0.74	0.46	0.32	0.16
	X	1.00	1.00	0.92	0.86	0.56	0.32	0.22
	XI	1.00	0.98	0.94	0.74	0.52	0.28	0.18
	Ave.	1.00	0.99	0.93	0.78	0.51	0.31	0.19

TABLE II

Ob. P.

	Fractions	Number of Dots						
		4	5	6	7	8	9	10
Series I	I	1.00	0.92	0.84	0.60	0.06	0.00	0.00
	II	1.00	0.98	0.88	0.60	0.04	0.00	0.00
	III	1.00	0.98	0.92	0.48	0.08	0.00	0.00
	IV	0.96	0.88	0.82	0.56	0.08	0.00	0.02
	V	0.96	0.94	0.90	0.64	0.20	0.00	0.00
	Ave.	0.98	0.94	0.87	0.58	0.09	0.00	0.00
Series II	VI	1.00	0.98	0.86	0.62	0.30	0.00	0.00
	VII	0.98	0.96	0.80	0.56	0.06	0.02	0.00
	VIII	0.98	0.94	0.78	0.48	0.16	0.02	0.00
	Ave.	0.99	0.96	0.81	0.55	0.17	0.01	0.00
Series III	IX	0.98	0.96	0.80	0.58	0.24	0.04	0.00
	X	1.00	0.96	0.88	0.68	0.30	0.06	0.00
	XI	1.00	0.96	0.92	0.62	0.28	0.04	0.00
	Ave.	0.99	0.96	0.87	0.63	0.27	0.05	0.00

TABLE III

Ob. Y.

	Fractions	Number of Dots						
		6	7	8	9	10	11	12
Series I	I	0.98	0.94	0.86	0.72	0.54		
	II	0.98	0.94	0.90	0.68	0.64	0.42	0.20
	III	1.00	0.98	0.92	0.72	0.56	0.44	0.16
	IV	1.00	0.98	0.92	0.80	0.70	0.50	0.30
	V	1.00	1.00	0.98	0.90	0.72	0.56	0.30
	Ave.	0.99	0.97	0.92	0.76	0.63	0.48	0.24
Series II	VI	1.00	0.98	0.96	0.88	0.74	0.56	0.34
	VII	1.00	0.98	0.94	0.92	0.82	0.54	0.42
	VIII	1.00	0.98	0.94	0.86	0.68	0.48	0.28
	Ave.	1.00	0.98	0.95	0.89	0.75	0.53	0.35
Series III	IX	1.00	0.98	0.92	0.88	0.72	0.48	0.24
	X	1.00	0.98	0.92	0.88	0.78	0.54	0.28
	XI	1.00	0.98	0.96	0.88	0.74	0.56	0.34
	Ave.	1.00	0.98	0.93	0.88	0.75	0.53	0.29

TABLE IV

Ob. Ba.

	Fractions	Number of Dots						
		4	5	6	7	8	9	10
Series I	I	0.98	0.96	0.90	0.74	0.14	0.00	0.00
	II	1.00	0.96	0.96	0.64	0.12	0.04	0.00
	III	1.00	0.98	0.88	0.68	0.24	0.02	0.00
	IV	1.00	0.96	0.92	0.70	0.22	0.02	0.00
	V	1.00	0.96	0.90	0.72	0.40	0.16	0.02
	Ave.	1.00	0.96	0.91	0.70	0.22	0.05	0.00
Series II	VI	1.00	0.98	0.94	0.76	0.48	0.22	0.02
	VII	1.00	0.98	0.92	0.76	0.46	0.22	0.06
	VIII	1.00	0.98	0.94	0.72	0.48	0.24	0.06
	Ave.	1.00	0.98	0.93	0.75	0.47	0.23	0.05
Series III	IX	1.00	0.96	0.88	0.76	0.42	0.24	0.06
	X	1.00	0.98	0.92	0.74	0.24	0.12	0.04
	XI	1.00	0.98	0.94	0.78	0.38	0.18	0.06
	Ave.	1.00	0.97	0.91	0.76	0.35	0.18	0.05

TABLE V

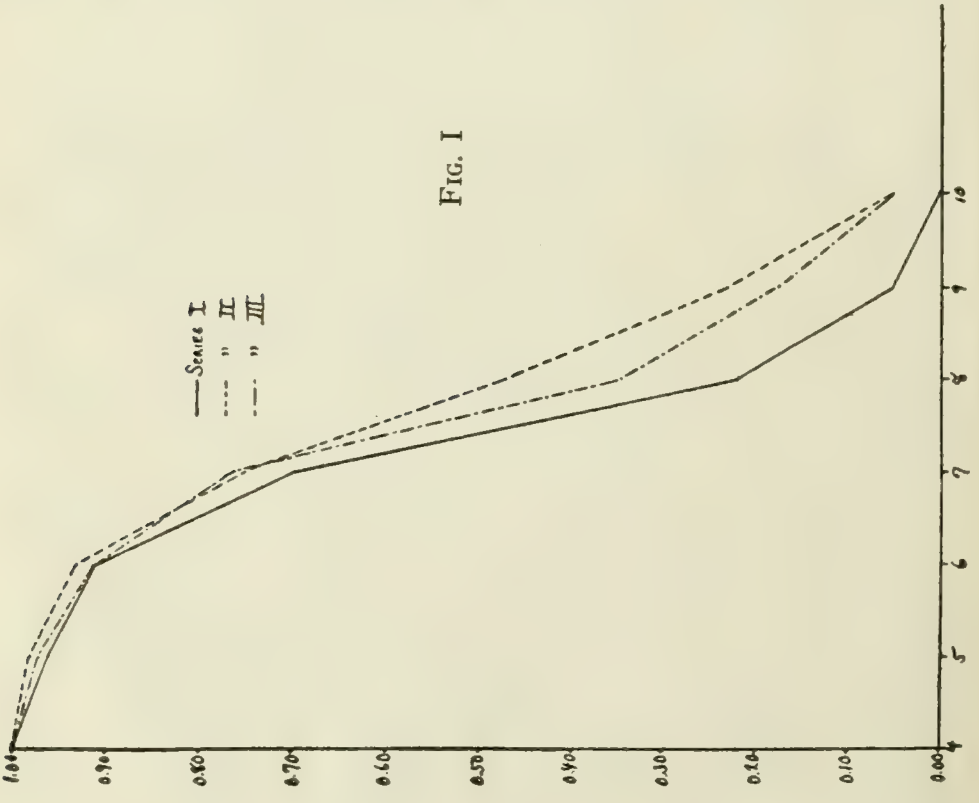
Ob. A.

	Fractions	Number of Dots						
		4	5	6	7	8	9	10
Series I	I	0.68	0.52	0.38	0.22	0.20	0.16	0.10
	II	0.88	0.72	0.50	0.36	0.30	0.16	0.10
	III	0.96	0.88	0.72	0.36	0.34	0.24	0.22
	IV	0.96	0.94	0.68	0.56	0.44	0.24	0.22
	V	1.00	0.94	0.82	0.60	0.42	0.24	0.20
	Ave.	0.90	0.80	0.62	0.42	0.34	0.21	0.17
Series II	VI	1.00	0.98	0.84	0.56	0.36	0.26	0.22
	VII	0.96	0.90	0.78	0.62	0.44	0.28	0.22
	VIII	0.98	0.94	0.80	0.64	0.38	0.20	0.16
	Ave.	0.98	0.94	0.81	0.61	0.39	0.25	0.20
Series III	IX	0.98	0.92	0.78	0.64	0.42	0.26	0.18
	X	1.00	0.98	0.92	0.76	0.56	0.28	0.20
	XI	1.00	0.96	0.92	0.72	0.48	0.28	0.18
	Ave.	0.99	0.95	0.87	0.71	0.49	0.27	0.19

The observed relative frequencies of correct judgments are to be found in Tables I-V, which give for each observer and for every fraction of 50 judgments the relative frequency of correct judgments on every number of dots employed. The averages for each series are averages of the fractions of that series. An examination of these tables shows that we are here dealing with a continuous function. In the case of every group for every observer, the relative frequencies of correct judgments are high for the low values of stimulus and decrease regularly as the number of dots increases. There is not a single inversion in the entire five tables.

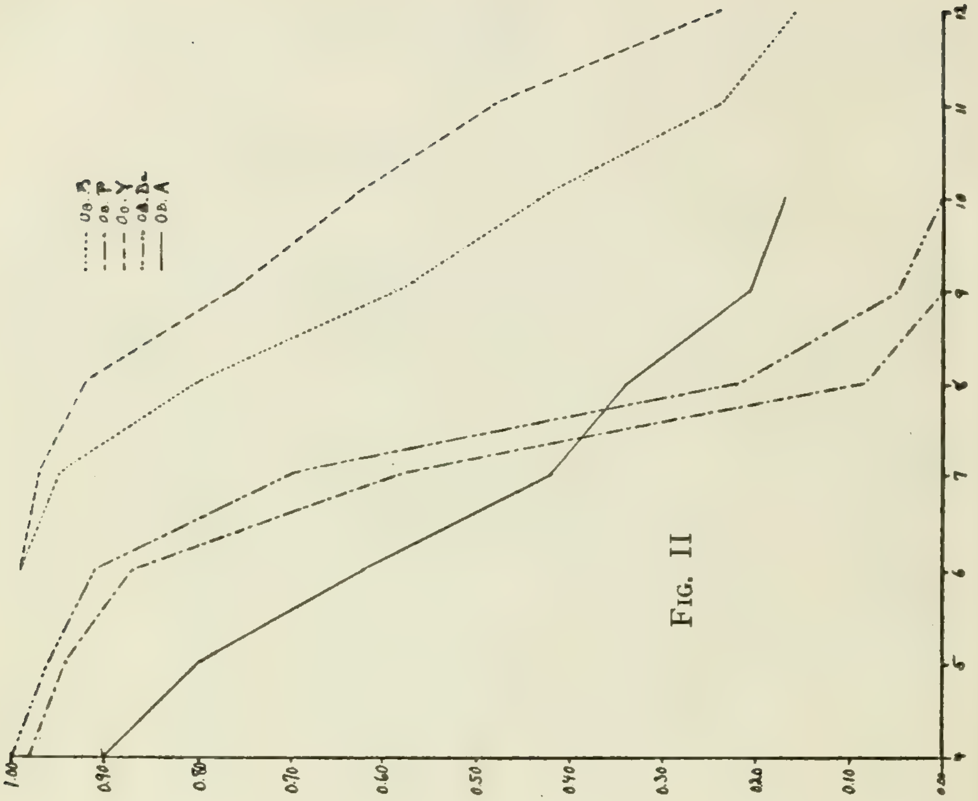
These relations become more obvious when the results are thrown into the form of curves. Figure I gives these curves for Ob. Ba. The average relative frequencies (Table IV) of the three series are plotted. The curves resemble the *phi-gamma function,* but are asymmetrical in that the dispersion of the supraliminal portion is less than that of the subliminal part.

The numerical values of the thresholds (S) and of the coefficients of precision (h) are given in Tables VI-X. It will be observed that both the values of the observed relative



Series I
" II
" III
1.00
0.90
0.80
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
4
5
6
7
8
9
10

Fig. I



Ob. B
Ob. P
Ob. Y
Ob. D.
Ob. A
1.00
0.90
0.80
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
4
5
6
7
8
9
10
11
12

Fig. II

TABLE VI

Ob. B.

Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.314	9.499	0.441	9.554	0.421	10.140
II	0.385	9.755	0.454	9.683	0.416	10.442
III	0.349	9.969	0.434	10.122	0.429	10.204
IV	0.312	9.695				
V	0.450	9.551				
Ave.	0.362	9.694	0.443	9.786	0.422	10.262

TABLE VII

Ob. P.

Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.642	6.939	0.586	7.359	0.555	7.097
II	0.862	6.996	0.644	6.864	0.595	7.376
III	0.880	6.942	0.590	6.848	0.640	7.305
IV	0.501	6.883				
V	0.492	7.246				
Ave.	0.675	7.001	0.607	7.024	0.597	7.259

frequencies and those of the coefficients of precision and the thresholds are relatively similar for the different series for each observer. It was expected that the form of the curves might vary for the different series, thus showing the influence of the brightness of the backgrounds and of the length of the exposure times. No such variation is present for any subject.

Probably the effect of practice obscures any slight differences that might otherwise occur between the series. Practice is evident in the higher values of both the threshold and the coefficient of precision. The effect of practice upon the

TABLE VIII

Ob. Y.

Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.345	10.204	0.365	11.252	0.390	10.884
II	0.331	10.334	0.342	11.535	0.365	11.126
III	0.389	10.374	0.381	10.912	0.365	11.252
IV	0.341	10.954				
V	0.425	11.138				
Ave.	0.366	10.601	0.362	11.233	0.373	11.087

TABLE IX

Ob. Ba.

Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.581	7.282	0.556	7.906	0.469	7.830
II	0.700	7.146	0.518	7.909	0.592	7.565
III	0.700	7.289	0.509	7.934	0.553	7.835
IV	0.687	7.294				
V	0.532	7.650				
Ave.	0.640	7.332	0.527	7.916	0.538	7.743

coefficient of precision is noticeable when the average values of every subject, except Ob. P, are compared. The effect on the thresholds is marked for all subjects, especially for Ob. A. These results accord with former experiments on the effects of practice upon visual apprehension.[10]

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[10] G. M. Whipple, The Effect of Practice Upon the Range of Visual Attention and of Visual Apprehension, *Jour. of Ed. Psychol.*, I, 1910, 249-262; K. M. Dallenbach, The Effect of Practice Upon Visual Apprehension in School Children, *Jour. of Ed. Psychol.*, V, 1914, 321-334, 387-404; K. M. Dallenbach, The Effect of Practice Upon Visual Apprehension in the Feeble-Minded, *Jour. of Ed. Psychol.*, X, 1919, 61-82; cf. *Journ. Exp. Psych., iii*, 1920, 228 f.

TABLE X

OB. A.

Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.204	4.974	0.336	7.757	0.338	7.768
II	0.276	6.435	0.303	7.825	0.413	8.276
III	0.290	7.291	0.373	7.660	0.400	8.102
IV	0.299	7.632				
V	0.339	7.776				
Ave.	0.282	6.822	0.337	7.747	0.384	8.049

Figure II gives a comparison of the results for the different observers. The psychometric functions for each subject in Series I are plotted together. The graphs illustrate the relative positions of the thresholds for the five observers. Ob. Y has the highest values, ranging for all of the three series about 11 dots. Next lower are the values for Ob. B, ranging about 10 dots. The values for Ob. Ba are about 7 to 8 dots; those for Ob. A in the neighborhood of 7 to 8 dots; and those for Ob. P are the lowest, being very close to 7 dots.

It seemed also of interest to determine just what were the mental processes underlying the formation of these judgments of visual apprehension. For this purpose, some introspections were obtained from each of the subjects. An analysis of these introspections reveals the following facts.

For Ob. B, the process was essentially one of utilizing the immediate memory after-image after the exposure was completed and of grouping the dots for purposes of apprehension. This grouping was present even for the lower values of stimulus (6 and 7 dots for this subject). The following introspection is typical for this observer:

> "This time the judgment came very quickly and was accompanied by an attitude of great subjective certainty. The spot-pattern was very distinct and the dots were very uniformly black and very sharply outlined. After the exposure, there was a visual memory after-image left for a moment after the light had streaked by. The counting in vocal-motor terms began while this was still present. The left part of this image was most distinct and was counted first. Fixation, with accompanying kinaesthesis of effort, of the exposure square persisted until the counting was finished. The counting was in groups with eye-movements from the location of one group to that of the next as I counted."

The process for Ob. P were very rapid and the judgment was reported as having been formed during the time of the actual exposure. Eye-movement did not seem to be present but, even though the exposures were as short as 100 and 60 sigma, there was grouping. This grouping was present, apparently, in shifts of clearness and of focality. The following is a typical introspection:

"When the shutter was removed, there was intensive, but indistinct visual perception of a light and rather extended area. This first immediate visual perception was rapidly followed by a standing-out, with respect to clearness, of the central portion of the area. In this central portion, I perceived focally the three dots (which formed the upper portion of the figure). Then there was a rapid shift in focality in which I perceived very clearly the remaining dots of the figure. There was also vocal-motor imagery, very rapid, telescoped and non-focal, present with the changing focality of the dots, of 'four—seven.' This was then vocalized and general relaxation followed."

Ob. Y's processes were very similar to those of Ob. P. The processes were rapid and the judgment was reported as being completed by the end of the exposure-period. Grouping was present, again in terms of shifts of clearness and focality. The following introspection will illustrate the processes employed:

"I had a clear auditory perception of the 'ready' signal, which was followed immediately by the clear perception of the dots arranged in the form of an uneven square. The attention then shifted to the uper right-hand part of the figure. Rapid perception of four dots which stood out very clearly. Then I had a clear perception of three dots in the lower left-hand part of the figure. Then immediately I had a very clear perception of another group of three dots in the central part of the figure. Then followed the verbalization 'ten' which was followed by intensive pleasantness."

Observers Ba and A were not so highly trained in introspection as were the other subjects. Still their protocols give an insight into the processes present. Ob. Ba employed a process very similar to that reported by Ob. B. A visual memory after-image of the stimulus was present immediately after the exposure. Grouping was present in terms of clearness-shifts in this image. The following introspection illustrates the process employed:

"There was first a perception of the stimulus as a whole, no part of it being any clearer than any other part. This was folowed by a visual memory after-image which lasted only a very short time. Then a true visual image of the stimulus came in. First the left-hand side was clearest and I had a vocal-motor 'four.' Then the right-hand side became clearer and I had a vocal-motor 'three.' Then the entire image became fairly clear and this was immediately followed by the verbalization of 'seven'."

Observer A employed an immediate visual after-image, and grouping was present in terms of clearness-shifts in this image. The following typical introspection is given:

> "Immediately after the exposure I had a visual memory after-image of the stimulus. At first three dots in the upper portion of the figure were very clear. This was followed almost simultaneously by a standing-out clearly of two dots in the lower portion of the figure. Then an image in which all five dots were clear and focal came in rather suddenly and gradually faded out. The judgment was then verbalized."

A consideration of these introspections reveals that grouping was always present for all of the observers. The exposure-times which were employed were too brief to admit eye-movements. Still the splitting up of the total stimulus into groups was accomplished by means of shifts of clearness and of focality. The observers may be divided into two distinct types. For Obs. Y and P this grouping of the dots by means of clearness shifts was reported as being accomplished during the actual exposure of the stimulus, and the judgment was completed by the end of the very brief exposure-time. For Obs. B, Ba and A, this grouping took place in a visual memory after-image which appeared immediately after the exposure was completed. One might hope here to discover a basis for the differences in the size of the limen between the different subjects but, unfortunately, the hope is disappointed. The grouping on the side of process is: Immediate type of judgment, Obs. Y and P; use of visual memory after-image, Obs. B, Ba and A. The grouping of subjects on the basis of limen-values is: Higher values, Obs. B and Y; lower values, Obs. Ba, P and A. The groups in the two cases do not agree. Ob. Y, who employed the immediate type of judgment, had the highest limen-values; while Ob. P, who employed the same type of process, had the lowest.

From a consideration of the introspections we are convinced that the range of attention is an erroneous title for this sort of experiment. We have, therefore, followed Whipple and Dallenbach in calling the problem, in this sort of experiment, the range of visual apprehension.

Conclusions

1. The observed relative frequencies of correct judgments in experiments upon range of visual apprehension follow a continuous function of ogive form
2. The statistical limen, that stimulus-value for which correct judgments are given in 50 *per cent.* of the cases, is the most reliable measure of the range of visual apprehension.
3. In our experiments the individual limens for simultaneous visual apprehension ranged from six to over eleven stimulus objects (dots).

EMOTIONS AND INSTINCTS

By Henry C. Link

For many years the instincts have been identified, to a greater or lesser extent, with the emotions. In 1880, G. H. Schneider[1] gave the first developed expression of this identity. James soon after suggested the co-ordination between instinct and emotion in two successive chapters under these titles.[2] But the most ambitious attempt to establish this identity is that made by W. McDougall, who even couples specific emotions with specific reactions.[3] For example, he identifies the emotion of anger with the instinctive reactions of pugnacity, the emotion of fear with the instinct of flight, disgust with repulsion, wonder with the actions of curiosity, the tender emotion with the parental instinct, elation with self-asserting activities. He names other instincts and emotions such as gregariousness, the constructive instinct, and hunger; but these, he says, have no definite corresponding emotion.

The question which all of these views promote is the question as to the character of this identity. First of all, are the emotions and instinctive motor reactions identical in time? It is often stated that they are, and we are sometimes told that the emotions are but the subjective aspect of the instincts. However, the tendency is to regard either one or the other as prior. The James-Lange theory insisted upon the priority of the instinctive response and the subsequent presence of a characteristic emotion. James himself repudiated this view to a certain extent when he admitted that the perception of a total situation, and not a mere physical object, initiated both the emotion and the instinctive response.[4] The extensive experiments of Sherrington, described especially with reference to their bearing upon the James-Lange theory, led him to assert as his theory that the bodily changes and the psychic factor or the emotion occur concomitantly.[5] Sherrington also says that " of points where physiology and psychology touch, the place of one lies at emotion."[6] Further experiments, made by W. C. Cannon, confirm this theory. Cannon says: " We do not feel sorry because we cry, as James contended, but we cry because when we are sorry or overjoyed or violently angry . . . there are nervous discharges by sympathetic channels to various viscera, including the lachrymal glands."[7] That is, the two are simultaneous as far as time is concerned. In fact, it is difficult to see how one could be found to precede the other. In animals, for example, where, as Sherrington says,[8] we have to infer the character of the emotions from the physical manifestations, it is impossible to say whether one or the other comes first. In ourselves it is impossible to decide because, when our attention is turned to the active side of emotion, we are only secondarily aware of the emotion itself; whereas, when we are most conscious of the emotion, we are only secondarily aware of its motor accompaniments.

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[1] G. H. Schneider, *Der thierische Wille,* 66, 96, 1 and 6 ff.

[2] *Principles of Psychology,* vol. ii.

[3] *Social Psychology,* 45 ff.

[4] W. James, "The Physical Basis of Emotion," *Psychological Review,* i, 1894, 523 ff.

Another way in which instincts and emotions are identified is by defining them both as dependent upon the same bodily mechanisms. Each emotion has its corresponding visceral and organic activities and characteristic bodily expression. This, in its most extreme form, is the view held by McDougall, who says: " In the case of the powerful instincts, the affective quality of each instinctive process, and the sum of visceral and bodily changes in which it expresses itself, are peculiar and distinct."[9] However, the experiments of Sherrington and Cannon may be regarded as having disproved this theory. The view that the differentiated features of emotions are due to the viscera was discredited by Sherrington, when he demonstrated that emotional responses occur in dogs in which practically all the main viscera and the great bulk of skeletal muscles have been removed from subjection to and from influence upon the brain, by severance of the vagus and spinal cord. He admits that the visceral reactions strengthen the emotion; they are nevertheless of relatively small importance " as compared with the cerebral reverberations to which is adjunct the psychical component of emotional reaction."[10] In support of this view, W. B. Cannon says: " The evidence which I have given above, as well as that which he (Sherrington) has offered, favors the conclusion that the viscera are relatively unimportant in an emotional complex, especially in contributing differential factors."[11] On the other hand, d'Allonnes[12] points out, very shrewdly, that Sherrington's experiments would upset the peripheral theory only if we could prove that animals actually experience the emotions whose signs they manifest. He himself takes the opposite view, basing his conclusions upon observations on a woman who, having lost her visceral sensibilities of pain, hunger, thirst, and fatigue, exhibited all the signs of anger, fear, chagrin, etc., but at the same time asserted that she did not feel any of these emotions. In this case, the proof hinges upon whether or not we can believe what the subject said.

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[5] *Integrative Action of the Nervous System*, 258.

[6] *Op. cit.*, 256.

[7] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 280.

[8] *Op. cit.*, 266.

[9] *Social Psychology*, 46.

[10] *Op. cit.*, 268.

From the above testimony it must be apparent that the physiological method with animals can never demonstrate what connection exists between emotions and bodily activities. In the first place, it begins with the assumption that certain acts express certain emotions, which in itself is a questionable assumption. In the second place, it concludes that, whenever the characteristic motor expressions are present, as for example the retreat of a dog as if frightened, the emotion is also present, and whenever these motor expressions are absent, the emotion is also absent. Goltz, for example, made experiments upon dogs which showed that when the vascular and visceral fields had been severed from connection with the head, the animal was still able to perceive objects and react to them with the characteristic motor responses of joy, fear, excitement, etc., while the dog whose cerebral hemispheres had been removed was able to respond only to the affective situations of pain and pleasure.[13] What such experiments can prove at the most is that certain characteristic motor or organic reactions are dependent upon certain parts of the nervous or vasomotor system, but they cannot prove that those reactions are invariably connected with the emotions they are supposed to represent.[14]

Watson cuts through this difficulty by discarding, almost entirely, the traditional distinction between instinct and emotion. Instead of regarding emotion as the subjective or psychological aspect of instinct, he regards it simply as a slightly different type of motor response. " Emotion is an hereditary

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[11] *Ibid.*, 283.

[12] G. Revault d'Allonnes, *Les inclinations, leur rôle dans la psychologie des sentiments*, 1908, 228 ff.

[13] Sherrington, *op. cit.*, 263, 266.

[14] *Op. cit.*, 266;; E. B. Titchener, *Text Book of Psychology*, 481 ff.

'pattern-reaction' involving profound changes in the bodily mechanism as a whole, but particularly of the visceral and glandular systems."[15] Whereas "instinct is an hereditary pattern-reaction, the separate elements of wihch are movements principally of the striped muscles."[16] Without discussing the refinements of these definitions we easily see that they indicate a radically different conception of the whole subject of emotion and instinct. By defining instinct and emotion both as somewhat different but essentially similar objective phenomena, Watson avoids, for all practical purposes, the many confusions and philosophical speculations which have characterized the studies dominated by the James-Lange point of view. Whether the differentiation he makes will prove valuable is another question. At least we have here a statement which recognizes the futility of the traditional views and sets out on a track which has a fair chance of being proved or disproved objectively.

It seems likely that the attempt to define and classify emotions through introspection can never become more than purely conjectural. The endless variety of opinion which we find among psychologists as to the character and number of the emotions is eloquent testimony to the futility of this procedure. It is the very nature of emotion to connect itself with almost any objective expression whatsoever, and hence, from a subjective point of view, it is hopeless to try to establish even relatively invariable connections between definite reactions and emotions. The tendency for emotions to become more and more diffuse, and to be identified with an ever-increasing range of objects and interests, but confirms this statement. James, although he associated instinctive responses with emotions, clearly recognized this difficulty when he said: "In short, every classification of emotion is seen to be true and natural as any other, if it only serves some purpose; and such a question as 'what is the "real" or typical expression of anger or fear?' is seen to have no objective meaning at all."[17]

In spite of this obvious problem some psychologists have presented classifications of emotions and ascribed to them objective phenomena with a completeness and certainty which is certainly not warranted by the facts. McDougall, for instance, begins with a certain number of innate instinctive emotional dispositions, and upon these as a basis constructs the entire fabric of life as it is found today. "The instinctive impulses," he says, "determine the ends of all activities and supply the driving power by which all mental activities are sustained; and all the complex intellectual apparatus of the most highly developed mind is but a means towards these ends, is but the instrument by which these impulses seek their satisfactions."[18] McDougall enumerates a group of primary instincts, about seven in number, each of which he claims is differentiated by a distinct emotional core and a characteristic bodily activity.[19] Upon these instincts, then, he proceeds to build the various institutions, customs, habits, and interests which characterize society. He traces to them the development of morals; of religion; of the higher sentiments of love, hate, and respect; of self-consciousness, volition, ideals; in short, of the whole range of life. A more thoroughgoing psychological rationalism it would be difficult to imagine. The very comprehensiveness of the scheme gives it an increased plausibility, for, having begun with the statement that the instincts are the factors which determine and sustain "all" the activities of life, he has not omitted to catalogue all those activities under some instinct or other.

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[15] *Psychology from the Standpoint of a Behaviorist,* 195.

[16] *Op. cit.,* 231.

[17] *Psychology,* 381. f.

In the first place, what is the principle by which McDougall distinguishes the primary emotions and instincts? "Each of the principal instincts," he says, "conditions some kind of emotional excitement whose quality is specific or peculiar to it; and the emotional excitement of specific quality that is the affective aspect of the operation of any one of the principal instincts may be called a primary emotion."[20] This principle, McDougall holds, is "of very great value when we seek to analyze the complex emotions into their primary constituents."[21] However, this is merely a postulate or an arbitrary statement of what McDougall intends that an instinct-emotion shall mean. Whether such entities exist, and how we can distinguish them if they do exist, is still the problem. McDougall suggests two principles which he thinks will be of great help in picking out the primary instincts. The first is that "if a similar emotion and impulse are clearly displayed in the instinctive activities of the higher animals, that fact will afford a strong presumption that the emotion and impulse in question are primary and simple."[22] The second principle of discovery is to inquire whether the emotions and instincts in question are subject to morbid hypertrophy or excitability.[23]

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[18] *Op. cit.,* 33.
[19] *Ibid.,* 45 ff.
[20] *Ibid.,* 47.
[21] *Ibid.,* 47.
[22] *Ibid.,* 49.
[23] *Ibid.,* 49.

With regard to the first of these criteria, the analysis presented above suggests the trend of our criticism. If, in the first place, we infer the presence and quality of emotions in animals by comparing their attitudes with our own, how can we legitimately infer the number and kind of our own primary emotions by referring them to the analogy which we have created? To create an object by introspection, and then to use it as an objective criterion by which to establish the primal quality of our own emotions, is indeed a vicious circle. Moreover, the claim that " in case of the powerful instincts, the affective quality of each instinctive process and the sum of visceral and bodily changes in which it expresses itself are peculiar and distinct "[24] is not borne out by the facts. Even if it were possible to take the higher animals and base a deduction of the emotions upon the instinctive activities which they manifest, it would still be impossible to draw this inference; for, as the above analysis shows, the motor reactions which characterize certain affective processes are dependent upon visceral disturbances in only a minor degree. As for the second criterion, it seems reasonable to think that if each instinctive emotion is a " relatively independent unity in the constitution of the mind," it would be liable to morbid hypertrophy or abnormal excitement. However, the forms of abnormality display as great a complexity as the forms of normal life, and hence do not offer that decisive individuality which the given definition of instinct would lead one to expect. And if it is difficult to connect typical acts with primary emotions in our own minds, and impossible to do more than conjecture the nature of the connection in animals, how much more difficult is it to connect the disarranged and abnormal activities of an abnormal person with any particular emotion? Psychiatry has succeeded in connecting ideas with ideas, events with events; but it has only partly suggested the exact nature of the connection between specific acts and specific emotions.

The choice of instinctive emotions which McDougall makes clearly bears out the above criticisms. For example, the gregarious instinct, according to McDougall, is not marked with an emotion " sufficiently intense or specific to have been given a name ;"[25] nevertheless, he attributes to it the tremendous growth of cities and an immense range of social phenomena. The same may be said of the constructive instinct and the instinct of acquisition. Both of these are responsible, in McDougall's social structure, for a great deal, in spite of the fact that they are not marked by any specific emotion.

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[24] *Ibid.*, 46.

[25] *Ibid.*, 86.

On the other hand, joy, which is thought to display itself in one of the most immediate and manifest responses in some of the higher animals,[26] is listed by McDougall among the sentiments. The instincts of subjection and elation which McDougall considers primary are obviously of a questionable character. They are so general that they may be interpreted into almost any situation. Finally, the principle which governs McDougall's choice of instincts is too susceptible to inversion. By that I mean that McDougall is more intent upon reading the facts back into instinct than upon reading the facts out of the instincts. Given a certain class of phenomena, he believes that there must be some instinct to account for it, and he never fails to find one appropriate. Strangely enough, McDougall severely criticizes this kind of procedure, giving as an example the statement made by V. Cousin as the fundamental assumption of his philosophy of history. This statement is as follows: "The various manifestations and phases of social life are all traced back to tendencies of human nature from which they spring, from five fundamental wants, each of which has corresponding to it a general idea. The idea of the useful gives rise to mathematical and physical science, industry and political economy; the idea of the just to civil society, the State, and jurisprudence; the idea of the beautiful to art; the idea of God to religion and worship; and the idea of truth in itself, in its highest degree and under its purest form, to philosophy. These ideas are argued to be simple and indecomposable, to co-exist in every mind, to constitute the whole foundation of humanity, and to follow in the order mentioned." McDougall then adds: "We have here the spectacle of a philosopher, who exerted a great influence on the thought of his own country, and who rightly conceived the relation of psychology to the social sciences, but who, in the absence of any adequate psychology, contents himself with concocting on the spur of the moment the most flimsy substitute for it in the form of these five assumptions."[27] McDougall's criticism is just. He himself has the advantage of a knowledge of comparative psychology, and hence has been able to identify man with the brute. This always creates a presumption in favor of the profundity of an analysis. However, as far as scientific coherence is concerned, McDougall's presentation may seem as fanciful to future psychologists as Cousin's statement does to present day philosophers. The criticism which Titchener makes of McDougalls' procedure is applicable here. He says that the list of instincts which McDougall presents is "a matter of individual preference rather than of scientific finality."[28] Titchener's criticism points out the root-fallacy in McDougall's work. The very nature of the situation is such as to make a scientific formulation of instinctive emotions impossible. And if it is impossible to distinguish the separate instincts and "driving" emotions, it is futile to attempt to trace to them the particular facts and values which now confront us. This is the more true because, as McDougall himself says,[29] the emotional centers, through their cognitive receptors, can come to enforce almost any particular motor expression or fact. Taking into consideration, therefore, the difficulties mentioned above, it is impossible to see to what practical result McDougall's list of instincts can lead. McDougall's confessed purpose is to make a contribution to the Social Sciences by analyzing the dynamic and determinative factors in human nature. However, he has left us with a mosaic of facts which are connected only in a schematic way. Beginning with a handful of supposedly specific, but in actuality hopelessly vague causes, he has left us with an infinitude of results, all obvious but all more or less irrelevant.

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[26] *Integrative Action of the Nervous System,* 265 f.

[27] *Op. cit.,* 12 f.

However, McDougall's analysis is admirable in its attempt to account for the dynamic nature of instinct by means of emotive forces. No matter what view is held about the number and kind of the emotions, the general opinion is that they are, as the word suggests, moving forces, "contributing not a little to the energy of behavior" (Morgan). Granting, for the time being, that there is a determinate number of instinct-emotions, and that they are the core around which all subsequent modifications and growth take place, we are led to ask: How, or by what principle, do these emotions combine to form the various purposes and values which we possess? How, in the event of a conflict between the various emotions, is the conflict resolved? Or, if the various emotions give rise to various values, all more or less independent, what will be the criterion of the 'relative' value of the desires which these emotions reinforce? McDougall states this problem and explains it by saying: "In the absence of sentiments our emotional life would be a mere chaos, without order, consistency, or continuity of any kind; and all our social relations and conduct, being based on the emotions and impulses, would be correspondingly chaotic, unpredictable, and unstable. It is only through the systematic organization of the emotional dispositions into sentiments that the volitional control of the immediate promptings of the emotions is rendered possible. Again, our judgment of values and of merit is rooted in our sentiments; and our moral principles have the same source, for they are formed by our judgments of moral values."[30] The problem which is vital to the discussion and which McDougall names only to ignore is this: How are the emotions organized into a system which shall thenceforth determine the manner in which those emotions shall express themselves? In answer to this question, we must say that in so far as McDougall regards the emotions as fixed and fundamental forces, he has made it impossible for them to create a principle by which they shall be subordinated. On the other hand, in so far as he admits that the instincts are subordinated and harmonized by some other force, or by other causes, he has introduced a principle which is in direct contradiction to his definition of the instincts.

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[28] *Textbook of Psychology,* 480.

[29] *Op. cit.,* 34 f.

The existence of some factors other than the particular emotions themselves, which determines how they shall express themselves, is generally recognized. It shows itself in the tendency, on the one hand, to analyze each emotion into elements more and more discrete and minute, namely, into sensation qualities; and, on the other hand, to reduce the separate emotions to two or three comprehensive emotions. All such explanations, whether they assume as fundamental a few general emotions, or analyze the general emotions into minute elements, show the same difficulty, namely, the impossibility of explaining the unity of the emotions in terms of units which, by definition, are discrete.

In pleasantness and unpleasantness, however, we have two factors which, in a sense, unify the various emotions by giving them a common character. All emotions are affectively characterized by pleasure or unpleasantness, and it is this common element which determines the direction of the emotion. It is often said that pleasure and unpleasantness serve to guide the emotions, rather than actively to direct or determine them. What it means to guide rather than to direct activity may be decided by those who insist that the man behind a flock of sheep is directing and driving the flock while the men on either side are merely guiding them. It is also very commonly held that pleasure and its opposite are merely signs indicating that the body is functioning normally or abnormally. This is to liken them to sign-posts or to static symbols between which the emotions proceed upon their path. It is impossible to interpret the affective processes in this negative fashion without destroying their real significance. The reason, probably, why the affective aspect of consciousness has come to be regarded as merely a "guide" is because it is usually diffuse and is concomitant with almost every conscious state. This, however, is not a good reason for depriving feeling of its dynamic significance. If we regard the emotions as dynamic, we must also regard pleasure and unpleasantness as dynamic. In fact, it is quite likely that pleasure and unpleasantness are more fundamental than the emotions, and that not only genetically but actually. The experiments which Goltz performed on dogs, for example, showed that the most persistent and durable reactions were those induced by a stimulus calculated to produce pleasure or pain.[31] Genetically, it is likely that the emotions were subsequent to the more general affective process of pleasure and unpleasantness. However, we must take one more step and ask: What determines the nature of the affective response, whether it be pleasurable or the opposite? Is it some single emotion? Hardly, because then the word as we have been using it would become meaningless. An emotion which determines all other emotions could certainly not be an emotion in the sense in which we have been using the word. The text-books answer this question by saying in several ways the same thing, namely, that when the organism is getting along well or functioning normally, the pleasurable state predominates; while, when the organism is getting along poorly, the unpleasant state predominates. But what does this imply? It implies that there is an organism which has a normal course, a positive and dynamic character of some kind, or possibly a purpose; and when this normal course, whatever it may be, is thwarted, displeasure ensues. When it proceeds normally, pleasure ensues. That is to say, no particular emotion and no particular feeling is the fundamental dynamic factor in life.

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[30] *Social Psychology,* 159 f.

Conclusions

The classical concepts of emotion and instinct, represented in the controversies which center around the James-Lange theory, are a hindrance rather than a help to future studies in this field. By regarding emotion as a psychical phenomenon, and instincts as the bodily changes which characterize the emotion, it is made absolutely impossible to establish any causal identity, or even a schematic identity, between the two.

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[31] *Integrative Action of the Nervous System,* 266.

Valuable objective studies, such as those of Goltz with dogs, are only made confusing by attempts to interpret into the bodily reactions the emotions which introspection has named. Therefore, the ambitious attempts on the part of some writers to identify instinctive responses with a group of emotions arbitrarily selected is open to serious criticism. And when the entire range of individual and social phenomena is attributed to such a group of uncertain causes, only the most fanciful and uncritical imagination can follow the process by which it is done. But even if we admit the validity of the assumption upon which such a superstructure rests, there is another logical difficulty which we encounter. If we assume, as does McDougall, that the emotions are the fundamental forces in the organism, and that these emotions are fixed in character, it becomes logically impossible to explain the process by which these conflicting forces are unified. How can independent forces give rise to a working principle or " sentiment " which is more powerful than the fundamental forces themselves? This difficulty is recognized in those views which seek to interpret all emotion in terms of the affective processes of pleasantness and unpleasantness. But the analysis of pleasantness and unpleasantness leads us again to the conclusion that even these affective processes derive their significance from some positive content of the organism itself, a content which the current theories of instinct and emotion have not yet exhausted. Whether or not we are able to give to this content any objective meaning is highly problematical. Our present purpose has rather been to point out certain contradictions and absurdities which characterize present thinking in this field, and particularly to call attention to the highly speculative character of the concoctions in which instinct-emotions are used as the prime ingredients.[32]

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[32] Save for the paragraph on Watson's view, this paper was written some time before the appearance of Kantor's *A Functional Interpretation of Human Instincts* (*Psych. Rev.*, xxvii, 1920, 50ff.). With slight differences, the writer is in substantial accord with the conclusions reached by Kantor. Indeed, the similarity of the criticisms of McDougall is striking. Kantor, however, has succeeded the better in giving consistent expression to the functional point of view. The writer hopes soon to present his own conclusions in more comprehensive form.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

XLI. The Relation of the Pleasantness of Color Combinations to that of the Colors Seen Singly

By M. F. Washburn, Dorothy Haight, and Jeanette Regensburg.

Professor Geissler contributed to the Titchener Commemorative Volume a study of the affective tone of color combinations, the most positive result of which was to show that "the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination." The materials used were the following saturated Bradley colors; red, orange, yellow, green, blue-green, blue, and purple. These were shown in areas of 5 cm. a side, placed side by side to make a strip 10 cm. wide. When single colors were to be judged, the left half of this strip was covered. The background was grey. The method was that of Paired Comparison; one color combination, or one single color, being shown 5 cm. above the other.

The design of our own experiments was to see whether the principle that the pleasantness of a color combination varies directly as that of its components would hold good when absolute rather than comparative judgments of affective value were made, and in general to see what light could be thrown on the problem by altering the method. Instead of the Method of Paired Comparison, we used the one which has been so often employed in our laboratory, that of presenting a single impression, either a color combination or a single color, and asking the observer to express his judgment of its pleasantness by using the number 1 to mean 'very unpleasant,' 2 'moderately unpleasant,' 3 'slightly unpleasant,' 4 'indifferent,' 5 'slightly pleasant,' 6 'moderately pleasant,' and 7 'very pleasant.' Another important difference between our method and Geissler's lay in our *use of shades and tints as well as saturated colors.* We used twelve saturated colors of the Bradley series, twenty-four shades, and twenty-eight tints. The colors were pieces 3 cm. square, pasted on white cards 2½ by 3 inches. Where color combinations were used, two of these squares were pasted side by side on the same card. Each of the two experimenters used a different set of colors, comprising thirty-two of the sixty-four single colors, which were shown and judged first, and sixteen combinations of these colors, which were shown and judged later in the same sitting. There were thus in all thirty-two combinations used. The number of observers, all women college students, was 105 for one experimenter, 106 for the other.

The relation between the pleasantness of the combinations and that of the single colors was found in the following way. The total affective value of every color, when shown alone, was determined by adding the numbers by which the observers expressed their judgments of its pleasantness; the sum, of course, would be larger, the pleasanter the color was found by the observers as a group. Then the sums thus obtained for each of two colors that were shown in combination were added; the combined sum represented the combined affective value of these colors when they were shown singly. This being done for each of the pairs of colors, it was possible to arrange them in an order representing their pleasantness as determined solely by their appearance singly. Then the numbers which represented the affective judgments of the observers on the combinations as such were added, and the combinations arranged in the order of the size of these sums. Evidently the rank difference correlation between these two arrays will give the degree of relationship between the pleasantness of a color combination and the individual pleasantness of its component colors. This correlation proved to be plus .747, with a P.E. of .0119. It is clear that to a very considerable extent, the pleasantness of a color combination depends upon the pleasantness of the individual colors.

But this fact can hardly be taken, as Professor Geissler seems to take it, for a manifestation of the simple summation of feelings. If the agreeableness of a color combination is due to summation of the agreeableness of its components, or its disagreeableness to summation of the disagreeableness of its components, then a combination of two pleasant colors should be pleasanter than either of the components when seen singly; and a combination of two unpleasant colors should be unpleasanter than either of the components seen singly. This follows from the orthodox doctrine of the simplicity of feeling tones. Now in the entire series of our experiments, in which thirty-two combinations were used, with over a hundred observers judging each combination, there were 861 cases where the component colors, when judged singly, were both found agreeable. In 263 of these cases, or 30.5%, the combinations of these colors were found positively disagreeable. There were 465 cases where the component colors, judged separately, were both found disagreeable; of these 72, or 15.4%, were agreeable in combinations. These figures support the conclusion to which ordinary experience points, that *the unpleasantness or pleasantness of a color combination is derived not merely from summation of the affective tones of its components, but from another factor dependent on the combination itself.* Our results suggest also that this factor more often gives rise to unpleasantness than to pleasantness; that it is easier to get an ugly combination out of two pleasing colors than a pleasing combination out of two disagreeable colors. It may, however, be true that our thirty-two combinations happened to include more cases where the combination factor operated to produce unpleasantness. The relation of the two factors, that derived from the component colors which is responsible for our correlation of plus .74, and that derived from the combination, may be conjectured to depend on whether attention is attracted to the colors as separate colors, when they appear in the combination, or to the total effect.

BOOK REVIEWS

Lehrbuch der experimentellen Psychologie. By J. FRÖBES, S.J. Herdersche Verlagshandlung, Freiburg. Bd. I, 1917, 605 pp.; Bd. II, 1920, 704 pp.

The first impression that one gets from this book is an impression of exceptional bulk; a royal octavo of 1,300 pages closely printed in large and small type, and with relatively few figures! The second impression is that of a wealth of citations from other works: the *Autorenverzeichnis* contains over 1,300 names (300 more than the fifth edition of Wundt's *Grundzüge*); and the author assures us, in the preface, that with very few exceptions he has quoted no book or monograph which he has not read through and judged for himself. Moreover, some of the writers are cited not once only, but many times; Wundt and G. E. Müller over a hundred, Fechner forty-six times; between these extremes come in descending order of frequency Lehmann, Meumann, Ebbinghaus, Titchener, Ribot, Stern and James, and Helmholtz. Our author has evidently not only read diligently, but has also chosen wisely. It was his aim, as he tells us again in the preface, to write a text-book of Psychology of the order of Tigerstedt's *Lehrbuch der Psychologie* or of Ebbinghaus' *Psychologie.* Furthermore, the book is designed to serve as an introduction not only to experimental psychology, as the title might lead us to suppose, but also to the other branches of psychology. Finally, the writer says that he has not found it necessary to give in the present book an exposition of his psychological faith. This he hopes to do in another place at some future time; in the meanwhile, the reader who is curious to know more on the point may find views similar to his own in Geyser's *Lehrbuch der allgemeinen Psychologie.*

The first volume of Fröbes' book is devoted to what he regards as the elementary psychological processes. It comprises a short introduction; two long sections, the one on sensation and feeling, the other on perception; and two shorter sections on psychophysics and on the association of ideas. The second volume is concerned with the 'higher mental activities.' It begins with a transitional section on cortical localization and derangement of associations. Then follow four long sections on *intellectual processes* (attention, the self, memory and learning, intelligence, creative imagination, and language); on *emotion* (emotion in general, classification of emotions, the aesthetic feelings, and expressive movements); on *will* (movement and reaction, the will-consciousness, custom and religion, and personality and mental development); and finally on *anomalies of consciousness* (sleep and dreams, hypnotism and suggestion, and mental pathology in general). Under some of these chapter-headings are materials of differential psychology, social psychology, educational psychology, juristic psychology and the psychology of aesthetics. An enormous programme! What does the author make of it?

He defines experimental psychology as the "science of mental processes, their laws, and their connection, as based upon observation and experiment." By the term 'mental processes' he means psychical phenomena or phenomena of consciousness, the objects of inner experience. He dismisses definitions by point of view (of which he refers to Wundt's as an instance) solely by the assertion that physics and psychology have no objects in common. The problem of psychology is the description and explanation of the phenomena of consciousness; explanation, of sensation at least, is to be found in physical objects and their effect upon the nervous system. All this is said in the most summary fashion. Fröbes devotes, however, nine pages to method. He recognizes as methods of psychology introspection and observation of behavior (*Fremdbeobachtung*). His treatment of the former is mainly an abstract of the views of G. E. Müller; concerning the latter he follows in the main W. Stern. His notion of sensation is reminiscent of James, Höfler and Geyser: sensations are the elements of the more objective processes, the *Erkenntnisvorgänge;* they are items of knowledge of attributes which belong not to the knowing subject but to the sensory describable object; they combine to form perceptions; and they are also the elements which, at still higher stages, give rise to conceptions and judgments. Every sensation has four attributes, quality, intensity, temporal duration and spatial extension; the two latter, however, do not in Fröbes' thinking belong to sensation in the narrow sense; they are rather 'formal elements,' which assist in the formation of perceptions. Simple feelings, on the other hand, he regards as the elements of the 'more subjective processes' like Emotion and Will. They are dependent upon sensations, but are differentiated from them principally by virtue of their subjectivity. Fröbes also recognizes simple and complex ideas (*Vorstellungen*); the former he considers as 'renewals' (*Erneuerungen*) of earlier simple sensations; the latter are complexes of simple ideas, as perceptions are complexes of simple sensations. Although he discusses differences in intensity, in activity, in direction of attention (Fechner), and Müller's 'indistinctness' of normal images, as marks of differentiation between sensation and simple idea, he concludes that no one of them is essential. In all these systematic questions, however, one feels that the author is not seriously interested; the definitions seem to be merely introductory, purely formal, and are given as if they were to be expected and must be got out of the way. Moreover, the simpler processes are not inherently necessary to the higher. Attention is, for example, placed among the higher intellectual processes, of which sensations are by definition the elements; yet attention suddenly appears full-blown as clearness of higher processes; it is characterised as the opposite of distraction, of wool-gathering, and as subjective (not the objective clearness or distinctness of objects); after this characterisation the literature of attention is discussed under the familiar rubrics; and that is the end of attention. Again, when we come to emotion, the higher feelings are differentiated from the lower by difference in temporal course, in ease of inhibition, in dependence upon mental conditions, etc.; but the question how the simple combine into the higher feelings is never even raised. This lack of interest in writing a psychology that is systematic in any sense other than that of mere classification along conventional lines occasionally leads our author to unfortunate results. Among these is a failure at times to understand clearly the systematic views of other writers. For example, in his discussion of the attributes of sensation he says: "Titchener replaces the attribute of extent (*Räumlichkeit*) by that of clearness, which is determined by the strength of attention" (I, 28). Another result is a confusion in classification. For instance, the section on thought (in which the Würzburg and the Cornell studies, Wundt's reaction against the thought-experiments, Koffka's book, etc., are reported) divides the chapter on perception into two parts; it occurs after the detailed accounts of the perceptions of space, time and movement, and before those of synaesthesia and the perception of form. The author gives no reason for this grouping; and whatever we may guess leads to the conclusion that the problem of perception has never been fully realized. What, then, it may be asked, is the author's positive contribution to our science?

The book is a collection of facts on and about psychology, drawn from a large number of different sources; the results of experiments conceived from various points of view, selected according to the compiler's judgment, classified under the usual chapter-headings, and moulded into readable form. The attempt is unique in psychology; the nearest approach, Myers' *Text-book of Experimental Psychology,* is still very different. The positive value of the book depends, therefore, upon the relative value of the facts selected, the completeness with which the ground is covered, the accuracy with which the facts are reported, the skill with which they are presented, and the aids supplied for the reader's convenience. From these points of view let us examine the book once more

The section on sensation is, aside from that on association of ideas, the most complete of the entire work. The facts of visual sensation are taken principally from Hering and his pupils, v. Kries, Titchener, Ebbinghaus, and G. E. Müller. Helmholtz and König are, however, not ignored. The chief innovation is the extended incorporation of Müller's views, drawn not only from the well-known *Zeitschrift* articles but also from Fröbes' manuscript notes of Müller's lectures taken in 1903. The chapter on auditory sensation is based upon Stumpf, Helmholtz, and Schäfer's article in Nagel's *Handbuch.* Two pages are given to Révész' *Qualität*; and in the paragraph on *Klangfarbe* there is a report of the vowel-controversy, which includes the work of Köhler and of Jaensch; the solution of both these problems is, however, left to the future. The principal omission is that of tonal volume, which is mentioned only as a 'quantitative element' of *Klangfarbe.* On the side of theory we have only a brief account of Helmholtz', and a still shorter reference to that of Ewald; for others the reader is referred to Schäfer. Taste and smell, cutaneous, kinaesthetic and static, and organic sensations are likewise reported from the standard sources, and on the whole are faithfuly reported and clearly presented. Apparently, neither Henning's work on taste and smell nor Boring's on sensations of the alimentary canal was available; but, at any rate, are missing. The account of simple feeling is mainly a summary report first of the controversies about feeling (which suffers for want of insight into the reasons for the differences of opinion), and secondly of the results of experiments by the method of expression, particularly those of Lehmann and the Wundtians. All of the experimental work based upon the method of impression is ignored. The experimental work on 'simple ideas' is also insufficiently reported. In the long section on perception, the principal facts are taken from the important secondary sources like Hermann's and Nagel's *Handbücher,* the treatises of Wundt, Ebbinghaus, Stumpf, Witasek, etc. There are also many reports of original experiments and occasional abstracts of long books. One wonders at times at the relative amount of space assigned to various topics;

why, for instance, six pages should be given to a digest of Katz' *Erscheinungsweisen der Farben,* significant as that book is, and only about the same amount to the geometrical-optical illusions. Or again, one may miss a fact like Wertheimer's Phi-phenomenon which seems important enough for mention. On the whole, however, the section is well done. The point of view of the *Psychophysik* is that of Müller; and the treatment of methods, and discussions of the significance of the limen, of Weber's law, etc., are drawn principally from Fechner, Müller, Titchener, and Lehmann. The article is, however, brought up to date; the views of Wirth and Urban are presented; and the section closes with a long chapter on correlation. The best parts of the final section of the first volume, on association of ideas, are, as one might expect from a pupil of Müller's, those which deal with the methods and results of the association-experiments. The worst part is the chapter on ideational types, which is inadequate as regards the available facts. The association-reaction, including Jung's *Tatbestandsdiagnostik,* is described in this section.

This brief analysis of the first volume must suffice for the present review, although the second volume is a different book, and might well have had a different title; it is a 'general' psychology. And because under many chapter-headings there is no large body of accepted fact upon which to draw, there is greater opportunity for disagreement as regards the choice of the facts which go into these chapters. The author has, however, in most cases chosen his authorities wisely: Meumann and Müller for memory and learning, Wundt for language, Ach for will, Fechner, Groos, Witasek and Külpe for aesthetics, Stern for differential psychology, Preyer and Stern for child psychology, Kraepelin, Störring and Janet for abnormal psychology. If no one of the chapters based on such authorities would fully satisfy the expert, they will at least give the reader an orientation, not in the points of view of the various psychologists, but in the principal facts. And this, we suppose, is the author's aim. It must, however, be added that arrangement and emphasis often leave much to be desired: let the reader try, for instance, to determine from Fröbes' references the various usages and the preferred signification of the important term 'empathy'!

Taking the book as a whole, and overlooking minor errors and omissions, we must judge it as a well-selected compendium of the facts of psychology. So far as our reading has gone, we have found the reports of the principal facts reliable; the author is not so trustworthy in reciting controversies or in expounding systematic views because he has not taken the trouble to go beneath the controversies or sympathetically to understand systematic differences. He has, as he planned, written a book of the order of Tigerstedt's *Physiologie,* but he has not approached (as he also desired) Ebbinghaus' *Psychologie.* For Ebbinghaus had a point of view which he tried to drive through the *Grundzüge;* and however inadequate he may at times have been, he always had an insight which penetrated to the psychological significance of his facts, and he was at all times critical, keenly discriminative, and constructive. Fröbes, on the other hand, is a reporter who has read widely and painstakingly, but from the outside; if he attempts criticism, he is likely to be superficial; if he is discriminative, it is seen only in his selection of facts for treatment and not in his treatment of the facts; and, lastly, he is never constructive; he throws no new light upon old facts, opens no novel perspective, makes no new generalization.

H. P. W.

Vorlesungen über die Menschen- und Tierseele. By W. WUNDT. Sechste neubearbeitete Auflage mit 53 Figuren im Text. Leipzig, L. Voss, 1919. pp. xvi, 579. Price (at time of publication) Mk. 26.

This book has had an amazing career. It began in 1863 as a two-volume work whose programme was nothing less than the rounding-off of all departments of psychology, experimental, animal, social, into a systematic whole. Since in those early days Wundt had not learned to distinguish between psychology and applied logic,—he soon came to the discovery, and he never tired of proclaiming it,—there is no reason to wonder that his attempt was a failure. Critics declared, and the Wundt of 1906 is not unwilling to agree, that the book so far from inaugurating a reformation in psychology, was merely an immature and misdirected essay.

In course of time, nevertheless, the edition was exhausted; and in the late eighties the original publisher came to Wundt with the suggestion of a new issue. Wundt hesitated for some time, but finally acceded to the request. There was, he thought, a certain amount of the old matter that might still stand, and there was a vigor and freshness of style that he might not easily recover. The book was, also, something of an incubus; the printed word, however emphatically he should elsewhere renounce his change of view, bound him to statements that he would gladly revoke; and a new edition would formally supersede the earlier work. So a single volume appeared, as a second edition, in 1892. The logical framework was discarded; the lectures on social psychology were excluded; the whole text was rearranged and recast. This is the edition that Creighton and I translated in 1894—the first of Wundt's books to be published in English.

A third German edition was called for in 1897, and a fourth in 1906. The fourth edition is remarkable on the ground that Wundt here tried, for the first time, to do rough justice to the modern developments of animal psychology. The incident is eminently characteristic: Wundt, who knew nothing at first hand of the lower forms of life save what he had learned—how many years before!—in the zootomic laboratory of Johannes Müller, and who knew nothing at first hand of the higher forms save what he had learned by intercourse with the domestic cat and dog, undertakes at the age of seventy-four to review the 'literature' of animal psychology, to pass critical judgment upon it in the light of human psychology, to bring out the fundamental questions of principle, to show connections, to mark lacunae, all for the sake of three or four chapters of a popular book. It is needless to say that these chapters, and the preface that goes with them, inadequate as they must appear to the professional student of animal behavior, are nevertheless of high value to psychologists at large; they give us animal psychology in the Wundtian setting; they show us the relation in which Wundt placed animal psychology to individual human psychology no less clearly than the encyclopaedic *Völkerpsychologie* shows the relation of social psychology to the same central discipline.

The new chapters were popular; a fifth edition came out in 1911; and a sixth and final edition—let us hope that no editor rises up to lay hands upon it—in 1919. There is little that is new in this last revision: a note that brings the physical theory of relativity into connection with the psychological is, perhaps, the most important addition; all changes are minor. The book remains what it was in 1892, "a pretty mixed medley of new and old," but a medley so cunningly mixed that the layman will never suspect the diversity of origin, and a medley so various that the psychologist may find in it an epitome of the history of modern psychology. It is a long stretch from 1858 to 1919, a long stretch especially in the history of a young and rapidly growing science. Wundt has been able, as no one else can ever be, to compress a vast deal of this history, for the benefit of those who read with understanding, into the limits of a single volume.

E. B. T.

Fugitive Essays. By J. Royce. With an Introduction by J. Loewenberg. Cambridge, Harvard University Press, 1920. 429 pp.

Here are printed fifteen of Royce's essays. All of those previously published were virtually inaccessible; all but three belong to the period 1878-1882; and four appear in print for the first time—"The Practical Significance of Pessimism" (1879), "Tests of Right and Wrong" (1880), "On Purpose in Thought" (1880), and "Natural Rights and Spinoza's Essay on Liberty" (1880). The editor contributes an illuminating introduction (37 pp.), in which he emphasises the continuity of Royce's earlier and later work. The volume contains a good portrait of Royce; it should have had an index.

Collected Essays and Reviews. By W. James. New York, Longmans, Green & Co., 1920. pp. x (Preface by R. B. Perry), 516.

Annotated Bibliography of the Writings of William James. By R. B. Perry. New York, Longmans, Green & Co., 1920. pp. ii, 69.

All psychologists will be grateful to Professor Perry for the labor of love that he has spent on these two books. The first contains thirty-nine scattered articles and reviews written by the late Professor James between the years 1869 and 1910. They include the "Remarks on Spencer's Definition of Mind as Correspondence" (1878), "The Sentiment of Rationality" (1879), "What is an Emotion?" (1884), "The Original Datum of Space-Consciousness" (1893), and many another classical paper. The Bibliography, based upon James' and Holt's List of 1911, comprises (if a hurried counting may be trusted) 311 titles, arranged chronologically from 1867 to 1920. Most of the titles receive brief comment, and an alphabetical index is appended.

Social Scandinavia in the Viking Age. By Mary W. Williams. New York, The Macmillan Co., 1920. pp. xiv, 451, with 50 illustrations and map. Price $6.00.

This is an interesting and scholarly book—undoubtedly the best introduction to its subject that we have in English. It meets fairly the charge of our childhood's histories that the Northman, admirable in courage, was at the same time drunken and licentious, an inveterate gambler, a violator of his oath. After an introductory sketch of the land and the people, the chapters take up in order the ties of kinship and nationality; the classes of society; infancy, childhood and youth, dress, ornament, personal refinement; marriage and divorce; the position of women; homesteads and houses; house-furnishings and food; agriculture and the routine of farm life; hunting, fowling, fishing; internal travel. ships and nautical science; trade and commerce; markets and towns; the career of the Viking; his weapons and warfare; government; system of justice; social gatherings, recreations, amusements; language and literature, the runes; learning in general; scientific knowledge, art; objects of worship; places and methods of worship; superstition; death and burial. The illustrations are well chosen, and the book ends with a useful bibliography and a full index.

The Philosophy of Don Hasdai Crescas. By M. Waxman. New York, Columbia University Press, 1920. pp. xii, 162. Price $1.75 net.

Hasdai Crescas, the last of the Jewish mediaeval thinkers, was born of a noble and wealthy Catalonian family in 1340, lived the greater part of his life at Barcelona, and died at Saragossa in 1410. He wrote a work entitled "The Light of God," in which among other things he criticised Maimonides' proofs of the existence of God, and his views of the divine attributes and of the relation of God to the world, and set forth his own positive convictions. In the present study Dr. Waxman expounds the system of Crescas, whom he exhibits as an intellectual rebel against Aristotle, a probable influence upon Spinoza, an analyst surpassing Maimonides himself. The Introduction deals with Jewish and Arabic philosophy at large, with the treatment of the problem of the existence of God in pre-Maimonidean Jewish philosophy, and with the theology of Aristotle. Part I then discusses the problem of God, his existence, essence, unity, attributes; and Part II the relation of God to man, as exemplified in the questions of free-will and determinism, providence, immortality. The study concludes with bibliography and index.

The following books have been received:

L. Klages. *Vom Wesen des Bewusstseins, aus einer lebenswissenschaftlichen Vorlesung.* Leipzig, J. A. Barth, 1921. Pp. vi, 94. Price Mk. 12.

G Heymans. *Einführung in die Metaphysik auf Grundlage der Erfahrung.* Dritte durchgesehene und vermehrte Auflage. Leipzig, J. A. Barth, 1921. Pp. vi, 364. Price Mk. 56.

F. Weltsch. *Gnade und Freiheit.* München, Kurt Wolff Verlag, 1920. 157 pp.

J. B. Pratt. *The Religious Consciousness, a Psychological Study.* New York, The Macmillan Co., 1920. Pp. x, 486.

E. Galli. *Alle Radici della Morale.* Roma, "Unitas," 1919. 415 pp.

E. Galli. *Nel Domino dell' "Io."* Roma, "Unitas," 1919. 205 pp.

NOTES

Wilhelm Wundt died at Leipzig on the afternoon of Tuesday, August 31, 1920, a fortnight after the celebration of his eighty-eighth birthday. Psychologists of all interests and of all shades of opinion unite to do homage to the memory of the foremost representative of their science. We hope in a later number to print a sketch of Wundt's life and psychological work.

Théodore Flournoy, who held the chair of psychology and history and philosophy of science at the University of Geneva, died on Nov. 5, 1920, at the age of 66. Flournoy published in 1893 a volume on *Des phénomènes de synopsie,* and became widely known by his *Des Indes à la planète Mars: étude sur un cas de somnambulisme avec glossolalie,* 1900 (translated into English in the same year). He published also: *Le génie religieux,* 1904; *La philosophie de William James,* 1911 (translated 1917); *Esprits et mediums,* 1911 (English abridgment, *Spiritism and Psychology*); *Une mystique moderne,* in Arch. de psychol., xv, 1915; *Mètaphysique et psychologie,* 1919 (reprint, with preface by H. Höffding, of the edition of 1890); and many other studies in the *Archives* and elsewhere. He was co-editor with Professor Claparède of the *Archives de psychologie* since the foundation of that journal in 1902.

Alexius von Meinong, professor of philosophy in the University of Graz, died on Nov. 27, 1920, at the age of 67. Meinong is best known by his contributions to the border-discipline which he named *Gegenstandstheorie*; his *Gesammelte Abhandlungen* were published in three volumes in 1913-14. His doctrine of supposals (*Ueber Annahmen,* 1902, 1910) attracted widespread attention. Meinong also wrote on topics of direct interest to experimental psychology (Weber's law, the color-pyramid, etc.), and in 1894 founded at Graz the first Austrian laboratory, from which has come a long and valuable series of experimental studies. He was strongly influenced by Franz Brentano, but with his friend Alois Höfler arrived at conclusions which Brentano vigorously combatted; indeed, we owe to Meinong much of the new matter in Brentano's *Von der Klassifikation der psychischen Phänomene* (1911).

Elmer Ernest Southard, professor of neuropathology in the Harvard Medical School, from 1912 to 1919 director of the psychopathic department of the Boston State Hospital, and in 1919 appointed director of the Massachusetts State Psychiatric Institute, died Feb. 8, 1920, at the age of 44. The Bulletin of the Massachusetts Department of Mental Diseases, iv, no. 1, Feb., 1920, which is issued as a Southard Memorial number, contains a characteristic portrait, an appreciation by Dr. R. C. Cabot, and a bibliography.

SYSTEMATIC PSYCHOLOGY

Professor Stumpf has recently published in the Memoirs of the Prussian Academy of Sciences, and also as separate works, two important monographs in systematic psychology.[1] (1) The first (1917) deals with the attributes of visual sensations. Aside from spatial and temporal characters, which he does not consider, Stumpf recognises three attributes: quality, brightness, and intensity. The change which differentiates the progression of blue-red-yellow from the progression black-grey-white is a change of quality; the change which is common to the two progressions is a change of brightness. The achromatic series thus shows, like the color series, changes both of quality and of brightness. Intensity, the outstanding difficulty of the psychology of visual sensation, receives thorough discussion. Stumpf frankly accepts the attribute, on the analogy of the other senses, and seeks evidence for it. He finds in the grey (which, in popular speech, is a dark grey) of the resting eye—G. E. Müller's middle grey, Révész's critical grey—the weakest visual sensation, and in direct sunlight the strongest. Theoretically, all the visual qualities, including of course the blacks which are darker than the critical grey, lie in a single straight line of intensity between these terminal points. As a matter of fact, the qualities that we see in ordinary diffuse daylight are of approximately constant intensity. If, however, we observe strongly illuminated (especially yellow) surfaces on a dark background we may note definite intensive changes: the yellow, *e. g.*, remains the same yellow, but as the illumination increases becomes yellower, not in the sense of greater saturation, but in the intensive sense of more strongly yellow. These observations, Stumpf argues, are sufficient to establish the attribute of intensity: colateral evidence is found in the experiments upon color limens.—Saturation or chroma then necessarily disappears as a visual attribute. Stumpf declines to recognize insistence, and apparently declines to recognize clearness or vividness, though formally he leaves this issue open.

(2) In the second monograph (1918) Stumpf seeks to determine the difference between sensory and imaginal experience, between *Empfindung* and *Vorstellung*. He begins by discussing, with special reference to tones, the criteria currently applied: presence and absence of external causes, specific diversity of contents, specific diversity of acts, gradual difference of intensity. The evidence points decisively to a mere difference of degree. Stumpf accordingly adopts this position, and from it rebuts a number of objections: that, with a merely intensive difference between sensation and image, we could not talk of an imaginal fortissimo; that metric comparisons of the intensity of sensations and images ought to be possible; that images could not be present simultaneously with sensations of the same sense; and that the facts of the limen would be unintelligible. He then turns from hearing to sight, and considers the intensity of visual images, their spatial properties, the phenomena in the neighborhood of the limen, and the separation of colors and greys in ordinary ideas and in hallucinations.

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[1] "Die Attribute der Gesichtsempfindungen," *Abh. d. kgl. preuss. Akad. d. Wiss.*, Jahrgang 1917, phil.-hist. Klasse, no. 8. Einzelausgabe, Berlin, 1917; price (at the time of issue), Mk. 5.25. 88 pp.

"Empfindung und Vorstellung," *Abh.*, etc., Jahrgang 1918, phil.-hist. Klasse, no. 1. Einzelausgabe, Berlin, 1918; price (at time of issue), Mk. 6.75. 116 pp.

The upshot is that all *sinnlich-anschauliche Erscheinungen,* all the 'sensory' and 'imaginal' experiences, of a determinate kind form a single intensive series, ranging continuously from the weakest 'image' to the strongest 'sensation.' From the purely phenomenological point of view there is no reason for a distinction of the two classes. We gradually learn, however, that experiences of a certain range of intensity are usually due to outside causes, and we thus come in course of time to an immediate, unreflective differentiation of perception from idea. Scientifically, the line of division is drawn at the stimulus-limen. But once the dividing line has been drawn, once the scale of intensities has been calibrated, a conscious reference-to-object is unnecessary; henceforth intensities above the limen go by the very fact of their intensive rank to the class of sensations, intensities below the limen to the class of images.

Stumpf proceeds to the enumeration of secondary criteria. *Vorstellungen* are poorer than *Empfindungen* in immanent and concomitant characters; they are fleeting, or at any rate less sharply delimited in duration; they are largely modifiable at will. And, as a result, the affective influence of *Vorstellungen* is on the average less than that of *Empfindungen*; and "if the question of the real significance of the phenomena arises, it is in the case of *Vorstellungen* bound up with the consciousness that the belief in reality needs justification, while in face of *Empfindungen* this belief is, at least for the naive consciousness, immediate."

If all images lie below the intensity of the stimulus-limen, it follows that many experiences which we currently number among images must be renamed sensations; Stumpf devotes a section to subjective sensations and hallucinations. From these he passes to the collateral evidence for the qualitative likeness of sensation and image: associative memory, the fusion of reproduced and perceptive elements in the unitary empirical object, associative reproduction and the recognition of purely imaginal formations, likeness of sensory and motor effects. Finally, he distinguishes between images of memory and images of recollection, but hesitates to recognize a class of images of imagination.—

Whether or not we agree with Stumpf's conclusions, we may be heartily grateful to him for the patient thoroughness with which he has worked them out; and fortunately they may, in most cases, be put to the ultimate test of experiment. The brightness-attribute of visual sensations will, one is disposed to think, find ready acceptance; the intensive attribute will doubtless be disputed, but has at any rate achieved respectability. Saturation may be dispensed with the more easily if one accepts Dimmick's view of grey as not the specific mid-term of a single black-white series and the end-term of chromatic series, but the end-term of six specifically qualitative series. As regards sensation and image, I confess that Stumpf's exaltation of intensity does not so far convince me, and that his account of the observer's behavior in the liminal region does not square with my own experience. I incline rather to a differentiation by 'body,' by collocation of attributes. Here as elsewhere, however, experiment will decide.

E. B. T.

THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The twenty-ninth annual meeting of the American Psychological Association was held at the University of Chicago, December 28, 29, 30, 1920, with approximately one hundred fifty members present. The Sections of Psychology and Education of the American Association for the Advancement of Science met at the same time and in the same building and members of both organizations circulated freely between parallel programs. The fifty-seven papers presented before the American Psychological Association were about equally divided between experimental and general psychology on the one hand and applied psychology and mental tests on the other. After the annual banquet the retiring President, Dr. Franz, gave the presidential address on "Cerebral-Mental Relations," in which he brought together the recent evidence against fixed localization of function in the brain. Messrs. Cattell, Judd, Scott, and Pintner spoke informally in appreciation of Wilhelm Wundt, presenting personal reminiscences of their work with him.

The meetings were marked by considerable discussion of plans of organization in psychology and of the ways and means for meeting the demands which the recent extensions of psychology have placed upon scientific sources of fact and personnel. There was also evidence, as numerous papers attested, of a desire on the part of experimentalists to break down distinctions existing at present between schools of psychology and to emphasize psychological community of thought rather than differences of points of view.

The election of officers resulted as follows: President: Margaret Floy Washburn, Vassar College; Members of the Council, 1921-1923: George F. Arps, Ohio State University; Walter S. Hunter, University of Kansas; Nominees for appointment to the Division of Anthropology and Psychology of the National Research Council: Walter B. Pillsbury, University of Michigan; George M. Stratton, University of California.

E. G. B.

RECOGNITION OF FACES

The prompt and sure identification of persons by recognition of their faces is so obviously a matter of practical importance that it is amazing that it has not earlier become an object of critical and experimental study. Dr. Henning[1] has, however, laid an excellent foundation in analysing the problem and working out a method for a quantitative attack upon it, from which follow in natural sequence a method for the training of those whose business it is to recognise faces, and a new field for testing in vocational guidance. Incidentally he has also supplied psychology with a striking instance of the operation of the "apperceptive mass," and furnished ethnology with a new method of racial classification.

For the details the paper itself must be consulted; but it may be noted that unerring identification of faces is dependent upon the analysis of them, *i. e.*, upon attention to certain primary facial elements (eyes, eye-brows, nose, mouth, etc.) and their variations—

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[1] H. Henning, Das Wiedererkennen menschlicher Gesichter in kriminologischer Hinsicht. Mit 14 Abbildungen. *Arch. f. Kriminologie, 72,* August, 1920, pp. 235-254.

inexpert looking at faces is always satisfied with unanalysed total effects—and that the method consists in finding the minimal facial area, determined on a photograph or a real face by means of an Aubert diaphragm or something similar, which permits certain recognition.

The illustrations show the bilateral asymmetry characteristic of most faces and the difficulty of recognising even well-known faces when seen in part only. It is regrettable that the pictures illustrating bilateral asymmetry complicate the matter by introducing a gross and unnecessary difference in the arrangement of the hair.

E. C. S.

A FURTHER WORD ON SUPERSTITIOUSNESS

Since publishing my study of the superstitions of college students (this JOURNAL, 30, 1919, 83), I have discovered a somewhat similar study made over thirty years before and published under the rather misleading title of "First Report of the Committee on Experimental Psychology" (*Proc. of American Society for Psychical Research,* 1, No. 3, 1887). The report is signed by the committee's chairman, Professor C. S. Minot. A form of questionary was used, and the study was designed to "test the prevalence of a tendency to superstition in the community." The three questions in the list upon which the conclusions of the report were based sought the frequency of tendency to superstitiousness concerning the number thirteen, Friday, and seeing the new moon over the left shoulder. Why these three and only these were selected is not made clear. Five hundred returns were tabulated, but no definite statement appears in the report concerning the class of people from whom the returns came, except a vague statement in the concluding paragraph about "the educated portion of our community" and a reference to New England.

This report concludes that 10% of men and 20% of women have a tendency to superstition, and the committee expressed surprise at the large number. My own results indicate a much larger percentage of both men and women (men admitting present belief or practice 40%, and women 66%). Can it be that there has been such an astonishing increase in superstitiousness in thirty years? Probably not. The small figures which the committee obtained are more likely due to the curious limitation of their study to three superstitions. It is true that the committee's returns were from adults chiefly, while mine were entirely from adolescents; but an analysis of their tables indicates that the returns which they included from those of adolescent years present the same small percentages.

Isolation of the committee's returns from adolescents, for comparison with my own returns, indicates that the committee found about 11% of young men with a superstitious tendency and about 17% of young women. My results for the same age-group were 40% and 66%, approximately the same ratio. That two studies made thirty years apart, in localities three thousand miles apart, and by somewhat different methods, should indicate the same degree of difference between the sexes adds much to the reliability of the conclusion.

Neither Dresslar's work nor mine, however, supports the conclusion of the Minot committee that there is a greater tendency to be superstitious about the new moon than about either Friday or the number thirteen. As our combined work covers so many more cases, it must certainly be that the Minot committee were led astray by a chance result

University of Oregon. EDMUND S. CONKLIN.

COMBINATIONAL TONES REGISTERED BY THE TONOSCOPE .

In a recent number of this JOURNAL[1] the writer reports the effectiveness of the tonoscope for registering difference-tones of the first order. Later experiments justify further conclusions.

In this study difference-tones of higher orders were investigated. For this purpose the generators employed were the bugle and the voice. Two points were essential in selecting the range of the generating notes: that their difference-tones should lie within the limits of the tonoscope-readings, or at least not beyond a very small multiple of the readings, and that the various difference-tones should be far enough away from one another and from their generating notes to prevent overlapping and blurring of the dots on the tonoscope. When these precautions were observed, records of difference-tones of the second and third orders were as easily and clearly obtained as those of the first order had been in the earlier experiments.

BUGLE TONE		SINGING TONE		
Approximate note	Tonoscope Readings in vibs.	Approximate note	Tonoscope Readings in vibs.	Tonoscope Readings for Difference Tones in vibs.
$a\#^1$	120 × 4=480	$f\#^1$	123 × 3=369	D_1 111 × 1=111
	160 × 3=480		184 × 2=368	D_2 129 × 2=258
				D_3 147 × 1=147

Records for summation-tones obtained from these generators were then attempted without success.

Many theories have been advanced regarding the nature and origin of summation-tones. W. Preyer,[2] following a suggestion made by G. Appunn, explains them as differential tones derived from the action of partials rather than from the generating tones themselves. He declares that all cases of summational tones on record might conceivably fit into this theory. This view suggested a means of approach through the tonoscope. If, for example, the summation-tone of 500 vibs. arising from two notes with vibration-rates of 300 and 200 respectively may be regarded, not as the sum of the two generators, but as the difference between the first generator's second partial of 600 vibs. and the first difference-tone of 100 vibs., then generators possessing a strong second partial ought to register this 'summation' tone as easily as difference-tones, and generators lacking the second partial in any strength ought to fail to register this tone although difference-tones be clearly recorded.

To check this supposition trombones were selected because of their strong second partials, and clarinets because this partial is an almost negligible component of the tone.[3] Difference-tones of the first two orders were attempted with good results from both instruments.[4] The two trombones gave a clear reading of a summation-tone from the following generating notes: f, 176 vibs. and A, 117 vibs. The tonoscope recorded 147 × 2, or 294 vibs.[5] The clarinets were uniformly unsuccessful in producing readings for summation-tones, although conditions were well adapted for recording them if present.

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[1] E. Gough and G. Robison, The Tonoscope as a Means for Registering Combination Tones, *Amer. J. Psychol.*, 31, 1920, 91ff.

[2] H. Helmholtz, Sensations of Tone; Additions by the Translator, A. J. Ellis, 1895, 532.

More delicate and more extensive work needs to be done to investigate this theory further. These experiments tend to support the conception that summation-tones are to be regarded more accurately as difference-tones dependent on the presence of a strong second partial tone.

Smith College. EVELYN GOUGH.

APPOINTMENTS

Dr. Samuel W. Fernberger, recently Assistant Professor of Experimental Psychology at Clark University, has been appointed Assistant Professor of Psychology at the University of Pennsylvania. Mr. C. C. Pratt has been appointed instructor in experimental psychology at Clark University.

Dr. F. L. Wells has left the McLean Hospital, Waverley, Mass., to become head of the Psychological Department at the Psychopathic Hospital, Boston, Mass.

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[3] The writer was exceedingly indebted to the generosity and the patience of Northampton musicians in carrying on this section of the work. Mr. Carl Dodds and Dr. C. E. Perry played the trombones and Miss Myrna Wilderson and Mr. Carl Brand the clarinets.

[4] The clarinet gave good readings only for tones produced by closing most of the keys. Otherwise the air escaped through these openings instead of being directed into the tonoscope. The trombone proved an excellent instrument for this use.

[5] The second partial itself could never be read obviously from the tonoscope.

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WILHELM WUNDT

By E. B. Titchener

Wilhelm Max Wundt was born, the son of a Lutheran pastor, at Neckarau in Baden, on August 16th, 1832. We know nothing of the family life of his parents, and nothing of his schooldays, though we may guess that he attended the Gymnasium at the neighboring town of Mannheim, of which in later years (1907) he became an honorary citizen.[1] The biographies begin with the statement that he spent the years 1851 to 1856 at the universities of Tübingen (where his uncle was professor of anatomy), Heidelberg and Berlin. His interest, at any rate for the greater part of his studentship, lay not in physiology (though he worked for a while in Johannes Müller's Institute) but in the purely medical subject of pathological anatomy. In 1855-6 he was assistant in the Medical Clinic at Heidelberg, and his *Inaugural-Abhandlung* (Untersuchungen über das Verhalten der Nerven in entzündeten und degenerirten Zuständen) is dedicated to C. E.

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[1] It is difficult to secure these details. I am not even sure of Wundt's middle name: the authorities here accessible give it as Max, but I seem to remember having seen it printed Maximilian. Nor do I know if Wundt had brothers and sisters.

Neckarau is a small place lying close to Mannheim, with which I believe it is now incorporated. Mannheim and Heidelberg are themselves only half-an-hour apart by rail.

Hasse, its director.[2] The biographies inform us, next, that he became Privatdozent at Heidelberg in 1857. His titular subject, whether as a matter of choice or of academic accident, was physiology.[3] He remained in this position, working for some years as assistant to Helmholtz, who came to Heidelberg in 1858,[4] until 1864, when he was appointed *extraordinarius.* Again there was a wait; Wundt's apprenticeship to the academic career was longer even than Kant's. In 1874, however, he received a call to Zurich, to the chair of inductive philosophy founded by F. A. Lange; and in the following year he was made a professor of philosophy at Leipzig.[5] Here he lived and worked for forty-five years,—rector of the university in 1889 (only fourteen years after he had joined the philosophical faculty), honorary citizen of the town in 1902, orator of the university at its five-hundred-year jubilee in 1909,[6] professor until 1917; and near by, at Grossbothen, he died, on August 31st, 1920, a fortnight after his eighty-eighth birthday.[7]

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[2] Wundt held the doctorates of medicine, philosophy and law. The doctorate of law was conferred upon him, *honoris causa,* by the University of Göttingen, in 1905. This year was the fiftieth anniversary of his doctorate of medicine, which was accordingly taken in 1855. The Heidelberg thesis bears the date 1856. (My copy has no *Vita;* but as the plate at the end is duplicated the *Vita*-leaf may have been omitted.) Did it serve both for *Dissertation* and for *Habilitationsschrift?*—I do not know where or when the doctorate in philosophy was taken; I have been told that it was an honorary degree.

[3] See title-page of *Die Lehre von der Muskelbewegung,* 1858 (Preface, 1857) : a book dedicated to E. du Bois-Reymond, from whom and from whose pupils Wundt was presently to suffer sadly.

[4] The relations of Wundt and Helmholtz have not, to my knowledge, been thoroughly worked out. Personally, tradition says, the two men were uncongenial; and that would not be surprising, since their training was similar and their gifts and temperaments most dissimilar. But they speak of each other with mutual respect in the *Physiologische Optik* (1856, 1860, 1866) and the *Theorie der Sinneswahrnehmung* (1858-1862). When Helmholtz went to Berlin in 1871 his chair fell not to Wundt but to W. Kühne.

[5] His chief opponent was A. Horwicz. G. S. Hall tells us (*Founders of Modern Psychology,* MCMXII., 311) that the scale was turned in Wundt's favor by the local Herbartians. It must, surely, have been for them a choice of evils! For, if they had every reason to dislike Horwicz, they could still hardly have been much impressed by the preface to the *Physiologische Psychologie.*

[6] I have no list of Wundt's public honors. In 1911 he received the order *Pour le mérite,* one of the most highly prized of European distinctions (30 German and 30 foreign members) ; and he was knight of various, I suppose Saxon, orders. He also became a *wirklicher Geheimrat* of Saxony, and was addressed as *Exzellenz.*—It may be mentioned in passing that Wundt once attempted politics. In 1866 he was chosen representative of Heidelberg in the Baden second chamber. He very soon resigned.

Outwardly, then, Wundt's life was as uneventful as could well be: seventeen years at Heidelberg, and forty-five at Leipzig, with the Swiss interlude of a single year between. We have now to see what he made of this scholar's life; and we turn, naturally, to his books.

The book of primary importance for our purpose is the *Beiträge zur Theorie der Sinneswahrnehmung* of 1862. We do not know what led Wundt to the problem of perception: perhaps the Kantian atmosphere that he had breathed in Müller's laboratory,[8] perhaps the cases of anaesthesia that he met with in the Medical Clinic at Heidelberg, perhaps the discovery of a kindred spirit in E. H. Weber. At all events he writes a full-blown theory of perception, tactual and visual, four years before Helmholtz issues the third part of the *Optik*. We need not, however, concern ourselves with the psychology either of this book or of the more comprehensive *Vorlesungen über die Menschen- und Thierseele* which appeared in the year following. It was the psychology of the student of clinical medicine, of the biological technologist, made up out of general knowledge and common sense and medical case-histories as occasion required; we have plenty of it with us today, without needing to explore the work of half-a-century ago. What is of solid and enduring interest is the thirty-page introduction, *Ueber die Methoden in der Psychologie,* in which Wundt sets forth three ideas of first-rate importance: the idea of an experimental psychology, the idea of a social psychology, and the idea of a scientific metaphysics.

(1) If psychology is to advance, Wundt says, it must follow the inductive path. Two inductive methods are available: the method of statistics and the method of experiment. The former is an indirect method, since it bears primarily upon practical and not upon theoretical psychology. It brings with it, nevertheless, an extension of psychological observation; it furnishes psychology with new facts, guaranteed by the law

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[7] Wundt's war-utterances we can only try to forget. We may be glad that he suffered no personal loss and (as it appears) no considerable personal discomfort during the troubled years; that he was able to work steadily to the completion of the *Völkerpsychologie;* and that he saw his son established as the successor of R. Eucken in Jena (W. Wirth, *Arch. f. d. ges. Psych.,* XL., 1920, xvi.).

[8] Kant and Herbart were the influences against which Wundt had to fight most continuously. They were accordingly the influences which most strongly affected him.

of large numbers; and in so far it is related to the direct method of experiment. This second method, Wundt declares, is in principle applicable over the whole range of general psychology. There is no hint of the restriction with which we later become familiar.[9] But neither is there, so far as I can see, any hint that the use of experiment is to safeguard the procedure and assure the results of that *Selbstbeobachtung* with which all psychology begins. Observation seems to remain pretty much what it had always been; only, by varying the conditions of observation, Wundt hopes to vary the mind's response to external stimuli and thus presently to arrive at laws of the mental life as such.[10] Not, I think, until 1881 did he express the modern view that "die exacte Beschreibung der Thatsachen des Bewusstseins . . . das einzige Ziel der experimentellen Psychologie [ist]."[11]

Whence, now, did Wundt derive his idea of an experimental psychology? I have no wish to belittle his originality; if I had, the attempt to do so would be futile. Ideas of this sort, however, do not spring readymade from the thought of an individual. And I believe that the proximate source of Wundt's idea is patent. No one can read the introduction to the *Beiträge* without being reminded of the sixth book of John Mill's *Logic;* and no one, I think, who after such reminder compares the two compositions can doubt that Mill, for whom psychology is explicitly a science of observation and experiment,[12] gave the cue both for Wundt's emphasis on improvement in method and for the concrete means to improvement, statistics and experiment, which Wundt propounds. There is marked difference, over and above the cardinal difference that Mill talked about experiments and Wundt carried them out; but I have no doubt of Wundt's indebtedness to Mill.[13]

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[9] "Man ist häufig der Ansicht gewesen, gerade im Gebiet der Empfindung und Wahrnehmung [Ebbinghaus was nearly a quarter of a century in the future!] sei die Anwendung der experimentellen Methode noch möglich, . . . dagegen sei es ein vergeblicher Versuch, auch in das Bereich der höheren Seelenthätigkeiten auf experimentellem Wege vordringen zu wollen. Sicherlich ist dies ein Vorurtheil" (*Beiträge,* xxvii.).

[10] *Ibid.*, xxix.

[11] Ueber psychologische Methoden, *Philos. Stud.,* i., (1881) 1883, 3. The statement is sharpened in 1888. "Selbstbeobachtung [in the technical sense of *Beobachtung*] ist ausführbar, sie ist es aber nur unter der Bedingung der experimentellen Beobachtung" (Selbstbeobachtung und innere Wahrnehmung, *Philos. Stud.,* iv., 301). Cf. the appraisal of reaction-experiments in 1894: "Der Hauptwerth . . . dieser Versuche besteht . . . darin, dass sie die psychischen Vorgänge exact geregelten Bedingungen unterwerfen und auf solche Weise eine genaue Analyse der in der Selbstbeobachtung gegebenen Erscheinungen möglich machen" (Zur Beurtheilung der zusammengesetzten Reactionen, *Philos. Stud.,* x., 498).

[12] J. S. Mill, *A System of Logic,* etc., bk. vi., ch. iv., § 2; ch. v., § 5 (ii., 1856, 426, 447).

(2) The idea of a social psychology was in the German air at the time of Wundt's writing. In 1859-60 M. Lazarus and H. Steinthal had published the first volume of their *Zeitschrift für Völkerpsychologie und Sprachwissenschaft,* and their elaborate programme gave Wundt something positive to react against. We must follow the course of this reaction through several books.

In the *Beiträge* social psychology appears as an auxiliary science. General psychology must not only be improved methodically from within but must also be supported from without; and the supporting disciplines are two: first, developmental psychology, the psychology of the child; and secondly comparative psychology, the psychology of the lower animals and of human societies.[14] The preface to the first volume of the *Vorlesungen* of 1863 contains the sentence: "Wo das absichtliche Experiment aufhört, da hat die Geschichte für den Psychologen experimentirt."[15] This seems to look more directly toward the future: only, when we read the preface to the second volume, we find that the chapters in which experiment cannot be applied are those concerned with feeling, desire and action! Ethnological enquiry replaces experiment for the construction of a general theory of feeling; anthropology and the natural history of the lower animals give us an insight into instinctive actions, which leads on to a theory of the will; and the development of language serves to confirm psychological conclusions regarding the development both of feeling and of cognition.[16] We are wholly within the confines of general or individual psychology. Indeed, in a later note Wundt declares expressly that he has not, in the *Vorlesungen,* entered on the field of *Völkerpsychologie* as understood by Herbart, by Lazarus and Steinthal, and by Waitz.[17] In this book, then, we find—what is not infrequent in Wundt's work—a positive statement side by side with a flatly negative reaction to its immediate excitant.

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[13] Mill's *Logic* appeared in 1843, and the first German translation in 1849. See, e. g., *Beiträge,* 441. In the Psychologismus und Logizismus of 1910 (*Kleine Schriften,* i., 523) Wundt dates the first German edition 1862. Is this an evidential *lapsus,*—or did Wundt simply take the date from the second edition that he had used (1881) for his article on Mathematical Induction?

[14] *Beiträge,* xiv. f. The three departments of *Völkerpsychologie* are Sprachkunde, Culturgeschichte and Sittengeschichte.

[15] *Vorlesungen,* i., 1863, ix.

[16] *Ibid.,* ii., 1863, iii. f.

[17] *Ibid.,* 452.

The first three editions of the *Physiologische Psychologie* do not take us much further. In 1874 social psychology is essentially a descriptive, as opposed to an explanatory science. It has to do with complex phenomena, which must be illuminated by the laws of the individual consciousness; its task is largely classificatory.[18] In 1887 psychology is divided into (1) subjective psychology, which relies wholly on inner perception, and (2) objective psychology, which attempts to perfect and to supplement inner perception by objective means. Objective psychology, again, divides into (*a*) experimental or physiological psychology, which brings inner perception under the control of experimental appliances, and (*b*) social psychology, which seeks to derive general laws of psychological development from the objective products of the collective mind, from language, myth and custom. Formally, therefore, experimental and social psychology are co-ordinate and complementary. Materially, they are also mutually dependent; for the collective mental life everywhere points back to the mental capacities of the individuals that make up the society, and the individual consciousness, especially in its more highly developed modes, is supported (*getragen*) by the mental life of the community.[19]

In all this there is nothing distinctively Wundtian. And even the essay of 1888 confines itself to a justification of the choice of language, myth and custom as the subject-matter of social psychology, and to the drawing of a cautiously qualified parallel between these three topics and the idea, feeling and will of the individual consciousness.[20] Not until 1893 are experimental psychology and social psychology "the two main branches of scientific psychology." Now, at last, we reach the peculiarly Wundtian position that experiment breaks down on the far side of perception and memory, and that thenceforth the psychological system must be built up by way of *Völkerpsychologie*.[21] It is clear that, in the matter of experimental psychology, Wundt knew from the first what he was about, and modified his attitude only as his own psychological growth proceeded; whereas, in the matter of social psychology, he swung between different opinions, and reached his final standpoint only after a long course of trial and error. The difficulties in the one instance were mainly external; in the other, internal. Realising this, we shall give him all the more credit for keeping the troublesome subject of social psychology continually in mind.

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[18] *PP*, 1874, 4 f. So i., 1880, 4, except that the determination of the task is omitted.

[19] *PP*, i., 1887, 5 f.

[20] Ueber Ziele und Wege der Völkerpsychologie, *Philos. Stud.*, iv., 1888, 20, 25 f.

[21] "Glücklicherweise fügt es sich jedoch, dass gerade da, wo die experimentelle Methode versagt, andere Hülfsmittel von objectivem Werthe der Psychologie ihre Dienste zur Verfügung stellen:" *PP*, i., 1893, 5.

(3) The third idea of the introduction to the *Beiträge* is the idea of a scientific metaphysics, a philosophy which makes the results of all the other sciences the object of its own special investigations.[22] To prepare himself for constructive work in the light of this idea, Wundt wrote, after the *Psychologie* of 1874, his *Logik* (1880) and his *Ethik* (1886). The *Logik* falls into two parts: *Erkenntnislehre* and *Methodenlehre.* The former, strictly logical part is at any rate competently done; the book takes its place with the best logical treatises of its generation. Its value pales, however, before the lustre of the *Methodenlehre,* a work that is absolutely without peer. Wundt's occupation with physiology had brought him familiarity with mathematics and the procedures of the exact sciences; his study of psychology had made him equally familiar with the methods of the mental sciences.[23] The result of this " encyclopaedie and round of knowledge " is a book that would of itself alone set its author in the front rank of contemporary thinkers. The *Ethik* deals, in four parts, with the facts of the moral life, the development of theories of the universe, the principles of morality, and the departments of the moral life. The characteristic feature of the work is, again, its scientific tendency, its attempt to derive the principles of morality from an empirical survey of the facts of moral living.[24]

After this manifold preparation Wundt went about the writing of his *System der Philosophie* (1889). The question had been, of course, whether the thing could be done; whether a full compass of scientific knowledge had not ceased to be possible, if not with Aristotle, at any rate with da Vinci;

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[22] *Beiträge,* xiii.

[23] It has been said that the biological chapter falls below the standard of the others. I cannot agree. We have to remember the status of biology at the time when the chapter was written, and we have also (whether we like it or not) to presuppose Wundt's view of teleology.

[24] I confess that I have never felt at home with the Law of the Heterogony of Ends. It seems likely, if one pushes it far enough, to run sheerly counter to any ethical equivalent of the law of sufficient reason.

whether a modern, even if he had written a Physiology, a Medical Physics, a Psychology, a Logic and an Ethics, could rise on their basis to a genuine philosophy. Wundt replied by doing the thing in question. He draws up a complete programme of scientific philosophy, in every line of which he keeps his touch with science;[25] and he propounds a system in which no problem of that programme is shirked. We may accept or reject: Wundt has proved that this way of philosophising is still feasible.[26]

With the publication of the *System* it might well appear that Wundt had fulfilled his circle. He was fifty-seven years old; and he had enough to do, it would seem, in the revision of former texts (for all the larger books, the *Vorlesungen,* the *Physiologische Psychologie,* the *Logik,* the *Ethik,* the *System* itself, were going into new editions) and in the preparation, collection and revision of minor works (*Grundriss der Psychologie,* 1896; *Einleitung in die Philosophie,* 1901; *Essays,* [1885] 1906; *Kleine Schriften,* 1910-11; *Einführung in die Psychologie,* 1911).[27] As a matter of fact, he began forthwith to plan the largest of all his books, a book which causes us to retrace the path which we have too hastily been following: the ten-volume *Völkerpsychologie,* whose dates run from 1900 to 1920. The title-pages of the completed work still carry the familiar legend *Sprache, Mythus und Sitte;* but the plan grew with execution and revision,—Wundt's readers again demanded new editions; and the contents of the successive volumes are now distinguished as Language (2), Art, Myth and Religion (3), Society (2), Law, and Civilisation and History.

It is needless to lay stress on the intellectual vigor of a man who begins the publication of a work of this magnitude when he is sixty-eight, and continues its production over a period of twenty years. It is needless also to inform the JOURNAL'S readers that Wundt's reputation has not suffered, has rather

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[25] "Eintheilung der wissenschaftlichen Philosophie," *System,* 1889, 33 ff.

[26] The one large logical flaw of the *System* is the acceptance of the *Idee des letzten Weltgrundes.* Wundt honestly shows us his hand: "abweichend von allen anderen Vernunftideen ist dieselbe nämlich nicht durch einen directen Regressus von der Erfahrung aus erhalten worden, sondern nur infolge der allgemeinen Forderung, dass zu dem im Fortschrift der geistigen Entwicklungen sich vorbereitenden idealen Enderfolg ein dem letzteren vollständig adäquater Grund hinzugedacht werde" (439). He was himself subject to influences, historical and personal, which we who read him may not feel.

[27] A bibliography of Wundt's scientific writings will be found in this JOURNAL, vols. xix. (1908) ff.

(if possible) been enhanced, by his last achievement. I wish, however, to linger a little over the *Völkerpsychologie* in order to protest against a belief, current in recent years and in some measure encouraged by Wundt himself,[28] which I take to be grounded at best in a half-truth. A legend has grown up—I cannot call it anything else—to the effect that social psychology was Wundt's first and fondest love, and that all his life, up to about 1890, was spent in clearing intruders out of the way, that he might ultimately return to it. In part, the long stretch of years devoted to the *Völkerpsychologie* may be responsible; in part, as I have just said, certain statements of Wundt's own, made in what appears to be unnecessary self-defence;[29] in part, perhaps, a misunderstanding of the part played by social psychology in the early *Vorlesungen,* which are naturally more talked about than read. I should not accept this legend if it came with Wundt's own subscription; I should mistrust an old man's memory. I do not think that anyone can accept it who knows intimately the course of Wundt's development as his books portray it. At the beginning and for many years social psychology was rather for Wundt, as I called it above, a troublesome subject.

The kernel of truth in the legend is that Wundt was always attracted by troublesome subjects of a certain sort, subjects offering a certain type of data and inviting a certain kind of method. All of the major books bear a like stamp; they round up an incomplete and scattered subject-matter into tentative union and completeness; they are anticipations of system. They all, therefore, have about them a temporary and provisional air; they seem to promise new editions, to warn the reader that they will presently change. The preface to the first edition of the *Physiologische Psychologie* strikes the key-note: "die Orientirung über den Thatbestand einer . . . im Entstehen begriffenen Wissenschaft ist ja bekanntlich das beste Mittel, die noch vorhandenen Lücken zu entdecken." That note recurs, with such changes rung on it as the nature of the case demands, in every preface that Wundt wrote, from the *Vorlesungen* to the *Völkerpsychologie.* "Man kann möglicherweise zweifeln," he says of the *System,* "ob es angemessen sei, für eine derartige Untersuchung den alten Namen der Metaphysik zu wählen:" it is a new systematisation that he is attempting, the exposition of things from an unaccustomed point of view. Wundt is an essayist, only that his topics are not items but fields of knowledge.[30] It is small wonder, then, that—psychologist as he always was—he should be disquieted by the status and haunted by the problems of *Völkerpsychologie,* and should rejoice at last to bring psychological order into that chaos. But this is not to say what the legend says.

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[28] E. g., in the preface to *Die Sprache,* 1900.

[29] Wirth, if I understand him aright, thinks that Wundt found Fechner's controversial insistence troublesome, and was a little afraid of getting side-tracked by applied mathematics: *Arch. f. d. ges. Psych.,* xl., 1920, xii. ff. Wirth, I take it, is also on the defensive.

The twofold character of Wundt's work, as at once systematic and provisional, is a source both of strength and of weakness. It is obviously a good thing, if you are laying a case before the public, to think it steadily through, to view it in relations, to state it whole; so the argument becomes not only more impressive but also easier to grasp. It is a good thing, if you rely upon observations of fact, to sweep all your facts together, to organise them within a logical framework; so you become aware of support in unexpected quarters as well as of gaps that further work must fill. It is a necessary thing, if you are a man of science, to keep your ideas fluid, to let your theories sit lightly on you, to be open-minded toward new facts, to hold obstinately fast to nothing save the scientific point of view. But these good and necessary things imply a balance, and the balance of system and try-out, of system and first attempt, is not easy to maintain. Wundt was perpetually changing his evidence of observed fact and his minor perspectives; he expected to change them; the early data were but approximate and his first organisation of them must reflect their faults.[31] In so far he was plastic

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[30] This view of Wundt's work is substantially the same as that taken by E. Meumann in the appreciation written for Wundt's eightieth birthday (*Deutsche Rundschau,* clii., 1912, 217 f., 220 ff.). Meumann and I roomed together during my second year at Leipzig, and by dint of endless discussion and reference succeeded in pigeon-holing Wundt to our satisfaction.

[31] Critics have made Wundt's readiness to change a ground of complaint; he changed his views surreptitiously, they say, without warning the reader or giving due credit to the men who forced the change. In so far as this charge implies moral obliquity on Wundt's part, it is ridiculous; Wundt, as all who knew him will testify and as his whole public career shows, was as honest as the day. Where he found a positive reason for noting change, he could be meticulously definite: witness the second edition of the *Vorlesungen.* Usually he thought it enough to assure his readers that he had taken the task of revision seriously, that the new edition was an edition and not a reprint, and to give a bare indication of the chapters most affected.

There is, however, no smoke without fire; and the critics in question are, I think, in fact objecting to a temperamental trait of Wundt's, his natural mode of reaction to criticism and suggestion. Külpe, with whom I once talked this matter over, pointed out to me that Wundt's development was always a development from within; his immediate and receptive to an uncommon degree, and at an age when most men have settled down to fixed opinion. He did not either hesitate to throw overboard large theoretical constructions that his riper thinking disapproved; there is a great gulf between the *Beiträge* and the *Vorlesungen* on the one hand and the *Physiologische Psychologie* on the other. Yet he succumbed, without any doubt, to the temptations of the system. After 1874 (to take a rough dating) he showed little inclination to discard or revise his conceptual schemata; what had once been mere scaffolding thus tended to become an integral part of the actual building; or, to vary the figure again, Wundt poured the new wine of his later thought into the old bottles that he had more or less hurriedly assembled for his first successful vintage. I know, from many conversations, that he held his theories far more loosely than his readers ordinarily suppose, and that his greatest reverence was for fact. Yet it remains true that, when he had erected a theory, on however scant a basis of fact, he seemed as if in honor bound to defend it in his subsequent work. The theory might be changed contentwise out of all recognition; formally, nevertheless, it remained the original theory.

Had Wundt himself been aware that he was moving farther and farther away from his conceptual starting-points he would, with his indefatigable industry, have set about the task of revision. He was in fact aware, I imagine, rather of the continuity of his thinking; the later views seemed to him to be straightforward developments of the earlier, and therefore to be capable of expression in the same general terms. This sort of logical *Seelenblindheit* has had two regrettable consequences. The one is that Wundt was exposed to a hostile criticism which, as blind as he to the real issue, aimed only at the external and superficial, and which he accordingly and

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reaction to external suggestion was likely to be negative, but the new idea stayed with him, was incubated, and presently—perhaps long after—emerged with fresh coloring, in a novel context, variously modified, as a component of his own thinking. There were two consequences, which critics might very well find irritating. The first was that Wundt might read into early utterances of his own a pregnancy that they did not in truth possess; and the second (I have given an instance in the text) was that a positive statement might stand beside a negative criticism of the pre-Wundtian view which had, to all appearances, given occasion for the modified Wundtian formula. Other circumstances, social and professional, would possibly have made Wundt both more accessible and less sensitive to outside influence. But seventeen years of depression, followed by a rapid rise to a position that may almost be called pontifical, naturally served to harden his temperamental tendencies.

properly resented; the other is that students of Wundt must read his books in series, and can never hope to understand him fully from any single presentation of his thought.

We came to this discussion by way of the *Völkerpsychologie.* Retracing our steps still further, we arrive again at the first of the three ideas of the *Beiträge,* the idea of an experimental psychology. What Wundt made of this idea, so far as results go, all the world knows; what obstacles he had to overcome, and with what fortitude and persistence he overcame them, we shall probably never know.

In 1874 appeared the *Grundzüge der physiologischen Psychologie,* Wundt's most influential work. Beginning as a single-volume book, it grew to two volumes in the editions of 1880, 1887, 1893, and to three volumes in those of 1902-03 and 1908-11. In the first edition Wundt's psychology is in many ways crude; but it is nevertheless psychology, and not the applied logic of the *Beiträge* and the *Vorlesungen;* Wundt has struck his gait.[32] The controlling influences of his career were evidently operative between his thirty-first and forty-second years, though it is difficult to make out what they were. Perhaps the forthcoming autobiographical *Erlebtes und Erkanntes* will inform us.

Meanwhile we get no help from the list of publications. Wundt was busy, during the critical period, with his Physiology (1865, 1868, 1873); with *Die physikalischen Axiome* (1866); with the Medical Physics (1867); with the first part of his Mechanics of Nerve (1871). There is only a solitary article of 1867 entitled *Neue Leistungen auf dem Gebiete der physiologischen Psychologie.* And when he comes to write the *Physiologische Psychologie,* he relies for his physiological chapters, to be sure, on the work of these years of transition, but for his psychological data he goes back primarily to the *Beiträge* and secondarily to the *Vorlesungen.* No doubt he was maturing, fulfilling his normal inward growth. I think it a safe guess, however, that a strong negative influence emanated from Helmholtz, the final parts of whose *Optik* were issued in 1866. Even the outward form of the *Psychologie* seems to indicate a certain feeling of rivalry.

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[32] The *Jubiläums Katalog der Verlagsbuchhandlung Wilhelm Engelmann in Leipzig* (i., 1911, facing p. 90) contains a facsimile of the letter in which Wundt offered the manuscript of the *Physiologische Psychologie* to the firm for publication. The letter is dated Decr. 8, 1872, and suggests that printing may begin in Feb. of the following year. Wundt outlines the work in five parts: the physiological properties of the nervous system, the doctrine of sensation and idea, the doctrine of organic movements, criticism of psychological doctrines, and general theory of psychophysical occurrence.

Be that as it may, the *Physiologische Psychologie* was accomplished. Throughout the first four editions Wundt tried to keep it encyclopaedic, to make it a handbook of experimental psychology at large; the third edition is the best of these four. In the fifth and sixth editions he gave up that attempt, and frankly set forth his own psychological system. The change coincides with the ending of the *Philosophische Studien* and the founding of Meumann's *Archiv,* and its result is rather to maintain than to alter the status of the book. Wundt's laboratory had long been the heart and centre of psychological production; now the laboratories had multiplied.[33]

Here, then, is Wundt's first achievement in the domain of his experimental psychology. We can hardly overestimate it. As a work of reference the *Physiologische Psychologie* has been invaluable; its mere bulk and solidity have been an asset to a struggling science; the labor spent upon its revision has advantaged us all. But a greater achievement was to come.

In 1879—so runs the line in the biographies—Wundt founded the first psychological laboratory. We may let the bare line stand, if only it stand in lapidary letters. For that foundation was a world-event; it determined the very fabric and texture of modern psychology. Where John Mill theorised, Wundt performed; and the spirit of his performance has spread over the civilised world.[34]

Lastly, in 1881 Wundt began the publication of a magazine, *Philosophische Studien,*[35] the last two of whose twenty volumes (1902) constitute a *Festschrift* prepared by his former students for the occasion of his seventieth birthday. Unwearied as ever, he started a new series, *Psychologische Studien,* in 1905,[36] and carried it through ten volumes to his retirement from the Leipzig chair in 1917. These two *Studien*-sets have an individuality that will always mark them off from other psychological periodicals. In the earlier, we see experimental psychology in the making; problems at first are few, methods are imperfect, mode of presentation is uncertain, perspective is almost lacking. But there is a steady growth, extensive and intensive; a professional attitude forms; and when we reach the *Festschrift* we find topics from the whole range of psychology—physiological and philosophical, normal and abnormal, individual and social, current and historical—competently and fruitfully handled in the Wundtian way. The later volumes present a different picture. Here we see the specifically Leipzig problems attacked with the utmost refinements of Leipzig technique. The *Philosophische Studien* thus have the attraction of *eine im Entstehen begriffenen Wissenschaft;* the *Psychologische Studien* have the fascination of expert workmanship in a single style.

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[33] In 1902 the *Zeitschrift* had reached only its thirtieth volume, and the division of the two *Abteilungen* was still ten volumes away.

[34] I knew, in my Leipzig days, something in detail of the difficulties that Wundt had to encounter. I wish I could trust my memory to rehearse them. I recall that strenuous objection was made to the new laboratory on the ground that continued self-observation would drive young persons to insanity! Instead of that, the success of the novel enterprise moved older persons to imitation. Wundt was fortunate enough to gain, in 1891, a colleague like-minded with himself,—the historian K. Lamprecht; and Wundt and Lamprecht together are primarily responsible for the development at Leipzig of those *Forschungsinstitute* that are a legitimate source of pride to the university.

[35] Wundt has more than once rationalized the title of this publication. The author of *Ein Druckfehler bei Kant* might refer to the preface of the *Vorlesungen,* where Wundt remarks "dass *die philosophischen Studien* nur in den Erfahrungswissenschaften den Boden einer fruchtbringenden Entwicklung finden können."

It was, of course, a physical impossibility for Wundt, at his advanced age, personally to oversee the details of the experimental work carried out in his institute; W. Wirth was appointed co-director in 1908. But Wundt's editorship of the *Studien* was never perfunctory, and his interest in experimental psychology was always vigorous. In 1898 he was experimenting with the geometrical-optical illusions. In 1902-3 —*Die Sprache* appeared in 1900—he was, for the first time, overtly systematising his general or individual psychology. In 1906 he upheld the sensory character of black. In 1907 he launched his attack upon the methods of the Würzburg school: in the interest, truly, of his own social-psychological theory, but in the most intimate terms of laboratory experimentation. In 1908 he published the first volume of the new *Physiologische Psychologie,* whose ninth chapter bears witness to an extraordinary resurgence of interest in the fundamental problems of psychophysics. In 1909 he discussed the issue of pure and applied psychology. In 1911 he revised and republished the *Psychologie und Naturwissenschaft* of 1903. As late as 1914 he wrote about the illusions of reversible perspective. Surely, there is no gainsaying this evidence! The

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[36] Meumann's *Archiv* was first issued in 1903, overlapping the eighteenth volume of the *Studien.* Wundt was one of its cooperating editors, and agreed to publish in it the studies from the Leipzig laboratory. For the reason stated in his *Vorwort*—Wirth (*op. cit.,* ii) gives it as " die damalige glückliche Lage des deutschen Buchhandels "—Wundt preferred to recur to an organ of his own.

Völkerpsychologie, if I may repeat what has been said above, is a typically Wundtian book, an anticipatory system on the grand scale; it is the resolute outcome of a long period of perplexity; it furnished a grateful occupation for his old age; it is a work of exceeding value. But the dominant idea of Wundt's life, the idea upon which his reputation is most solidly based, the idea that persisted with him up to the very end of his university activity, is the idea of an experimental psychology.

And in a footnote to this list of his services in behalf of the idea, let us remember that Wundt was the first psychologist to bring demonstrational apparatus into the lecture-room. In his earliest Leipzig lectures he exhibited instruments and went through the motions of experiment. Very soon, however, he came to see the real purpose of a demonstration: the provision, namely, of conditions under which the audience may observe for themselves the fundamental phenomena of the subject-matter of discussion. His use of the lantern with illusions of reversible perspective, a brief account of which he published in 1907, is a very model of demonstrational procedure.[37]

It is plain that Wundt, whatever his intellectual gifts, could not have compassed this bulk of scientific work had he not been dowered with a good physical constitution and had he not lived a strictly regulated life. His days passed, in fact, with the regularity of clockwork. The morning was spent upon the current book or article; then came the *Sprechstunde.* The afternoon was taken up with the formal visit to the laboratory, a walk, the lecture, and a second, informal return to the laboratory. The evening was variously employed; Wundt might listen to a reader, or attend a concert or opera, or receive a group of his colleagues. For all his immersion in science, he managed remarkably to 'keep up' with current movements of literature and art. In personal intercourse he was unassuming, cordial, tolerant; by no means given to monologue; showing frequent flashes of a pleasant, wholly academic humor. There was no trace, as one sat with him in his own study, of the roaring lion of controversy or the somewhat Olympian arbiter of science and philosophy. He disliked public ceremonies, and could not be persuaded even to attend a psychological congress, though when occasion demanded his public appearance he played his part with dignity and success. He also disliked travelling, and his holiday excursions never took him far afield. These reluctances undoubtedly narrowed the sphere of his acquaintance, and so perhaps of his personal influence; but when the influence was already worldwide, when everybody who was interested in the things of mind came sooner or later to Leipzig, and when a greater *Geselligkeit* would have meant loss of productive time, they did not after all much matter. Wundt lived the simple family life of the old south-west German tradition, a retiring, sheltered life, which was probably the one condition under which his tremendous self-appointed task could have been accomplished.

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[37] Wundt exerted a great and ever increasing influence as a lecturer. His habit was to throw his ideas into shape during his afternoon constitutional, and to speak without notes, though he always had a rough scheme of the topic in his pocket. I remember an occasion when his memory played him false in the matter of the name of a minor Greek philosopher; he extracted the paper of notes, and scanned it while still talking; but the notes, too, left him in the lurch, and the philosopher for that day went unmentioned. This is the only time that I knew him to refer in lecture to any written aid; and he remarked afterward that the experience had not been encouraging.

As to the ultimate significance of that task, it would be the part of wisdom to keep silence; we stand too near to Wundt to see him in a just perspective. But I have formed my judgment, and will state it for what it may be worth. I take Wundt to be the first great figure in the history of thought whose temperament—disposition, attitude, habitual mode of approach to scientific problems—is that of the scientific psychologist. Whatever else Wundt might be doing, he also psychologised. He did not easily find himself; we have seen that there were years of wandering in the wilderness, and we have seen that the guidance which led him out of it is not readily determinable. When once he was free, however, he walked steadfastly in the path; year by year his psychology became sounder, as it also became more and more inclusive. A distinguished European psychologist wrote to me recently that he held no high opinion of Wundt's psychology because its theoretical views seemed to him to be nearly always wrong. Personally I do not greatly care about theoretical views; they are nothing more than an individual's blundering effort to bracket together and make manageable some large unruly body of observed facts. We may be sure, realising the limits of our acquaintance with fact, that whatever view we adopt will be inadequate, and we may fairly expect that increased knowledge will wholly discard it. We can only do our best with the facts available, as Wundt did, and trust to the future to do better by aid of further facts. But if a man is to gain his niche in history, he must have the total vision, the generative idea. And for that reason I believe that when Wundt's special theories have utterly perished his fame will still endure; it will endure because, for all the hampering influence of the past, he established a new point of view and from it surveyed the whole scientific and philosophical domain. In this sense I am prepared to say that Wundt is the founder, not of experimental psychology alone, but of psychology.

PORTRAITS OF WUNDT

The earliest portrait of Wundt that I know of is the academic photograph, reproduced by G. S. Hall in "Founders of Modern Psychology," which shows him in three-quarter face at about the age of forty-five. The portrait is of especial interest because Wundt's right retina had not yet suffered the injury that led to strabismus. An academic photograph of some ten years later is an excellent profile picture. I have also a very good platinotype enlargement of a three-quarter face, made by the university photographer, C. Bellach, in 1897. The *Berliner Photographische Gesellschaft* publishes a reproduction of a painting (almost full-face) by Dora Arnd-Raschid, which is an admirable rendering of first impression and remains, to my mind, distinctly preferable to the later official photographs. N. Perscheid's photograph of 1904 (published by the *Berliner Photog. Ges.* and reproduced in the album of photographs edited by M. Brahn for the Leipzig University jubilee) has its merits of pose; but it, as well as the photograph accompanying Wundt's *Festrede* in the official Jubilee volume, gives him a look of stolidity which is altogether misleading. The current postcard photograph exaggerates this effect of stolidity. There are photographs extant of the group that gathered at Tambach in the Thüringer Wald on the occasion of Wundt's seventieth birthday; all four members of his family appear in them. The Jubilee book entitled "Die Universität Leipzig 1409-1909" contains a full-face pen-and-ink sketch by O. R. Bossert, which at first sight strikes one as caricature, but which takes on resemblance as one grows familiar with it. Wirth publishes as frontispiece to the 40th volume of the *Archiv* a pencil-sketch (profile) made shortly after Wundt's death by Felix Pfeifer.—These, aside from the print in the Open Court Series and a few unimportant reproductions in popular magazines, are all the portraits of Wundt known to me.

In 1905 (the year of the golden jubilee of Wundt's doctorate) a bronze plaque showing the face in profile was prepared by Pfeifer. The bronze is eminently satisfactory; the reproduction accompanying Wirth's memorial article is disappointing; the rounding of the temple and the hollowing of the lower cheek, both characteristic features, are largely lost. The bronze can be obtained in two sizes; Wirth gives price and other particulars. Some ten years later a larger bronze plaque was made by Max Lange. There is a reproduction in the *Leipziger Illustrierte Zeitung* (1916?). There is also a bust by Max Klinger. Wirth calls it "gewaltig, aber künstlerisch stilisiert." I have seen neither original nor any reproduction.—

I do not know if there are any official photographic memorials of the old laboratory in the *Konviktgebäude.* I have a pencil-sketch, looking from the first room, with its stove and chronograph, through the *Vorzimmer* and past the resonance box for the giant fork to the entrance-door; and I have five amateur photographs of rooms, one of them showing Külpe lecturing in the auditorium. I shall be glad to know if there is anything else.

Wundt, in his historical article (*Festschrift . . . der Universität Leipzig,* iv., 1, 1909, 118 ff.), says that this old laboratory had five rooms; I imagine that one or two had been partitioned. At any rate I remember more. The *Vorzimmer* (1) was a narrow entry that served only as storeroom. Then came (2) the first room, with chronograph, case for tools and instruments, and table for optical work. Out of this opened (3) the dark room, in which "eine mit Rüböl gespeiste Moderateurlampe" used up more than its fair share of oxygen. Beyond the first room lay (4) the second room, with chronoscopes and instrument-case. Somewhere alongside of this, probably continuous with the dark room, was (5) Wundt's private room, which must have been served by a special staircase, since Wundt used to appear out of it and disappear into it without passing through other rooms. As I remember the glimpses through the open door, it contained nothing but a table and a couple of chairs. Finally, beyond the second room, came (6) the last of the suite, the *Lesezimmer.* Across the corridor were (7) a room containing the reaction keys and stimulators, electrically connected with (4), and (8) a small room containing the gravity phonometer, the Wundt pendulum, the Fechner pendulum, etc. If we count only (2), (4), (6), (7) and (8), we have Wundt's five rooms.

THE CHILD MIND

By Henry Jones Mulford, M. D., Buffalo, N. Y.

The world always has found the mind of the child interesting. Heretofore that interest has been an idle interest aroused by seemingly incongruous manifestations within the child-mind; but latterly the interest has taken on a serious intent. The merely glancing curiosity has become a focussed curiosity. The incongruous has appeared so regularly and so constantly that there has seemed to be some order in its manifestations, and the world at last has been forced to change its attitude. The attitude now is scientific. The world now seeks for the reason behind the manifestations, and in that search is exploring the whole aspect of the human mind. But the quest has had its difficulties. This territory, a veritable *terra incognita,* has been difficult of approach. In such territory direction must be sought and paths cut; and it has followed, naturally, that the investigator has made many starts in false directions.

But now, while the quest *is* difficult, is it as difficult as it has been made? Is mind the unknowable thing it has seemed to be? It is if we accept mind as an entity by itself, as a superphysical manifestation; for then the method of its manifestation is difficult to understand. It is not, if we accept mind as a natural phenomenon, as a phenomenon having a physical basis; for then all that we have to do to make it clear is to discover its physical origin.

And here we do not have far to seek, not farther than the brain; for is not the brain the physical basis of mind? Is not the evidence all in favor of this hypothesis? The gradual development of mind keeping pace with the gradual development of the brain; the absence of mind where there is absence of brain; the imperfect mind where there is imperfect brain; the suppression of mind where there is pressure upon the cerebral cortex, and its return when the pressure is removed. We find in fact that, as is the cerebral cortex of man, so is his mind. Every cell in that cortex reflects mind, every swelling convolution is an index of its increasing power.

Mind, therefore, is a physical manifestation; and, as an individual expression, reflects the peculiarities of the individual through whom it is manifested. The manifestation proceeds through the brain, but may be modified by the behavior of any of the other organs of the body. That is, the physical constitution determines the psychical constitution; the make-up of the individual determines his trend. That the mind is nothing beyond a physical expression must be so, for no living structure can express itself except in terms of its own cells: every tissue is limited by its own constitution. Among the living things upon this earth each separate group has its own radius of action; and in each group the individuals of that group have their own idiosyncrasies arising from individual peculiarities of structure. In man there is a greater individuality; there is a wider latitude in brain development; and it is this wider latitude that has brought about the development of the human mind. This greater radius of action has proceeded through the greater flexibility of the human brain. This brain has come into its own through the development of self-consciousness, through the faculty of conscious direction. *Brain has developed mind, and now mind is developing brain.*

A contradiction now becomes apparent. If the make-up of the individual determines the trend of his mind, how is it possible for that mind to choose its own direction? There may be discovered in this situation a suggestion of the highest importance leading to the proper understanding of the human mind; it contains a revelation of the process through which mind is developed. There is here a conflict; a conflict between the Past and the Present for the determination of the Future; the Past represented by the structure of the brain, the Present by environment as it acts upon that structure, and the Future by the result, that is, the individual. The conflict here is the old, never-ending conflict between heredity and environment as to which shall control the individual. And this is a real conflict. The Past, with its ancestral line extending back to the very beginning of human life, objects to resigning a control so long established; while the Present, *conscious of its own purpose,* demands that it be given a voice in the direction of affairs. The outcome of this conflict will be as the outcome of any conflict: the stronger will win. Following this rule the outcome will not be always to the advantage of the individual; he will not always have the choice as to his own direction. But there is a way to obviate this result. The individual may, if he wishes, make his own choice. But this will depend upon the state of development in which his mind rests, upon his degree of self-consciousness, *whether he knows what he is doing.* If he does know, if his state of consciousness is such that his mind can rise above impulse, his future is safe in his own hands.

Finding, then, that mind is an expression of the brain, we find also that, following the law of development of animal organs, the expression varies with the age. In accordance with that law the child-mind is a primitive mind; it is the primitive expression of a primitive organ. In this organ the conflict between the past and the present is, though very acute, very one-sided. The past is the dominant influence, but the present is making strenuous efforts to secure a foothold. Heredity and environment are striving for the mastery, but heredity holds the advantage in that it is the older.

Our understanding of the child-mind, then, depends upon our understanding of the child-brain, how it is developed and what it means. The child-brain represents the primitive human brain, the brain of a million years ago. In this brain the conflict between the past and the present has only just begun. It is emerging from the purely animal stage, but it is carrying with it the attributes of that stage; the brute is becoming the man, but the brute brain, following the habit of another million years, still asserts itself. It is this double expression of function in the human brain that confuses us. The Past and the Present are two separate entities; but, using the same apparatus, they appear as one. Our confusion has arisen through our looking upon these two as being, both of them, manifestations of mind. And there has been the great error. Only one is a manifestation of mind, the other being merely reflex action. Mind is consciousness, is the knowledge how to direct the brain reaction; the reflex is pure automatism, the response to external stimuli without conscious direction. But the mind, while itself a directing power, may be directed by the reflex, even though the reflex lies outside of consciousness. This is, in truth, the dominant reaction within the child-mind. Let us see how this comes about.

Every individual living at this moment is the present manifestation of an ancestral line extending back into the very mists of the beginning. Every human being represents a line of *human* life at least one million years long, and of life behind the human of no one knows how many millions. It may be perceived from this how complicated a structure the human organism is. It is a structure built up by slow, toilsome effort through countless epochs, each epoch having left its mark thereon. That this is so is becoming more and more evident; the slowly accumulating evidence of man's past points always in the one direction. Is it not true that the living things upon this earth to-day are but the descendants of other remote forms? Nature proceeds from one form to another; there is no spontaneous generation of higher forms. Man being a part of Nature can be no exception to its rules. He must have come from a lower form, and that lower form must have been an animal very closely related to him in structure. Of a truth the early man was little better than an animal. It must follow, then, that the man-brain in the beginning was no greater than its possessor, for the man and the brain are synchronous.

The transition from animal to man covered a period of great length. And this was in accordance with another law of Nature: the longer the period of development, the better the individual. In the beginning Nature made it easy for the developing man, for it was her purpose to nurse him into a strength that should endure through the long ages that were to be his upon this earth. The primitive environment was a supremely comfortable one; it was, in reality, a lazy one, for it was non-stimulating. The climate always was mild, food always was within easy reach; the primitive mind had little to disturb it. Life was at ease. The primitive mind did not even have to think for itself, its environment not being thought-inducive; and, not being obliged to think, the primitive mind remained as it had begun, a merely automatic function. It responded to external stimuli through the reflex; what little thought it had was reflex thought; mind was not yet strong enough to control the reaction.

The animal, whether brute-animal or man-animal, is a creature of reflexes: he is governed by reflex action. In the man-animal these reflexes manifest themselves in three directions. There is the basic reflex or cell irritability, there is the motor reflex, and there is the thought reflex. If we examine these carefully we shall find that they have developed as the human organism has developed; we find, in fact, that the organism has developed through these. If we go back a hundred million years to the solitary cell, to the time when it is estimated that life began upon this planet, we catch our first glimpse of the reflex. There we find cell irritability answering every purpose of the cell. The reaction there is relatively simple, being the reaction to a primitive environment. But now, advancing our investigation a few millions of years, we come to a period where the single cells have associated together to form organs, and the organs to form organisms. In these positions independent cell-action would mean inharmonious action; for not only are the cells grouped together, but they also have become differentiated. Cells thrown together into a group cannot act as independent cells, they must act together; and the different groups also must act together, else the organism will disrupt itself through the clashing of independent movements. And so we find here the demand for something that will harmonise the activities of the various independences. The primitive nervous system answered that demand, a system made up of a few nerve fibres conducting impulses to a central station, a station that was nothing more than a receiving station. Again jumping our investigation ahead, we find ourselves at a period when the organism, having reached to a very high degree of development, needs self-direction in order to utilize its higher power. It is at this period that we find the human brain coming into existence.

All of these reflexes interest us, each one according to its position. We are interested in the first, in cell irritability, because it is the starting point of all the reflexes. In the primitive cell the reflex was the expression of the reaction between the cell protoplasm and its environment. The purpose of this reaction was to maintain the well-being of the cell. It was, primarily, a reaction to food, but out of it have been developed consciousness and sex. In its search for food the cell came into contact with three varieties of substance: substances that maintained the integrity of its protoplasm, food; substances that were harmful to its protoplasm, foreign bodies; and the substance of other cells that revivified the cellular protoplasm and furthered the increase in the number of cells through segmentation. Out of contact with foreign bodies was developed tactile sensation, and, out of tactile sensation has come consciousness; out of intercellular contact has come sex. Consciousness, being a supremely higher expression of function and far more subtle, has required the full term of life upon this planet for its development; while sex, being an earlier requirement of animate organisms, was developed very early in the evolutionary process. It might be said, too, that out of this reaction to environment comes pathology. Overstimulation of the cell protoplasm produces excitement, and overexcitement in its turn produces fatigue, and there will follow either an impairment of function or its entire suppression.

Coming now to the motor reflex, we reach the reflex that is of the first importance in our present discussion, for it is through this that the mind had, *and has,* its real beginning. It was the first in actual brain development; it was the first visible reaction of the primitive nervous system; and it has been the dominating influence in the development of that system. Development of the higher centres adjacent to the motor has followed through the motor. The primitive central nervous system was, as we have seen, nothing more than an automatic station where the incoming stimuli were received, synchronized, and returned as motor impulses. But these reactions were inflexible; they always were the same. A given reflex always performed in the same direction. Being automatic it could not vary; its mechanism was set in the one certain direction. And, later, when the real brain came into action, this method persisted; for this brain having developed through the primitive system could act only after the manner of that system. Even though the animal possessed a brain his actions were not man-actions, for he did not yet possess man-consciousness; his brain was not yet fully organized. It was not yet able to direct itself. At the same time its reach was beyond that of the primitive station. It was the function of the primitive station to preside over the non-conscious activities of the organism, while the brain, ultimately, was to preside over the conscious; the one synchronized the organic activities, while the other was to direct the organism as a whole. The one must, of necessity, be automatic; and the other, while destined to proceed beyond mere automatism, had to begin as had the other. And so, when we come to the primitive man-animal, we find his actions the actions of the primitive brute-animal. They did not proceed through the thought reflexes of the man, they came through the motor reflexes of the animal. His radius of action, therefore, was limited; being reflex it was inflexible.

A third and last jump along the evolutionary path advances us to the period of thought-development. The primary influence in the production of thought was environment. Brain already had been developed, but it was brain that was not conscious of itself. But now great variations took place in environment. Extreme mildness of atmosphere gave place to extreme cold, and cold again to mildness; fearful convulsions of Nature altered the face of the earth; food became more and more scarce, and other living things needing food more and more numerous. The most important event in the history of primitive man took place during this period: the differentiation of his hands. While yet an animal he had learned to use his fore-feet as hands; but this use was automatic in that he used both hands as one. But now a varying environment brought varying uses for his hands. The work of the two hands was becoming finer, that of the right preceding and going beyond that of the left. It was the diverse influences of a varying environment that developed conscious man out of a merely automatic animal; but it was the increasing importance of his upper extremities that furthered the development of man's consciousness.

The new man was making new movements with his hands, movements that were, literally, out of reach of the brute; and he was associating those new movements with his higher reflexes. *His consciousness was taking hold of them.* The new movements meant different and finer adjustments of his muscles, meant independent action of the various groups, and a finer co-ordination between the groups themselves. They meant, also, co-ordination between the reflexes of those muscles and the higher reflexes; they meant that thought was becoming associated with action. And this increase in the reflex radius made further and further demands upon the brain. Following these demands the brain was forced to adapt itself to the new stimuli coming in to it.

It is to be regretted that we have no fossil remains of the brains of our remote ancestors, that we might compare the structure of the perhistoric brain with that of the brain of to-day. A number of skeletal remains have been uncovered in the deep layers of the earth's crust, but the soft parts, because of their very nature, were not able to endure with them. But advancing knowledge, knowledge built upon research and reason, is bringing to us a revelation of what the primitive brain actually was. In the light of this knowledge we are catching glimpses of the brain of primitive man and of that of his immediate ancestor, pre-man. This knowledge comes to us from two directions: from the examination of skulls found among the skeletal remains of primitive individuals; and from the examination of living brains that seem to approximate, in size and structure, those of the primitive animal.

Inspection of a primitive skull reveals the following facts in regard to the individual to whom it belonged. First, the layer of the earth's crust in which it is found will determine the epoch of the earth's history to which the individual belonged when living. Secondly, the external configuration of the skull will determine whether the individual was brute or man. The determining factors here are: size and shape of the skull as a whole; size and external configuration of the individual bones, this applying especially to the bones to which the muscles of mastication and to those to which the muscles of the neck are attached. Thirdly, the internal configuration of the bones of the skull will give an idea of the development of the brain that occupied the skull cavity; and the size of the cavity will give the size of the brain.

The living brains of to-day that will help us to an understanding of the primitive brain are the brains of those primates known to us as the anthropoid apes. In very fact, if the brains of the entire monkey series be examined, a very suggestive progressive relationship in regard to size and function will be uncovered. Let me emphasize this by going over, in a very superficial way, several of the varieties of brains belonging to the monkey family, limiting the examination to the lateral aspect of the cerebral hemispheres.

Beginning with the brain of the *marmoset* we find the outer aspect of the cerebrum completely smooth, there being only the Sylvian fissure in evidence.

In the brain of the *capuchin monkey* the convolutions begin to appear, the smooth surface of the cerebrum being divided into broad convolutions by fissures more or less superficial. Of this surface it is seen that the sensori-motor area, the area embracing the precentral and the postcentral convolutions, occupies about one-third, and the areas of the frontal, the temporal, the occipital, and the parietal lobes each about one-sixth.

The brain of the *bonnet monkey* resembles very closely the brain of the capuchin monkey. There is the same general configuration, with the same relative proportion of cortex in the different areas; but here there is a greater attempt at fissure production, and hence a greater area of cerebral cortex.

In the brain of the *yellow baboon* we have an organ larger than either of the preceding, but one in which the configuration of the cerebrum is much the same. But here the fissures have become decidedly deeper and more numerous, with a consequent greater number and a finer arrangement of the convolutions. In this brain the sensori-motor area occupies about one-fourth of the cerebral surface, and the other areas about the same relative proportion of surface. A point to be noted here is that, while the relative proportion of the frontal lobe is the same, the actual area of that lobe is greater in this brain than it is in the brain of the capuchin monkey.

Coming now to the smallest of the anthropoids, the *gibbon,* we enter a region in which the brain topography suggests something more than mere brain. The sensori-motor region is still prominent; but those areas closely adjacent to it, the frontal and the parietal, the so-called areas of the higher centres in man, have altered. In the frontal and the temporal the fissures have become deeper and, for the first time, three distinct convolutions have appeared; in the parietal the convolutions have become more complicated.

The brain of the *orang,* one of the larger anthropoids, presents a still more complicated surface, especially in the frontal region. The convolutions of the entire surface of the cerebrum are larger and extend more deeply than the convolutions of the cerebra of the previous brains.

The last anthropoid brain which we shall discuss here, that of the *chimpanzee,* is the most interesting. In this brain there is a very close approach to the human type. Its fissures are deep and of good length, especially the Sylvian and the central; its convolutions are well modeled and of a more complicated pattern. Here for the first time we find the frontal region exceeding the sensori-motor in surface. The third frontal convolution has become tortuous and bent upon itself, being suggestively like the same convolution in the human brain. The parietal lobe has widened, and the temporal has increased its convolutions.

Comparing, now, the human brain of to-day with the preceding series we note the ascending similarity; and the thought comes to us that in evolutionary history these brains must all be chapters in the same story. We note the general increase in cortex over the entire cerebrum, but more specially in certain regions. The frontal, parietal and temporal lobes have now become well-defined portions of the brain. The difference here is the difference that accords with the new function taken on by the human brain, that of mind.

I am making no definite assertions as to the position of the immediate ancestors of man; I am saying, merely, that study of the brains of the monkey family will suggest much in regard to the developmental plan of the human brain. Interest in this study will direct our attention in three directions: first, to the external configuration of the brain; secondly, to the microscopic structure of its convolutions; and thirdly, to the configuration of the inner table of the skull against which the convolutions lie.

Following the suggestions it will not be difficult to trace the growth of the man-brain out of that of pre-man.

The pre-brain was a negative brain. It was not self-assertive, self-directing, self-conscious; it was entirely vegetative, merely a reflex station. It was not active, it was reactive; it was not conscious, it was non-conscious. Knowing how this brain performed, we can gather some idea as to its form and structure. This brain, having been a reflex brain, had as its main centres those that were purely reflex, the motor centres and those having to do with the special senses. Among these the area comprising the so-called sensori-motor region, the precentral and the postcentral convolutions, was the oldest. It may be that the postcentral convolution is older than the precentral. The fact that the sensory fibres are medullated before the motor would indicate that; but this is not a vital point. The two work so in unison that they may be considered to be of the same age. Following these in regular developmental sequence came the centres for smell and taste in the lobus pyriformis, the centre for sight in the occipital lobe, and the centre for hearing in the temporal.

These regions were the oldest regions of the animal brain; their centres were the first active centres, and the limit of action, of the pre-brain. If it were possible to inspect a pre-brain, and its accompanying skull, we should find that the brain was small and symmetrical, with the convolutions carrying the above-mentioned centres the most prominent ones of the entire organ. There would be other cortex, but there would be only enough to allow for the natural expansion that must follow in an organ of this character. We should find some cortex anterior to the precentral convolution, a very rudimentary frontal lobe; we should find some between the postcentral convolution and the centre for sight, a beginning parietal lobe; we should find a very small amount below the first temporal convolution; and there would be a small silent area in the occipital lobe. Inspection of the inner table of the skull enclosing this brain would reveal the impressions made thereon by the convolutions containing the primitive centres, while examination of the centres themselves by means of the microscope would show the characteristic cell and fibre arrangement of to-day in a primitive but well-marked stage of development. In the excess regions the microscope would reveal only a few scattered very rudimentary cells and fibres.

Advancing a further stage in the animal phylogeny, to a period just preceding the emergence of the man-brain, we should probably find a brain of the type of the chimpanzee-brain of to-day. In this brain we find the primitive centres still prominent, still making the deeper markings upon the inner table of the skull; but we find, also, that these markings have altered. The positions of the deeper markings have changed: they are more extensive and are further apart; and other markings are beginning to appear. The areas of the primitive centres have increased somewhat, while the increase in the new areas has been more marked. The region anterior to the precentral convolution has grown forward into a distinct lobe, with deep fissures and complicated convolutions. The third frontal convolution, *Broca's convolution in the human brain,* has become more complicated than any of the others of this region, while those convolutions closely anterior to the precentral are the next in the order of development. In the parietal and the temporal regions corresponding increases in the convoluted surfaces will be found.

We see in this arrangement of the cortex of the animal-brain what seems to be preparation for the higher function of the man-brain; in thus expanding, the animal-brain is laying the foundations of those centres that, arising through its own primitive centres, are to raise the animal to the man class. Comparison of the microscopic structure of the chimpanzee-brain with that of the man-brain will demonstrate this.[1] The minute structure of the cerebral centres of the chimpanzee will be found almost to duplicate those of man. The difference is the difference that might be expected between an undeveloped and a developed brain.

Another step with our phylogenic seven-league boots and we find ourselves actually within the domain of man. The brain of man, well-advanced along the developmental path, is before us. Looking at it carefully we note the difference between this brain and the brain of pre-man. The pre-brain is small, symmetrical; the man-brain is large, asymmetrical. The difference is eminently one of development; development has increased the scope and the complication of brain-function, and has caused the asymmetry. The newer regions have doubled their capacities in the man-brain; but in the development of the new centres the development has proceeded unevenly, some have developed earlier, and some faster than others; each centre, produced through long ages of effort, has its own position in a regular developmental sequence. It is thus that the human cerebra have developed irregular and unequal contours.

In this brain the promise of the pre-brain is fulfilled: the man-brain has become an assured fact. The centres most concerned in this advancement are those situated within the frontal lobe: the centre for speech, and those centres which are the outcome of the differentiation of the hands. The parietal, too, has become of great importance, but its importance is secondary to that of the frontal. The frontal leads in those functions which are peculiarly man-functions. The new functions are associated functions; that is, they are associated with other functions in their operating mechanism. This association is two-fold: with centres immediately adjacent, and with centres in other regions of the brain. As an illustration of the first we note the centre for speech in the frontal lobe, which seems to be merely an enlargement of the motor area; the centre for word-hearing in the superior temporal convolution, which is actually an extension of the centre for hearing in the same convolution; and the centre for word-seeing in the gyrus angularis, an offshoot from the centre for sight in the adjacent occipital lobe. As an illustration of the second we note the association of the centres in the precentral and in the frontal regions to the visual centre at the posterior extremity of the cerebrum.

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[1] See "The Localization of Cerebral Function," by Alfred W. Campbell, for very exhaustive researches in this direction.

But these new centres, while offshoots of old, have not been made in a moment; in the upbuilding of brain-tissue Nature requires time. There is no spontaneous creation of tissue here, it is the tedious process of slow ages. And in this tissue there is a peculiar situation. While it gives no evidence of activity during its upbuilding, still it is not inactive; while it cannot respond to external stimuli, it can receive them. This is in line with the developmental plan. Sensory fibres are medullated before the motor; sensory stimuli are received long before motor responses can be returned. It is the prickings of the ingoing stimuli that develop consciousness in the centre. This is not to say, however, that there is no attempt at expressing itself on the part of the centre during its upbuilding. It is possible for this centre to make the attempt, but the action resulting from such attempts must, of necessity, be imperfect. No centre is capable of normal action until the structural elements of that centre have become fully developed.

This is shown in the development of the speech centre. Pre-man did not have speech, but he did have sound, uncouth, unmodulated noises. In making these noises he used the muscles concerned in the act as he used his other muscles, as group muscles; upon the impulse *all* the muscles acted together. The action was entirely motor, merely reflex action; there was no consciousness behind the act. After a time the animal found that he could control his voice somewhat, that he could change from one tone to another, that he could modify the uncouth noises proceeding from his throat. He still made the uncouth noises under the stress of sudden emotion, but at other times he was able to guide his utterances into a kind of chatter. At first this was just an aimless chatter, but eventually he was able to inject a meaning into the sounds issuing from his larynx. He had become conscious of his larynx, and that consciousness gave to him the power to direct its action. But that power was a long time coming to him; from uncouth noise to co-ordinated sound covered a vast period of developmental effort. The mechanism of speech was too complicated to be adjusted by any shorter process.

This is the method through which the speech centre was developed, and it is the method through which all the brain-centres have been developed; it is the method through which the man-brain has been developed out of the animal-brain. It also is the method by which the individual brain of to-day is brought into function; that is, it is the method through which the adult brain is developed out of the child-brain. The child-brain is not born already developed, *it is born to be developed.* Its position is that of the man-brain during the childhood of the race: it is on the threshold of a higher life. But while its position is the same, its condition is different. The brain of the primitive child was an animal-brain, pure and simple. Its active centres were only those in the primitive areas; the other centres were not active for the reason that they were not yet fully developed, they were in the process of being laid down. *These centres were being created.* In the child-brain of to-day the condition differs in this respect: while the child-brain is an animal-brain in one direction, it is a man-brain in another. It is an animal-brain in respect that it is active mainly through the primitive centres; and it is a man-brain in respect that the man-centres already are laid down. It is actually an embryonic man-brain, for the foundations of the man-brain are there; the foundations are not being laid down, they are finished.

While, however, the child-brain is born in this condition, it is not yet a completed brain. As has been said, the child-brain is a brain to be developed. It is ready for action, but as yet it cannot act, that is, as a man-brain. The new centres, having been created, now must be brought into efficient function. And so, while the child-brain is getting its full growth, these centres are busy receiving stimuli and training themselves for action. At the end of eight years, post-natal, the human child-brain should be able to take care of itself.

During the period of life of the human brain beginning at the fourth month of pre-natal life and ending at eight years post-natal, it exhibits all the phases of development that we imagine the racial brain to have experienced. Up to the period of the fifth fetal month the cerebral surfaces are completely smooth, the Sylvian fissures being the only fissures in evidence. During the fifth month the other fissures begin to appear, the calcarine, hippocampal and collateral on the median surface, and the central, precentral and the superior temporal on the outer aspect, being among the first. At seven months the surface of the cerebrum is well convoluted, while at nine months the outlines of all the convolutions are completed. Up to this point the size and contour of the human brain remain small and regular, resembling very closely the outlines of the anthropoid brain, but differing from it in one important respect: the extent of the frontal region. After birth the human brain continues its growth; this further growth placing it well in advance of the primitive brain it was. In short, the period of intra-uterine life might be likened to the evolutionary period during which the foundations of the man-brain were laid down, and the period after birth to the period during which the man-brain enlarged those foundations.

Following our discussion of the phylogeny of the human brain we now are the better able to understand the meaning and the method of the child-mind. It now becomes certain that the child-mind is but the expression of a developing brain, and that the expression follows the method of the organ from which it emanates. A further fact here, the puzzling factor in the reaction, is that this expression is the expression of a new function in the course of its development out of an older, firmly established function, the development of the man-function out of the animal-function. This new function varies as it grows, and as it grows has to fight its way against the dominance of the old function. The man is in a contest with the animal: the Present is in a contest with the Past. Our position here, then, should be that of supervisor, of director. We shall need to assist the new function to establish itself, we shall need to assist the man in his fight against the animal, we shall need to assist the Present in its contest with the Past. Left to itself the new might, out of sheer inertia, allow the old to overbear it.

If the child-mind is the equal of the child-brain, then the child-mind is a primitive mind, just as the child-brain is a primitive brain, and the method of the child-mind will be the method of the primitive brain. We saw that the centres of this brain came into action one after the other in an orderly sequence; and we saw further that these centres were developed through the reflex, that their reactions were touch-and-go reactions. The action within the child-mind is purely reflex;

it responds to environment automatically. The mind comes into development slowly, in response to environmental stimuli repeated over a long period. *The centres of the brain have to be prepared for function before they properly can functionate.* To be sure, the foundations of the centres are there, but they are mere foundations put there for the support of the superstructure. But we must not lose sight of one very important fact here: the foundation is the foundation of the man-brain. Therefore, while the basic reaction of the child-brain is reflex, following its animal origin, the secondary reaction is that of the man-brain. In the child-brain these two are very closely associated, the first beginning during the intra-uterine period and the second manifesting itself early in the post-uterine.

The growth of all the human brain-centres illustrates this; but, for a specific illustration, let us touch again upon the centre for speech. We have noted the disposition of Broca's convolution in the anthropoid brain and in the man-brain; and we have noted, also, how function seems to follow that disposition. We note the progression from no-speech to full speech; and, following the development of the brain of the human infant, we find this same developmental sequence there. From the moment of his birth the infant begins to make sounds through his larynx; but these first sounds are not speech-sounds. They are mere noises, uncouth, incoordinated cries. They are signals of distress, that is, of pain, of hunger or of shock. They are pure reflex manifestations; and the capacity for these manifestations never is lost. But after a bit we find that the baby has increased his vocabulary, as it were. He begins to laugh and then to coo; and then, as his development continues, as he gets older, his utterances take on a letter or a syllabic form, and later still a word form. Following the coo the effort is over a single syllable, such as "da," for instance. But the baby will not be able to say "da" at first. He will say "d," haltingly, in the beginning; and then, as the sound comes more and more easily, he will repeat the letter over and over in quick succession. That established, it will be an easy step to the full syllable, and he will use the "da" as he did the "d'." Frequent repetition of "da" automatically creates a word, and soon we hear the baby saying "da-da." Then he learns to associate that word with his father, and speech for him has been established. The centre controlling speech has come into consciousness; and from "da-da" to "ma-ma" and other short, intimate words no great effort is required.

Two factors are behind the progress of the infant brain: that it is an embryonic man-brain, and that the baby is in close association with his environment, represented here mainly by his mother. It is the example of the mother that has encouraged the baby-brain to exert itself. She has laughed and cooed with the baby, and has urged speech upon him; but all her urging never would have made the baby-brain exert itself if that brain did not have the power to exert itself. If it were not an embryonic man-brain it could not respond to the man-stimulus. The human child takes in words, recognizes them and then repeats them; but he does these things because his brain belongs to the man-class, because his brain is developed for the purpose. The man-function responds to the call of the man-environment, but the response in the beginning is only a reflex response; the reaction is the primitive reaction, for the mechanism is still primitive. But soon the constant effort at responding to the incoming stimuli enlarges the grasp of the cerebral centres, they become more and more conscious of what they are doing, and they begin to lose that purely animal characteristic, the reflex. But in every stage of the development of the child mind the reflex remains the dominant factor. Consciousness should be the dominant power in this brain, but it is not. Consciousness does have the directing power, but it does not have the power actually to direct. That is, consciousness is not yet strong enough for independent action; and, as with consciousness comes thought, then thought can have no greater strength than consciousness. Thought itself is, at this stage, little better than reflex action. The fact of the dominance of the reflex is the fact of greatest importance in brain development. The reflex dominated the primitive brain, and the reflex dominates the child brain. But that is not the reaction most to be desired in the human brain. It is the thought reaction that should have first place, *directed action;* consciousness should have supreme control.

We have, then, in the mind of the child factors that are subtle and far-reaching. We have the Past, an influence-complex that reaches up a thousand hands out of a loosely knit and interminable ancestral line; we have the Present, an influence-complex developed out of the action of environment upon the millions of cells that make up the cerebral cortex. It is the reactions between these complexes that determine the condition of the child-mind, or of any mind. It is the Present-Past reaction that gives the interest to the subject: it makes the child-mind, in very fact, the most interesting thing in this world. The problem to be worked out is a problem in development: how may the child-mind be developed to its own best advantage? As we have seen, this is a matter of brain-development, not to be understood until the whole process of brain-development is understood. The basic facts here are these. The development of the man-brain out of the animal-brain, and hence the development of the centres of the man-brain out of the centres of the animal-brain; the development of the sensory fibres before the development of the motor; and the dominance of the reflex.

The development of the child-mind is merely the development of consciousness in the child-brain; the development of a man-power brain. But the method of developing that consciousness must be the method followed by Nature. It must follow through the reflex, and is entirely a matter of training, a training that is directed through the motor centres. Each centre must be approached in the direction of its origin. We must remember that each centre has a regular developmental position and its fibres a developmental sequence; first the afferent, next the efferent, and then the association. The afferent stimuli are the ones that arouse a centre to action and that give its reactions smoothness. After a centre has undergone this training for a certain length of time it becomes able to control its own machinery; the centre then has developed its own consciousness. But that is not enough, so far as the mind is concerned. It would suffice in the case of a purely automatic brain; but it does not suffice for independent mind. Making each centre independent makes for disharmony. Centres working alone do not work together. The ultimate endeavor, then, in striving to develop the child-mind, is, while we are developing consciousness in the centres, to make that consciousness overlap from one centre to another, to bring about an interaction between the centres. The aim is, through the development of supreme consciousness, to convert the human brain into a symmetrical and a harmonious organ; that is, an organ fully developed and fully able to take care of itself.

ON THE RELEVANCY OF IMAGERY TO THE PROCESSES OF THOUGHT[1]

By Claire Comstock

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In an article in the *Zeitschrift für Psychologie*[2] Dr. Koffka of Giessen says in criticism of an analysis of "Conscious Attitudes":[3] "It is obvious that analysis meant for the author and her observers nothing else than the exhibition of the sensory contents present at any given moment. . . . These sensory contents may [however] be irrelevant to the thought, or may be the necessary condition of the arousal of the thought, or may finally be the thought itself." Clearly the value of the analysis of a thought-process is dependent upon the relevancy of the contents which constitute that analysis to the thought-process analyzed. To find a criterion of relevancy and irrelevancy we undertook the following series of experiments. More particularly, we hoped to secure a basis for answering such a question as that asked by Koffka: How do we know that any sensory content is relevant or irrelevant to a thought? We have concerned ourselves especially with the relation of imaginal contents to thought, since it seemed wise to limit the problem. The *O*'s have, however, frequently mentioned in their reports the parts played by sensory kinaesthesis and by feeling.

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[1] From the Psychological Laboratory of Cornell University.

[2] 63, 1912, 219.

[3] H. M. Clarke, Conscious Attitudes, *Am. Jour. Psych.*, 22, 1911, 214-219.

Relevancy may be of two kinds, material and logical. It is possible for imaginal contents to be materially relevant but logically irrelevant to a thought. For example, in reply to a question concerning the number of small boxes contained within larger ones, an *O* gave the correct answer, 21. When asked to report the imagery upon which this answer was based, he described a complex visual image of white boxes about six inches square with a grey interior. Logically, the grey and the white and the dimensions have nothing to do with the solution of the problem. The boxes might just as well have been red, or have had no color ascribed to them. Materially, however, the imagery was relevant, since for this particular *O* the meaning ' box ' was carried by the particular kind of box described.[4]

We started out with the belief that we might find in the analysis of thought-processes a good deal of irrelevant material. We expected to have reported contents irrelevant to the thought concerned. Our task was then to be a determination of the psychological criteria of this irrelevancy. The results of our experiments have, however, forced us to the opposite point of view. They show that, if imagery is present as part of the contents of thought, it is *ipso facto* relevant to the thought. This conclusion we reached only at the end of a series of experiments, in every one of which we had been ' set ' to find irrelevant imagery in Koffka's sense.

We attacked the problem first on the side of relevancy, though always with the expectation of getting indirectly at irrelevancy. Our task was in part one of method, and the attempt to secure certain experimental conditions explains the sequence of the separate experiments which we undertook.

Experiment A. 1. Problems

Our aim here was to study the imaginal contents of thought with reference to its uses and relevancy to the thought. The method consisted of presenting to the *O* a simple problem to which he was asked to give an answer. After the answer had been given, he was asked to report the experiences upon which it was based. There were 83 problems or questions which may be roughly classified as follows:

1. Arithmetic problems (9).[5]

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[4] Cf. H. L. Hollingworth, The Vicarious Functioning of Irrelevant Imagery, *Jour. Philos. Psych. Sci. Meth.*, 8, 1911, 690.

[5] The numbers in parentheses refer to the number of problems of the various kinds.

Ex. At ten cents a yard, how much will eighteen feet of cloth cost?

2. Ingenuity problems (12).

(*a*) Easy (9).

Ex. A man wanted to catch a kitten, but the kitten ran up a tall tree which no person could climb. How could he get the kitten without hurting it?

(*b*) Difficult (3).

Ex. Out of 6 toothpicks make 4 equilateral triangles each one of whose sides shall be as long as a toothpick.

3. Abstract problems (3).

Ex. If the possession of money or wealth in any form should come to be regarded as dishonorable, what significant changes would result?

4. General Information problems (24).

Ex. Where is the painting, Mona Lisa?

5. Completion problems (4).

Ex. Supply the missing letters: F-r o-f-c-a- b-s-n-s- o-l-.

6. Enumeration problems (8).

Ex. If a box has 4 smaller boxes inside of it, and each one of the smaller boxes contains 4 little tiny boxes, how many boxes are there altogether, counting the big one?

7. Action problems (6).

Ex. Suppose that you stooped down to lift up a large bucket full of water, but that as you stooped down to lift it up it proved to be empty. What would happen?

8. Simple Judgment problems (5).

Ex. What is the thing to do if you go to sleep on the train, and do not wake up until you are several miles past the station where you wanted to get off?

9. Comparison problems (6).

Ex. If grey is darker than white, and black is darker than grey, what shade of those named in this sentence is lighter than grey?

10. Direction problems (3).

Ex. Suppose that you are going north, then you turn to your left, and then to your right. In what direction are you going now?

11. Imaginal problems (3).

Ex. Suppose that it is fourteen minutes before three o'clock. Now suppose that the two hands of the clock were to change places, so that the large hand takes the place of the small hand and the small hand takes the place of the large hand. What time would it then be?

We tried to include problems of various types, ranging from very simple questions, to which the answers were given immediately and automatically, to more difficult problems where complex processes of thought were involved. We hoped also to give opportunity for the use of different kinds of imagery, as visual (group 11), kinaesthetic (group 7), verbal-motor (group 1), etc; though we realized, of course, that the type of imagery used depends principally upon the imaginal type of the *O*.

At first the problems were typewritten and given to the *O*'s to read. With this procedure it was, however, impossible to control conditions, since the *O*'s tended to glance back over the problem, even though only one reading was formally allowed. This source of error prevented the taking of a time-record, which is sufficiently rough at best, since here the reading of the problem and its solution were inextricably combined. We then changed our procedure; *E* read the problem to *O* who was seated with his eyes closed and his back to *E*. When the last word of the problem had been read, *E* started the stop-watch, and stopped it when *O* gave his answer.

The instructions were: "I shall set you a simple problem. Your immediate task is to solve the problem. After you have reported your answer, I shall ask you to describe as well as you can the experiences upon which your answer was based." Later, to provide for a report in attributive terms, we changed to the following instructions: "I shall ask you a series of questions. Please give your answer as soon as it is ready. After you have answered, report so far as possible in attributive terms the experiences upon which the answer was based."

There were five *O*'s,[6] all of whom were graduate students or instructors in psychology; Miss A. H. Sullivan (S), and Messrs. H. Sheppard (Sh), M. J. Zigler (Z), H. S. Liddell (L), and L. B. Hoisington (H). Four of these *O*'s, S, Sh, Z and L, worked two periods a week; and one, H, worked one period. The observation-periods were usually one hour.

An analysis of the reports shows that imagery was used in a number of ways in the solution of the problems. A table indicating the different uses and the number of instances follows.

Uses	Number	
1. Anchor		
(*a*) Illustrative	136	
1. Memory experience	5	
(*b*) Non-illustrative	31	
(*c*) Combination of a and b	10	182
2. Subject-matter of Problem		
(*a*) Changing	155	
(*b*) Fixed	57	
(*c*) Combination of a and b	9	221
3. Regulation of Problem		
(*a*) Formulation	20	
(*b*) Anticipation	2	
(*c*) Criticism	17	39
4. Means of Escape from Problem	5	5
5. Illustration of the Answer	16	
(*a*) Reinforcement	5	21

Total number of reports = 308.
In 21 instances the answer came as a sensorimotor response.

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[6] Primarily the 'observers' in this study were 'subjects' who were performing a set task. We have named them 'observers' simply because our aim was to get them to describe the processes correlated with the meaningful stages in the performance of a task.

Illustrations of the different uses taken from the reports follow.

1. *Anchor.* (*a*) *Illustrative*

Problem 27. If the conductor on an Ithaca street-car rings up in one trip 41 fares, how much money has he taken in?

Report: "As the problem was read through, there was a scrappy visual image of something that meant 'conductor' (upper part of him) and of the tally machine at the other end of the car" (H).[7]

1. *Memory*

Problem 22. What would you do if a person who you know is crazy calls you ugly names?

Report: Visual image of a man sitting on a rock. It carries the meaning of the time when I was very small and one of our neighbors went crazy and my father struggled to keep him quiet" (Sh).

(*b*) *Non-illustrative*

Problem 56. If James had four times as much money as George, he would have sixteen dollars. How much money has George?

Report: "The two names were held by visual-verbal imagery. It meant the names 'James' and 'George' written in white on a black surface. These fluctuated in clearness. Occasionally both were present at the same time. They were spatially separated" (Z).

(*c*) *Combination of* (*a*) *and* (*b*)

Problem 64. What makes salt cake?

Report: "A visual image of a little pile of white salt. Verbal-motor repetition of problem: 'What makes salt cake?'" (Z).

2. *Subject-matter of problem.* (*a*) *Changing*

Problem 75. From what other method of transportation are the terms used on railroads taken?

Report: "Visual image of a blue-brownish mass. Meant 'boat.' Vanished quickly. Then a visual image of a coach going along a dusty road. In verbal-motor imagery the words: 'carriage,' 'coach,' 'pullman'" (L).[8]

(*b*) *Fixed*

Problem 25. If the two diagonals of a square are drawn, how many triangle are thus formed?

Report: "Visual image of a square with the diagonals not completely filled in. The field is white and the lines black. I saw part of all four triangles" (L).

(*c*) *Combination of* (*a*) *and* (*b*)

Problem 67. Name three countries of Central America.

Report: "A visual image of a map of South America with its countries. Verbal-motor imagery of naming the countries (from the map) before I spoke them" (Sh).

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[7] Cf. K. Bühler, Über Gedanken, *Arch. f. d. ges. Psych.*, 9, 1907, 353; H. J. Watt, Experimentelle Beiträge zu einer Theorie des Denkens, *ibid.*, 4, 1905, 361 ff.; and A. Messer, Experimentell-psychologische Untersuchungen über das Denken, *ibid.*, 8, 1906, 67 ff. The reports cited furnish instances of our 'anchoring' imagery.

[8] For illustrations of changing imagery cf. A. Messer, *op. cit.*, 57.

3. *Regulation of problem.* (*a*) *Formulation*

Problem 10. A boy was sent to the river to bring back exactly 7 pints of water. He had a 4 pint vessel and a 9 pint vessel. Show how he can measure out exactly 7 pints of water, using nothing but these two vessels and not guessing at the amount.

Report: "A visual image of two vessels, glass. One was half of the size of the other. One meant '4' and the other '9.' Then verbal-motor imagery meaning: 'You'll have to solve by interchanging in two vessels'" (Z).

(*b*) *Anticipation*

Problem 68. You say a flock of sheep, but a what of mackerel?

Report: "A visual image of sheep. Then I anticipated what was coming. This was carried by visual imagery of mother's flock of white leghorns and verbal-motor imagery in the naming of them" (Z).

(*c*) *Criticism*

Problem 17. What holiday comes nearest the middle of the year?

Report: "I thought of Christmas. This was carried by verbal-motor imagery" (L).

4. *Means of Escape from the problem*

Problem 70. What is a firkin?

Report: "I don't know. I thought of Oscar Firkins, a professor of English. This was carried by kinaesthetic and verbal-motor imagery" (S).

5. *Illustration of the answer* (coming after the answer)

Problem 47. Suppose that you are going upstairs in the dark and think that there is another step ahead of you. If there isn't, what happens?

Report: "Following the answer I had a visual image of the head of the stairs, and of a person there with his head down" (H).

(*a*) *Reinforcement*

Problem 6. Which is heavier, a pound of lead or a pound of feathers?

Report: "The problem was solved at the end of the reading. I had a visual image of the word 'Neither' and a period after it. The capital 'N' was typewritten. This came after the answer had been given" (L).

Discussion of Results

The results show clearly that in the solution of problems and the answering of questions our *O*'s had recourse to imagery. The most frequently occurring use is, as we might expect, that of imagery as the material for working the problem. Second in importance is the use of imagery as anchor. There were some instances (to be considered later) in which no imagery was used in the solution of the problems, but in nearly all of these cases the problem was anchored by imagery. It seems to be necessary to hold the meaning of the problem, or to fix its essential parts in some fashion, in order to answer it; and this is the use made of the anchoring imagery. The attitudes included under "Regulation of problem" (3) probably occurred more frequently than they were reported. They are so largely meaningful that the underlying process easily escapes report. The meaning here is most often carried by verbal-motor imagery, though not infrequently other kinds of imagery do the work of formulation; and a visual image may correct a mistake or carry an anticipated meaning.

All of the imagery so far discussed is certainly relevant to the thought that it carries. We have, moreover, the statement of the *O*'s under the instruction to report "the experiences upon which the answer was based." Of a somewhat different nature are the five instances in which the imagery was used in aiding the *O* to evade the answering of the question. In three of the five instances reported the *O* was unable to answer the question, and so took refuge in imagery irrelevant to the problem-imagery. In the other two cases the answers were reached with difficulty, and the *O*'s allowed themselves to be side-tracked. We must note, however, that what we find here is not irrelevant imagery as such, but rather a shift to an attitude which is irrelevant to the problem-solving attitude. The imagery is relevant to the alternative attitude. The imagery which is illustrative of the answer (5) does not, of course, help in the solution of the problem. Its use seems to be the reassurance of the *O* that his answer is correct; and it carries in part the meaning of a feeling of satisfaction. In some instances it is purely associative. In any case the imagery is relevant to the attitude concerned.

There remain for consideration four phenomena reported by the *O*'s. These are:

	No. of cases
1. A felt need for imagery	6
2. Imagery as a hindrance	4
3. Irrelevant imagery	4
4. Cases in which the answer came immediately and automatically	20

We discuss these in the order above presented.

1. These instances show the dependence of the *O*'s upon imagery. The reports run as follows: *a.* "I was unable to get the hands of the clock changed in my visual image" (L). *b.* "I wanted to visualize and couldn't" (S). *c.* "This is difficult because I couldn't get a picture of the triangle" (Sh). *d.* "I tried to image the formation and couldn't, so that I used my fingers to help me out" (L). *e.* "I tried to visualize some paintings I had seen, and I couldn't" (Z). *f.* "I tried to get a visual image (to anchor the problem) and couldn't" (S). In 4 of the 6 instances (*c, d, e, f*) an incorrect answer or no answer at all was given. Of the 2 remaining cases, in *b* the *O* finally succeeded in evoking a kinaesthetic image which helped in solving the

problem; and in *a* the *O* reported that he could not be certain of his answer because of the incompleteness of the image which he had to use. These instances show that imagery not only is used when it is present, but also that its presence may be essential.

2. There were reported 4 instances in which imagery seemed to hinder rather than to help the *O*. Interestingly enough, though these include the reports of 3 different *O*'s, they are all of the same sort. The imagery which is reported as "being in the way" serves in every case as the anchor of the problem. Ordinarily, images serving this purpose drop out with the beginning of the solution of the problem, or become carriers of the processes involved in solving the problem. In 3 of the instances the anchoring images had the lure of familiarity, and were carriers of experiences more pleasing to the *O* than the duty of answering the question. This seems to be a form of "Means of escape from problem" (4). In the other case the *O* could not for a long time solve the problem, so that the anchoring imagery, which was very complete, was not replaced by any other imagery.

3. We have included under the heading "Irrelevant imagery" all imagery which was reported by the *O* as having nothing to do with the problem which he was solving. Since such reports bear directly on our main problem, we shall consider each one separately. *a.* Problem 6. Which is heavier, a pound of lead or a pound of feathers? Report: "A visual image of a flock of geese which some of my old neighbors used to have. There were several on a large pond and some on the bank. One spread out his wings and flapped them. This didn't help me solve the problem. The problem was solved at the end of the reading and this imagery came after the solution" (Z). The concluding statement of the *O* makes it clear that this is not a positive case of irrelevant imagery, but a case in which the imagery is relevant to a situation other than that involved in the solution of the problem. *b.* Problem 44. Can more than one meaning be attached to a sigh? If so, what meanings? Report: "As the phrase 'attach meaning' was completed, I thought of Titchener's 'Beginner's Psychology.' Then I thought of the next to the last lecture the first term. This was carried by visual imagery. . . . All of this bore no conscious relation to the answer given. The imagery came before the reading of the problem was finished" (L). It is evident that this imagery is used as an illustrative anchor which is anticipatory of the end of the problem. The last word of the problem, 'sigh,' demanded a shift in attitude so that the imagery reported is relevant to the attitude set up by 'meanings attached,' but not to the attitude which determines the answer. *c.* Problem 59. How would you criticize the following statement made by a judge to a prisoner: "You are to be hanged and I hope it will be a warning to you." Report: "A visual image of a young man, a round, ruddy lad. This is very clear. Other people are in the room. This is in the court-room of my home town. I think this is irrelevant [referring to the court-room]" (Z). We have here an example of imagery used as illustrative anchor. The irrelevancy, if there be any, is of the logical sort. *d.* Problem 6. Which is heavier, a pound of lead or a pound of feathers? Report: "The first thing that came to me was a remembrance of Prof. Angell, in his joking way, proving that psychologically feathers are heavier. This is not directly connected with the problem" (Sh). Again we find imagery used as an illustrative anchor, here in the form of a memory-image. It is difficult to see how this imagery is even logically irrelevant, since it obviously gives a clue to the answer.

4. A classification of the questions to which the answers came as sensorimotor responses throws light on the reason for this mode of response.

	No.
(*a*) Comparison problems	4
(*b*) Arithmetical problems	3
(*c*) Simple Judg. problems	3
(*d*) Gen. Informat. problems	6
(*e*) Easy Ingenuity problems	4

In the comparison problems the answer is dependent upon attention to the reading of the problem, for the answer is implicit in the statement. It may be necessary for some *O*'s to restate the question in order to answer it, but for others the auditory perception touches off the answer. The questions asked of the types *b, c, d,* and *e* are of so simple and habitual a sort that the answers, having been previously worked out, are "on the tip of the tongue." In other words, all of the 20 cases are instances of the presence of brain-habit.[9] As we have said above, we frequently find in cases of this kind some anchoring imagery, but none which is used as material out of which the problem is worked; such imagery is not needed.

Conclusions

I. We have shown (1) that, in solving a problem or in answering a question, imagery may be used in no less than 5 different ways; and (2) that in all cases the imagery reported is relevant to the thought whose meaning it carries.

II. During the course of the experiment there became obvious many imperfections in the method, which we shall now briefly consider. (1) *Difficulty in selecting the problems or questions.* The selection of problems or questions is by no means a simple matter. At first the *O*'s were allowed to read the problems and to refer to them in the course of the solution. This procedure, however, did not permit of recording the time taken by the *O* to solve the problem or to answer the question. A time-record was deemed desirable as a check on the number of processes reported by the *O,* since he sometimes seemed to report experiences occurring, not in his solution of the problem, but during the period of introspection. We then tried reading the problem to the *O* and taking a time-record as described above. This proved to be a better procedure, but meant a change in the kind of problem used. A problem involving in its statement several terms or different steps, or a problem long in general, either could not be comprehended by the *O* or could not be held in mind from a single reading. Hence only those problems which could be simply and briefly stated and easily grasped could be used. Questions of general information were employed with the hope of meeting this difficulty. It is not easy, nevertheless, to find questions which demand thought or present a real problem, and at the same time to avoid questions to which the answer comes automatically or to which the *O* can give no answer at all. There is, further, the task of selecting problems which shall not involve in their solution one type of imagery to the exclusion of others. The "clock" problems, the "folded and cut" paper problems, and the "box" problems are, for example, stated by Terman to test especially the ability to visualize.[10] The attempt was made to select problems appealing to other types of imagery as well. For example, we hoped that kinaesthetic imagery might be used in the solution of problems included in group 7. (2) *Difficulty in stating the problem.* We encountered a further difficulty in the statement of the problem. From the reports it was evident that the *O*'s were giving experiences set up by the descriptive part of the problem as well as those experiences upon which the answer was actually based. We were interested in the second form of report. To obviate this difficulty we tried to state the problems so that the important part for the answers should come at the end, as, for example: "What is a shoat?" (3) *Difficulty occasioned by the influence of the experimental attitude on the part of the O.* The *O*, having been informed in the instructions that he was to report the experiences upon which his answer was based, seemed in some cases to be disturbed by this requirement. This is shown by the fact (*a*) that some *O*'s "pondered" over questions which were simple, until there came to report something definite in the way of experiences which might underlie an answer reached almost immediately. For example, one *O* in his report on a "comparison" problem said: "The word 'oil' was articulated before spoken. I went through the problem in internal speech. The answer occurred immediately" (L). Yet the time recorded by the stop-watch was 14.4 sec. The tendency to wait for something "reportable" is perhaps the explanation of some of the illustrative imagery frequently reported. Some *O*'s delayed their answers until they were "ready to report," explaining the longer time thus required for the solution of the problem by remarks such as: "I was trying to think how I got the answer." Such a statement shows the honesty of the *O*, but makes the time-record valueless. The same thing is shown by the fact (*b*) that some *O*'s seemed to keep continually in mind during the solution of the problem that they were to report at the end, and accordingly introspected as they solved the problem. For example, one *O* said: " My introspections in this problem were separate from getting the problem. I have to answer the question and then go back to introspect " (S). Another illustration of the influence of the experimental attitude is the apparent fact (*c*)—*E* has no experimental proof except the time-records, which are otherwise difficult of interpretation—that in reporting the *O* sometimes added experiences then occurring to him, but experiences which were not a part of those upon which the answer was based. This, of course, is a trap into which it is easy to fall, and which can be avoided only by practice. It constitutes, nevertheless, one of the difficulties of the method, and involves the danger of assuming that the contents of the after-period are the same as those of the experimental consciousness. (4) *Difficulty in interpreting results.* The method puts the " burden of proof " upon *E*. His is the final interpretation of results. At best he can only check his interpretations by comparing the reports of different *O*'s and by repeating experiments. He is also aided by interpretations which the *O*'s sometimes " let slip."

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[9] E. B. Titchener, Thought-Processes, 1909, 178 ff., 201.

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[10] L. M. Terman, The Measurement of Intelligence, 1916, 321, 328, 339.

That the method lacks the accuracy of other experimental methods is clear. Nevertheless, as a starting-point in an experimental investigation, it is valuable. " It will always be of service where new ground has to be broken, and where the formations are so complex that an immediate recourse to experiment in the strict sense is forbidden."[11]

Experiment A. 2. Repetition of Fox's Experiment

With a view to further study of method and interpretation of results we undertook the repetition of an experiment reported by C. Fox in the *British Journal of Psychology*.[12] His problem and method bore some resemblances to ours in the experiment described above, and we hoped that a comparison of our results (from the repetition of the experiment) with his might throw additional light on both method and interpretation of results. We shall state briefly his procedure and the general results of his experiment and then give the conditions of our repetition of the experiment and our results.

Fox's subjects were told that " they were to investigate the existence and importance of thought without images, and to try to find out the content of such thinking."[13] They were also " to distinguish, as far as they could, between the thinking and the thought."[14] He worked with

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[11] E. B. Titchener, The Method of Examination, *Am. Jour. Psych.,* 24, 1913, 429 ff.

[12] C. Fox, The Conditions which Arouse Mental Images in Thought, *Brit. Jour. Psych.,* 6, 1913-14, 420 ff.

[13] *Op. cit.,* 420.

[14] *Ibid.,* 420.

15 subjects, all of whom had had "previous practice in introspection."[15] The material used was 12 statements: 4 involving mathematical conceptions, 4 historical, 3 grammatical, and 1 a couple of lines from Milton. What the instructions were is not clear. We know only that "the subjects were told to record on a sheet of paper everything they could discover by introspection after each statement had been read twice by the experimenter. They were told to put down everything, however unimportant it appeared to them. . . . In fact all details, whether mental or physical, were to be noted. They were also told to put down first of all whether they realised the meaning of the statement read to them; and as soon as the meaning was realised the process of thought which had led to its realisation. If possible they were to state what the realisation consisted of; and whether it involved mental images or not. In cases where images did arise they were instructed to state whether the realisation of meaning preceded or succeeded the occurrence of the mental image."[16] Fox states later, however, that "these instructions were only fully carried out as regards that part of them which related to the realisation of meaning and to the occurrence of images."[17] The general results of the experiment he states as follows: "any delay or conflict in consciousness is a favorable condition for arousing a relevant mental image. . . . The experiments also show directly that the contrary set of conditions are (*sic*) unfavorable to the production of images."[18]

In repeating the experiment we used the same material, though the statements were read by *E* only once. Our instructions differed from those of Fox in the following respects. (1) We did not tell our *O*'s that they were "to investigate the existence and importance of thought without images."[19] (2) The reports were made orally by the *O*'s. (3) The instructions were less full and suggestive than those used by Fox, in accordance with his statement that his own detailed instructions "were only fully carried out as regards that part of them which related to the realisation of meaning and to the occurrence of images."[20] Since we were not certain whether Fox meant the same thing when he asked his subjects "to state what the realisation consisted of"[21] and when he told them to give "the process of thought which had led to its realisation," we used two sets of instructions which read as follows. (1) "I shall read you a statement. Please report whether you realise the meaning of the statement read and, if so, report the process of thought which led to its realisation." (2) "I shall read you a statement. Please report whether you realise the meaning of the statement read and, if so, of what the realisation consists." The 12 statements of Fox were used as material with the first set of instructions; but in order to make a comparable situation we were forced to use different statements for the second set of instructions. We endeavored, however, to find sentences equal in difficulty and similar, so far as possible, to those used by Fox; that is, 4 were mathematical, 4 were historical, etc. We had 5 *O*'s, 3 of whom, S, H, and Z, had acted as *O*'s in our first experiment. The other two, Messrs. F. L. Dimmick (D) and H. G. Bishop (B), were experienced *O*'s.

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[15] *Ibid.*, 421.
[16] *Ibid.*, 421.
[17] *Ibid.*, 421.
[18] *Ibid.*, 430.
[19] *Ibid.*, 420.
[20] *Ibid.*, 421.
[21] *Ibid.*, 421.

Following Fox, we shall present our results for each group of statements separately. As stated above, the first set of instructions was used with the original material of Fox. We had some difficulty in the use of these instructions. All of our *O*'s asked what we meant by "process of thought." They were told to interpret it as they liked; with the result that all took the instructions to mean that a report of both process and meaning was desired. We were also questioned about the meaning of the words "realise" and "realisation." One *O* (H) said: "To me 'realisation' is the concreteness of the situation; it is illustrative. I could say 'yes' as soon as you stop reading; but I have a determination to realise the meaning, and this has an inhibitory effect on the 'yes.'". Another *O* (Z) stated that "verification usually, but not always, seemed necessary;" while a third *O* (D) remarked: "I never am aware of the 'realisation.' It is nothing but what I talk about after I have finished. It is the attitude of accepting or rejecting the meaning." It is obvious that these three *O*'s will give different kinds of reports. We should expect the first, as was actually the case, to report more concrete imagery than the other two. Fox's subjects, for the most part, seem to have had no difficulties with the instructions.

We come now to a consideration of the reports on the propositions in the first group (mathematical statements). From the analysis of his subjects' introspection, Fox draws the following conclusions. 1. "In the first place it is perfectly clear that a considerable amount of thinking is entirely independent of mental images. Of the 60 thought-processes of the 15 different subjects, 24 or 40% occurred without mental imagery."[22] Of the 20 thought-processes of our 5 *O*'s, 2 or 10% occurred without report of mental imagery, but not without observable processes. Such processes were mainly kinaesthetic sensations of strain around the face and eyes, inhibition of breathing, etc. Our *O*'s tended to repeat in verbal-motor imagery the proposition after it had been read by the *E*. It seems strange that none of Fox's subjects should have reported a similar tendency. 2. "As the statements selected would involve mental images of a very simple and definite type, if they occurred, namely of simple geometrical figures; and as the frequent mention of images during the experiment would in itself act as a suggestion to arouse images which would otherwise not occur, it seems probable that under normal conditions of thinking images would not arise in more than 50% of the cases."[23] This is sheer speculation. In the first place one cannot foresee what kind of mental images such statements would arouse, whether simple and definite or otherwise; nor has one any right to assume that "mental images of simple geometrical figures" are necessarily easier to call up than images whose complexity is greater. The fact that the *meaning* is of simple, geometrical figures does not prove that the *images,* or carriers of the meaning, will be simple. In the second place Fox criticizes his own procedure when he says that the "frequent mention of images during the experiment would in itself act as a suggestion to arouse images which would otherwise not occur;" since it seems that one aim of an experiment of this sort must be to avoid setting up suggestions. Furthermore, it is as logical to assume that the statement to the subjects that "they were to investigate the existence and importance of thought without images" would be as effective in setting up an attitude to report thought without images as in arousing the opposite tendency. At any rate, it is difficult to see why the suggestion should be valued at exactly 10%. 3. "In some cases strong imagery interferes with the act of thinking."[24] We found no such cases, though we found cases in which inability of the *O*'s to get an image prevented the full realisation of the meaning. 4. "Images tend to appear if the realisation of the meaning is not at once clear, or if there is a delay or a struggle in consciousness."[25] Our results confirm this statement, though in the case of our *O*'s verbal-motor imagery, which carries the meaning of a debate, is more common than the visual imagery reported by Fox's subjects. "Where the meaning is easily grasped or where assent has been previously given there seems to be no tendency to embody the thought in an image."[26] Though such instances are rare with our *O*'s, the assent or the realisation of meaning is carried by kinaesthesis of some sort. 5. "Suspension of judgment and doubt, both of which may be regarded as instances of delay or struggle in consciousness, are conditions which facilitate the emergence of mental images."[27] As we have already said, such attitudes were for our *O*'s characterised on the processual side by verbal-motor imagery, other modes of imagery, and kinaesthetic and organic sensations, together with feelings of pleasantness and unpleasantness. Although Fox had instructed his subjects to note "muscular strain or tension,"[28] no mention is made of it in the quoted reports.

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[22] *Ibid.,* 423.

[23] *Ibid.,* 423 ff.

The introspective reports for the remaining 3 groups of statements confirm, on the whole, both Fox's results and our own as stated above. 1. He says, in discussing the propositions of the second group: "If a subject makes a deliberate attempt to concentrate his attention on the meaning of a statement he may succeed in suppressing images which would otherwise occur."[29] This remark seems beside the point; for the fact that no images are reported because of "attention on the meaning" does not prove that images were not present and were not carriers of the reported meaning. Furthermore, this is a particular set assumed by one *O*, and its assumption is not justified by the instructions. 2. "Emphasis or a pause which constitutes a break in the free flow of thinking is favorable to the production of imagery. . . . This condition, too, seems to fit in with the general idea of a conflict or struggle . . . ."[30] The introspective reports in question refer to a proposition of group 2 (historical). Fox says: "In reading this sentence a distinct pause was made after the words 'mechanical inventions.'"[31] An emphasis or a pause probably is favorable to the production of imagery; but this does not seem to be analogous to a conflict or struggle. On the other hand, it is quite possible for a pause to arouse a mental image which does not accord with the further meaning of the statement; in which case it certainly would not be used as are the images which appear as the result of a conflict. For example, in reporting on this same sentence, one of our *O*'s (H) said: "'Mechanical inventions' gave rise to auditory imagery meaning 'Edison' and visual imagery in greys of different brightness that meant 'transportation' or 'industrial activity.' I seemed to have settled the thing until the word 'England' came. With this the visual image dropped out. Then an auditory image of the word 'England' that meant to assure myself of what you said. Then followed a bit of visual imagery in greys that referred to conditions in England and meant the translation of the previous meaning from this country to England." 3. "A strong image may obstruct the attempt to understand."[32] Fox does not define 'strong,' so that we do not know whether he refers to clearness, or to details, or to stability and duration. His illustrations of "certain cases" in which the image obstructs understanding do not, however, seem conclusive. In any case they do not prove, as perhaps they are not intended to prove, that thought may be imageless. One of Fox's subjects reports: "When I tried to realise the significance of the statement it was twice obstructed; at first by the picture of my old history room at school, then by my history book open at the page on feudalism."[33] This seems to be an example of our anchoring imagery. It is true, of course, that attention to these images for themselves would involve a shift in attitude, and that they would thus prove an obstruction to realisation of meaning. 4. In considering the propositions of the third group, Fox states that "prompt and thorough understanding coincides with the absence of images."[34] This is evidenced by the reports of three subjects of whom Fox says: "Three subjects obtained what may be described as an associative image, namely an image not directly called up by the lines but evoked by association with their meaning. In these cases the image was that of a book on education in which a similar doctrine to that expressed in the lines was discussed. Now those who had these images must have realised the meaning before the images came, since such images depend on understanding the meaning."[35] Hence he concludes that these three subjects realised the meaning of the statement without the aid of images. We have not the introspective reports for reference, and it may be that these present evidence that the images were associative. From what Fox tells us, however, it does not seem necessarily true that "those who had these images must have realised the meaning before the images came, since such images depend on understanding the meaning." It seems quite possible that the image of "a book on education" might have carried the meaning of the lines instead of being dependent on them. In conclusion Fox states: "The experiments show that any delay or conflict in consciousness is a favorable condition for arousing a relevant mental image, that is, one that will in some way tend to help towards a cessation of the conflict. All the other conditions which we have found to be suitable for stimulating the production of mental images are reducible to this general formula. . . . The experiments also show directly that the contrary set of conditions are (*sic*) unfavorable to the production of images. Thorough or immediate understanding, an easily grasped conception, ready assent to a proposition, straightforward or unimpeded reasoning, are all cases in which, as a general rule, images play no part. Further, concentration of thought on meaning is unfavorable to the stimulation of mental imagery, but this cannot be brought easily under the above general formula."[36] We agree with Fox's statement with respect to the conditions favorable for arousing mental imagery. We cannot, however, fully accept his statement of conditions unfavorable to the stimulation of imagery. "Thorough or immediate understanding." as in cases where the propositions come to the *O* labeled "You've accepted me before," may be explained by the operation of brain-habit, so that no imagery would be needed. In "straightforward or unimpeded reasoning" it seems probable that, the attention being on meaning, the processes might be overlooked. Of the 60 thought-processes of our 5 *O*'s, only 3 or 5% occurred without imagery. We are, accordingly, inclined to believe that, in some at least of the situations mentioned by Fox as unfavorable to the production of imagery, the imaginal content was overlooked, so quick is the process of thought and so completely is the attention of the subject likely to be concentrated on meaning. We have a parallel case in the neglect of after-images and double images in everyday experiences when other things are in the focus of attention. We should, therefore, agree with Fox's last statement (if we may change the word 'stimulation' to 'report') that "concentration of thought on meaning is unfavorable to the *report* of mental imagery."

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[24] *Ibid.*, 424.
[25] *Ibid.* 424.
[26] *Ibid.*, 424.
[27] *Ibid.*, 424.
[28] *Ibid.*, 421.
[29] *Ibid.*, 426.
[30] *Ibid.*, 427.
[31] *Ibid.*, 426.

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[32] *Ibid.*, 427.
[33] *Ibid.*, 427.
[34] *Ibid.*, 429.
[35] *Ibid.*, 429.

The general results of the experiments in which we used the second set of instructions and material similar to the statements of Fox may be briefly mentioned. With the exception of one *O*, D, these instructions were interpreted as calling for a report both of meaning and of process. This *O* persisted in his statement that he was never "aware of the realisation" and so could not report on it. The reports with these instructions were much less full, and the use of the expression "of what the realisation consists" seemed to throw the emphasis on meaning rather than on process. One *O* (as we have reported above) said: "To me realisation means a sort of acceptance or approval of the meaning." If Fox's subjects interpreted his instructions in this way, it is evident that they were set to report meaning and not process; so that images, although present, may easily have been overlooked.

Two things may be learned from the repetition of this experiment: (1) the necessity of phrasing instructions carefully and accurately; (2) the need for as flexible and unbiased an interpretation of results as possible.

EXPERIMENT B. 1. LIKENESSES AND DIFFERENCES

Our problem remained the same as in Experiment A. In considering the results of this experiment we discussed some of the difficulties encountered in arranging material and determining procedure. In Experiment B we tried to overcome some of these difficulties. Our material consisted of paired words whose relations to each other with respect to likeness, difference, etc., the *O*'s were to give. It may be remembered that we found that one of the sources of error in the material used in Experiment A was the lengthy statement of the problem. We hoped that the series of paired words would eliminate this difficulty; the question asked in the instructons would finally be present only as a set, and the statement of the problem would be reduced to two words. It seemed also that this type of material might be a halfway house between the "wordy" problem and the single word-stimulus.

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[36] *Ibid.*, 430 ff.

The *O* was seated with his back to *E*, and was instructed to keep his eyes closed during the reading of the paired words and the determination of the answer. The instructions were as follows: "I shall present to you a series of paired words and I want you to tell me in what respect the members of each pair differ from each other. After you have done this, please describe as well as you can the experiences upon which your differentiation was based." Three series of 15 paired words each were used. The two words were read to the *O;* the stop-watch was started as the last word was spoken by *E* and stopped as soon as *O* gave his answer. The following are typical of the paired words used; cut-scratch, education-culture, possible-practicable, Dickens-Scott. There were five *O*'s. Two of these, Z and H, had worked in Experiment A. The others were Miss R. Stutsman (St), and Messrs. P. Cavanaugh (C) and E. Tolman (T). C had had some training in observation, but St and T were untrained. The experiment covered a period of two weeks. At the end of this time the *O*'s were asked to give likenesses instead of differences, and the instructions were changed accordingly. Thirty pairs of words, such as memory-imagination, water-air, nymph-mermaid, were used.

Uses	Number of Cases				
	a	*b*	*c*	*d*	*e*
1. Anchor					
(*a*) Illustrative	57	93	6	12	7
(*b*) Non-illustrative	18	24		2	
(*c*) Combination of a and b	12	8			1
2. Subject-matter of Problem					
(*a*) Changing	42	90	12	16	12
(*b*) Fixed	6	3		6	
(*c*) Combination of a and b		1			
3. Regulation of Problem					
(*a*) Formulation	9	8			2
(*b*) Anticipation					
(*c*) Criticism	2	3			
4. Means of Escape from Problem					
5. Illustration of the Answer	6	3			2
(*a*) Reinforcement	3				

a, *b*, etc., at the head of the Table, refer to the relationships listed on p. 213.

The experiment covered a period of a week. In order to eliminate certain difficulties, which we shall discuss later, we again changed the material and instructions. We still used paired words, but words which stood in various relations to each other. The instructions were: "I shall read you 2 words. You are to tell in what respect (*a*) they are like each other, (*b*) they differ from each other, (*c*) one is dependent upon the other, (*d*) one is convertible into the other, (*e*) one affects the other. After you have given your answer, please describe the experiences upon which the answer was based." Before pronouncing the 2 words, *E* indicated the relation between them to be given by *O*. The following are typical of the paired words used in this series: hypothesis-law (convertible), democracy-education (affect), capital-labor (dependent). The experiment lasted 2 weeks and with one exception (H) the *O*'s were the same as before.

We give first a classification of the imagery used in determining the various relations between the paired words.

The types of imaginal experience upon which the differentiations were based are:

1. Two images which may be present (*a*) simultaneously, (*b*) successively, (*c*) successively with a recurrence of the first image. Number of instances=104.

Stimulus: egg-stone. Report: "One is breakable, the other breaks. There was a scrappy bit of visual imagery, vague. It carried the meaning of falling and breaking. Over against that was a kinaesthetic image in the throat. It carried the meaning of hard, something that wouldn't break" (H).

2. Two images may be present and the basis for differentiation be carried by the second. Number of instances=13.

Stimulus: stumble-fall. Report: "To fall is to go clear to the ground. A visual image of myself stumbling on a brick walk. I was leaning forward. There was a weak kinaesthetic image in my chest and shoulders and a motor image in my toe. Then the word 'fall' came. With this there came, in just a flash, a visual image of a man lying on the ground. Then the difference came; to fall is to go clear to the ground" (Z).

3. One or two images may be present, the differentiation being made in verbal-motor imagery. Number of instances=27.

Stimulus: cook-fry. Report: "You boil in water, but you fry in grease. I had a visual image of something frying in a skillet. With 'cook' there was a visual image of a pot. I was trying to get at something in the pan and pot to differentiate them. This was in verbal-motor imagery: 'You don't fry in water'" (Z).

4. There may be a visual schema, accompanied or unaccompanied by anchoring images. Number of instances=15.

Stimulus: radical-progressive. Report: "A radical person has less sense than a progressive person. A visual image in dark grey of the two words written out on a neutral grey background. 'Radical' was above 'progressive.' At that point there was no difference. Then 'radical' moved rapidly to the right. The right meant 'ahead of,' 'away from,' or 'his going too far ahead to be sensible.' All this touched the answer off" (C).

5. There may be two modalities carrying the meaning of the first stimulus-word, the differentiation being based on the absence of one of the modalities in the imagery carrying the meaning of the second stimulus-word. Number of instances=7.

Stimulus: fry-cook. Report: "The difference is auditory. With 'fry' I had a vague visual image which meant a frying pan with meat on a gas-plate. This was very scrappy, and was accompanied by a clearer auditory image of equal intensity. With 'cook' there was a vague visual image of a pot. I saw nothing cooking in it, though the meaning was there. The auditory image was lacking here, and this lack forced the first difference" (H).

6. There may be verbal-motor imagery accompanied by illustrative imagery. Number of instances=3.

Stimulus: powerless-weak. Report: "You may have a little power and be weak, but if you have no power you can't do anything. I was at sea for a while. Then I thought (verbal-motor imagery) 'They're absolutely synonymous, but she asked for a difference.' Then I thought of an engine, an automobile engine, powerless; then of an engine running very weakly (visual imagery) and then came in the concrete experience which was the basis of differentiation" (Z). There were reported 3 instances in which the differentiations were based on different feelings (pleasantness and unpleasantness) set up by the two stimulus words; two instances in which the differentiation was based on two different sensations; and two in which it was based on a difference between image and sensation.

The types of imaginal experiences upon which the likenesses were based are:

1. Two images may be present (*a*) simultaneously, (*b*) successively, the likeness being determined by the presence of one or more similar qualities in the two images, or by full similarity, or by eye-movement from the one image to the other; or the answer may be set off immediately.[37] Number of instances=42.

2. There may be two images, the first of which carries the meaning of a superordinate class. Number of instances=16.

Stimulus: purple-orange. Report: "Both are colors. After the stimulus 'purple' I had a visual image of a patch of dark purple. With 'orange' the verbal-motor image 'color' came. The purple image meant 'color,' so that when 'orange' came I could give 'color' at once" (Z).[38]

3. There may be a visual schema. Number of instances=3.

4. There may be anchoring imagery together with verbal-motor imagery. Number of instances=12.

5. Illustrative imagery may follow or accompany a response given immediately. Number of instances=9.

Stimulus: apple-ball. Report: "Both are round. As soon as *E* had said 'apple-ball,' 'round' came. After I had made my decision visual images of 'apple' and 'ball' came" (Z).

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[37] Illustrations will be given only in cases in which the types of experience differ from those reported for the differentiations.

[38] Cf. the "übergeordneten Begriff" of A. Messer, Experimentell-psychologische Untersuchungen über das Denken, *Arch. f. d. ges. Psych.*, 8, 1906, 78.

The analysis of the experiences upon which the determinations of the other kinds of relations were based showed no new types of imaginal experiences.

Conclusions

I. We have obtained in this experiment further evidence of the importance of imagery. Although we reduced our problems to lowest terms, imagery was needed. Almost as important as the imagery is the part played by the "set" produced by the instructions. It will doubtless have been observed in the giving of both likenesses and differences that the presence of two images carrying the meanings of the stimulus-words was not enough in itself to touch off the answer. The determination to report a difference or a likeness was the essential thing. This is especially noticeable in the case of the differentiations.

II. Our material and procedure in this experiment were arranged with a view to eliminating some of the difficulties found in Experiment A. (1) *Material.* The shortened statement of the problem avoided the setting up of imagery present in the reading of a longer problem, and focused the attention on the question itself. This is clearly advantageous. On the other hand, unless the stimulus-words are chosen with care, much less thought is required than in the case of the problems. The acquisition of a set for a superordinate concept, as was the case with the likenesses, made *O*'s task considerably easier. The simplicity or perhaps the uniformity of presentation of the material seemed to make possible a fixed kind of reaction to it. The generality of the instructions permitted superficial answers and, accordingly, less thought on the part of *O*. The demand for a more fundamental likeness or difference meant the suppression of the first answer that came and the search for a second; all of which would increase the task of report. (2) *The task of the experimenter.* In the interpretation of results *E*'s work was much easier with the use of this material. The reports of the *O*'s were briefer and seemed to be more accurate and complete. The *O*'s were often able to say with reference to a specific process: "It was this that carried the meaning of likeness," thus marking it off from the other processes reported. If the *O*'s are to be trusted, *E*'s burden is appreciably lightened. (3) *The influence of the experimental attitude upon the O's.* We have already mentioned the fact that *O*, knowing he has to report his experiences, may be set to think in imaginal terms to a greater extent than is usual with him. Indeed, one of our

O's, after having given a full report of the imagery used in making a differentiation, said: "If I were asked on the street to give the difference between "old" and "obsolete," I think that the thing would go off in verbal terms." This remark led us to undertake our next experiment.

EXPERIMENT B. 2. EXPERIMENT UNDER NON-LABORATORY CONDITIONS

We wished to find to what extent imagery is present as a basis for answering questions when the questions are not a part of a laboratory exercise. In other words, we wanted to catch our *O*'s without the experimental attitude upon them, and without the knowledge that they would be asked to give "the experiences upon which the answer was based." Outside of observation-hours questions such as would arise naturally in the course of a conversation were asked casually. When the "*O*" had as casually answered, we asked him if there were any imagery present as the basis of his answer. We have 29 reports from 5 of our regular *O*'s, Z, H, B, D, and S; and 4 reports from persons having no training in psychology. These reports are, of course, necessarily incomplete; but with only 3 exceptions there was reported imagery of some kind upon which the answer was based. In 2 of the 3 exceptions kinaesthesis was reported; and in the case of the remaining exception the subject stated that the answer came automatically. We give an illustration of the procedure used. The "*E*" and the "*O*" were talking about Stout and his books. *O* was asked what Stout had written, and replied: "He wrote a couple of books that I know of and he stands for the theory of conation." *E* then asked: "Did you have any imagery when you answered my question?" The answer follows: "Yes. I had a visual image of the titles of the books and of their covers. I also saw printed, as if an excerpt from a book, the word 'conative.'" This group of experiments shows that imagery is of frequent occurrence in everyday thinking. The high percentage of cases in which imagery was reported is undoubtedly due to the fact that 5 of the 7 persons questioned were trained *O*'s. The demand for a report under these conditions implies a quick shift from a logical to a psychological attitude, of which only trained *O*'s are capable. It is clear, then, that a good deal of imagery is present in thinking, and that its presence is not solely the result of laboratory conditions.

EXPERIMENT C

Our experiments in thinking gave us no instances of irrelevant material. Though we had been attacking our problem on the side of relevancy, we had expected to get at irrelevancy indirectly. It seemed best now to concern ourselves directly with irrelevancy. We began this section of our experimental work with experiments at the perceptive level, resembling everyday experiences. Each one of the situations in these experiments included an irrelevant factor. We wished to obtain a description of consciousness at the instant when the irrelevant factor appeared in the situation.

There were 3 parts of the experiment. (1) We set the *O*'s simple tasks into each one of which an irrelevant factor was introduced. At the appearance of the irrelevancy we interrupted the *O* in his task, and asked him to describe his experiences at the moment of interruption. There were 4 tasks of this sort: (*a*) the sorting, according to 3 presented samples, of buttons drawn from a bag in which was one button unlike any of the samples; (*b*) the sorting, according to the arrangement of holes punched in them, of cards taken from a piled pack, one of which was different from all the others; (*c*) the arranging, in order of preference, of samples of cloth pasted on paper, among which there was an oblong of paper with no cloth on it; (*d*) the presentation, for brief study, of a picture about which were later asked questions one of which had nothing to do with the subject-matter of the picture. The instructions were: "Here are 3 buttons, and others of the same sort are mixed up in the bag. You are to put your hand in the bag and, taking out one at a time, arrange them in piles according to the samples shown. When I say 'Now,' you are to drop the task if it is still incomplete and describe your experiences at the moment of interruption." The first part of the instructions was changed to suit the nature of the task, but the latter part remained the same for all 4 tasks. The time was taken with a stop-watch chiefly in order to hold the *O* strictly to his task.

(2) The material for the second part of the experiment was a paragraph the latter part of which was entirely irrelevant to the beginning. The irrelevant part was begun on the last line of a sheet of paper so that the *O* was obliged to turn to the second sheet to continue his reading. This arrangement had the double advantage of preventing the reader from glancing ahead to the irrelevant part, and of letting *E* know when to interrupt. The instructions were: "I shall give you a paragraph which I want you to read carefully enough to give its contents after reading. When I say 'Now,' you are to drop the task if it is still incomplete, and describe your experiences at the moment of interruption." (3) The material for the third part of the experiment consisted of 4 paragraphs which contained such errors as: repetitions, omissions, misspellings, transpositions, and omission of punctuation. The instructions were: "I shall give you a short paragraph which you are to read carefully. After you have finished your reading, you are to give a summary of the paragraph." After the *O* had read and reported on all 4 of the paragraphs, he was asked if he had noted any errors of form, and if so to describe his experiences when he noted them. Only one of the foregoing experiments took place in any one observation-hour, and it either preceded or followed other experimental work. There were 5 *O*'s, 3 of whom, H, S, and Z, had observed in Experiments A and B; and 2 of whom, B and D, had observed only in the repetition of Fox's experiment.

Sorting Problems

Three of the tasks (1 (*a*), (*b*), (*c*)) were of the same general nature, *i.e.*, sorting problems. The reports show the presence of 4 stages in the realisation of irrelevancy:

1. Perception (usually tactual), accompanied by immediate judgment of difference and supplemented by imagery;
2. Visual perception of difference;

3. Feeling component:
   - (*a*) Astonishment
   - (*b*) Surprise and wonder
   - (*c*) Indecision

   Analyzable into kinaesthetic sensations, affective processes, and imagery
4. Reaction:
   - (*a*) Immediate (directed by determining tendency);
   - (*b*) Mediate (verbal-motor imagery).

We give a report of the experiences described in the performance of one of the tasks. "First a cutaneous perception which was different from the preceding one; the pressure was much heavier. There was a brief snatch of visual imagery which went with this cutaneous pressure sensation and carried the meaning of the type of button and the fact that it was different. I think I verbalised this as: 'I wonder what this is.' Added to this was the perception of the visual black [the odd button was black], and then without consciously intending to do anything, I threw it outside. The kinaesthetic process and the perception of throwing brought the meaning of rejection" (D). In the case of some of the other *O*'s the feeling-part of the experience was more marked. It was always present, and seems to be characteristic of the appearance of the irrelevant component of the situation.

The sorting of cards, as an experience containing an irrelevancy, was a failure. The card which differed in the arrangement of its punched holes from the other cards was in no case perceived as different, the difference apparently being so slight as to be overlooked. The reports on this situation are, however, valuable; they show the difference between a situation containing an irrelevant factor and one containing only relevant factors. We find reports of stage 2, the visual perception of difference, and of stage 4, the reaction. In this perception there was no tactual element, for the 'feel,' in general, of all the cards was the same. The significant thing is the absence of the feeling-component. Of importance, also, is the fact that in all cases the reaction was immediate, never mediate.

Reading Problem: Irrelevant Meanings

In their reports on the second part of the experiment, in which the material was the single paragraph, all *O*'s noted the "meaninglessness" of the irrelevant part. The absence of meaning was accompanied by feelings of strangeness, confusion, puzzlement, etc. The meaning of irrelevancy was carried by pressure sensations (staring at the page), muscular tension (an attempt to hold the meaning together), and verbal-motor imagery, which usually expressed an attempt to force a relationship to the preceding part of the paragraph.

Reading Problem: Typographical Errors

In the third part of the experiment we set our *O*'s too difficult a task. They could not give a report of processes, even when they had noted errors, so long after the observation had taken place. The difficulty was further increased by the set for relevant meanings. It should be said, however, that we were aware of the difficulty of this task; we had wished to arrange an everyday situation, like the reading of a newspaper, in which the irrelevancies should be commonplace. One *O* noticed no errors and said reproachfully when asked to report: "I didn't notice any errors; I wasn't set for proof-reading" (Z). The 4 other *O*'s remembered and reported mistakes in the paragraphs, but 2 of them could give no report of process because they had not taken the attitude for report. The third *O,* H, reported that he ignored the errors as he read, "for he was reading for thought rather than for grammar." He mentioned, however, a tendency to read aright in auditory imagery the mistakes, and also some verbal-motor imagery carrying the meaning of errors. The fourth *O,* B, reported verbal-motor imagery carrying the meaning of annoyance at writing so carelessly done by *E*.

Conclusions

We have seen that a marked irrelevancy is characterized by feeling accompanying the inhibition set up; and that where the irrelevancy is less marked, it tends to be overlooked because of the set for relevant meanings.

EXPERIMENT D. 1. PICTURES

In Experiment C we began to study irrelevancy directly. We arranged situations containing irrelevancies on the perceptive level, and we secured an analysis of the situations from our *O*'s. From these analyses we found that when an irrelevancy appeared in a situation it was accompanied by "feelings." We wished now to find out whether such feelings might be the criteria of the entrance of irrelevant imagery into an imaginal situation. To this end we had to arrange a situation in which there should be some imagery irrelevant to it; as, for example, the black button was irrelevant to the other buttons in the bag. We wanted, with our

O's set for imagery, to have a complex background of imagery upon which they could draw. To secure this we used 3 kinds of material and procedure.

1. We presented to *O* a series of 12 pictures, mainly narrative, brightly colored, and varying in size from 6 by 8 inches to 12 by 16 inches. The instructions were: "I shall show you a picture for a short period. Please observe it carefully. At the end of this period you will be allowed two minutes in which to write a description of the picture, giving its title." The *O*'s were seated in front of a grey screen upon which was mounted one of the pictures. The picture was at first covered by a curtain, which was raised just after the "ready" signal and let fall after an exposure of 15 seconds. This procedure took one observation-hour. At the beginning of the next observation-period we gave the *O* the following instructions: "I shall name to you one of the pictures that you learned last time. When you hear the name, I want you to recall the picture as vividly as possible. Three seconds after I have named the picture I shall read you a simple problem which you are to solve as quickly as possible. Say 'Yes' when you have solved it, and then begin to report as fully as you can the course of your experiences during the experiment." After each one of the pictures had been presented once for recall, they were presented again in a different order, and followed by different problems.

2. The material for the second part of the experiment was put in the form of a completion-test. We took from various books descriptive paragraphs having the following titles: "The Mountains of the Desert," "Rules of Hunting among the Greenlanders," "Daybreak" (poetry), "A Japanese Garden," "The Winter Dwellings of the Esquimaux," and "A Simple Chronoscope." Several words, including all parts of speech, were omitted from these paragraphs. The *O* was given the following instructions: "I shall give you a paragraph in which some of the words are missing. You are to fill in the blank spaces, each of which indicates an omitted word. You will be given a certain length of time in which to do this. At the end of this time I shall say 'Now' and read you a problem which you are to solve. After you have given your answer, report all of your experiences from the reading of the problem to the giving of the answer." No fixed period of time was allowed for filling in the words of the paragraph. The *O* was interrupted in his task when he was seen to be near the end of the paragraph.

3. The material for this part of the experiment consisted of 3 separate words. For the most part the words all referred to a single situation as crowd—touchdown—cheers; though there were a few cases in which the 3 words might set up different trains of thought, as for example turkey—star—electric. The instructions show the procedure. "I shall read you 3 words. Give yourself passively to any imagery that comes. After 15 seconds, I shall read you a problem which you are to solve and report on in the regular way." *O* was seated with his back to *E* and with his eyes closed. There were in the three parts of Experiment D 5 *O*'s, all of whom had served in the other experiments.

In the first part of the experiment we endeavored to supply the *O* with a background of imagery upon which should be impinged a problem to be solved. We desired, of course, to know what happened to this imagery when the problem came. We shall refer to the imagery called up by the *O* in this part of the experiment as the picture-imagery, since it was determined by the previous presentation of pictures. An analysis of the reports of the 5 *O*'s shows that the picture-imagery may behave in at least 3 different ways.

(1) It may drop out at the beginning of the reading of the problem, either suddenly or gradually.

Report: "The first imagery (picture-imagery) came quite quickly, so that when I repeated the name of the picture, it was there almost immediately. In quality it was greyish and reddish. It lacked a background. Then began the reading of the problem, and there was with it rather good visual imagery of trees. Somewhere here the first imagery went out" (D).

(2) It may remain during the reading of the problem, in which case the problem is (*a*) not sensorily clear, (*b*) not cognitively clear, (*c*) neither sensorily nor cognitively clear, and the picture-imagery loses in clearness, detail, meaningfulness; or there is a fluctuation in clearness between the picture-imagery and the problem, in which both seem equally insistent in turn.

Report: "The auditory stimulus (name of picture) carried the meaning of familiarity and with this there was a visual image of the bird sitting on a post out in Oregon where I knew the bird. Then quickly came the image of the picture as seen. The image was intense and rich in detail. Then the problem came. It was a sensation without meaning. The association with the bird persisted; its song came in auditory imagery and a visual image that meant 'bird' persisted all the time that the words of the problem were coming. It was there until nearly the end of the reading. During the last part of the reading the attention was on the auditory sensation more than on the meaning. Then an auditory image that meant the whole problem repeated twice. By this time the 'bird' was pushed aside and the attention was on the problem. The meaning of the problem became clear cognitively" (H).

(3) It may remain or may recur during the reading of the problem and be illustrative of it.

Report: "The auditory sensation of 'The Line Up' (the name of the picture) brought a visual image that meant the picture. It was rich in color-meaning. At first the auditory sensations were very obscure, not cognised. At the third word there was touched off a determination to solve the problem, and the visual imagery dropped right out. With the word 'cloth' (word of the problem) there was a recurrence of a bit of the picture-imagery meaning the red shawl on the man's back. From there on there was a determination to listen to the problem" (H).

What happens to the picture-imagery is obvious. Where it is irrelevant, it drops out; where it can be used,—that is, where it becomes relevant,—it remains. It is true that it may not drop out all at once; but in that case it interferes with the grasping of the statement of the problem, as well as being interfered with in its turn by the problem. There is a struggle between the two for the field of attention, and now the one and now the other is clear. There is, moreover, no awareness of irrelevancy so far as the picture-imagery is concerned. The irrelevant factor here is the auditory perception of the problem, and we find its appearance characterised in much the same way as was the appearance of the black button. For example, one *O* reports: "I couldn't shake the picture out of my mind (when the problem came). There was muscular tension and internal speech while I was trying to get my bearings. There was strain around the eyes, discomfort, and unpleasantness" (B). This is evidently our feeling-component of Experiment C; but here, as there, it is characteristic of a perception, not of an image. We have another kind of situation when the imagery remains or recurs during the reading of the problem. In this case the shift from the picture-imagery-awareness to the problem-awareness has been made. In two of the three reported instances of this sort the picture-imagery has entirely faded out, and recurs only to carry the meaning, in an illustrative fashion, of part of the problem. It is to be noted that only that part of the picture-imagery which is relevant to the problem-meaning recurs; the whole of the image does not come back. We have thus succeeded in giving our *O*'s a background of imagery, and they draw upon this where they can; for the rest, it disappears. In the third instance included under this heading, the picture-imagery remained during the reading of the problem. It began, however, to lose in detail, and now one part and then another of the visual image was clear. The problem was lacking in cognitive clearness. The first word of the problem to be comprehended was the word 'trees' which the *O* said "persisted" and was related to a tree in the visual image. What we have here is a reinforcement of the problem-meaning by a part of the picture-imagery. After this the picture-imagery dropped out and the problem became clear. There remain for consideration 3 cases in which the problem-imagery was superposed on the picture-imagery. These we shall discuss along with the results of the third part of the experiment.

Experiment D. 2. Completion-Tests

We expected in this part of the experiment, as in the preceding part, that the *O*'s might be "revelling in imagery" when the problem came; and we hoped that they might have recourse to imagery in their attempts to fill in the blanks in the completion-test. We interrupted them just before the paragraph was completed, so that any imagery present should have been at its richest. An analysis of the reports shows the experiences of the *O*'s when the problem came and the effect of the previous task on the problem.

1. There may be a complete and immediate shift in attitude from the completion-test to the problem.

Report: "In the paragraph the first blank was a nuisance to me. I was tired of the test and in a sort of careless attitude so that when the problem came I was glad; and the test dropped out entirely" (Z).

2. There may be between the test-attitude and the problem-solving-attitude a state characterised by the *O*'s as "a period of cognitive blankness" or as "a chaotic consciousness."

Report: "Consciousness was chaos at first. There was a series of auditory sensations having no meanings" (S).

3. The test-attitude, carried by (*a*) visual imagery, (*b*) kinaesthetic imagery or kinaesthetic sensation, renders the problem sensorily unclear, cognitively unclear, or both.

Report: "Visual imagery (from the completion-test) was present when you started reading the problem. As the problem was read I turned sharply toward you, meaning that I was going to pay attention. The first part of the statement came merely as auditory sensation and without specific meaning, though with the meaning 'here is the problem.' There was a reference back to the paragraph carried in kinaesthetic sensation; a tendency to turn the head back. Then there was verbal-motor imagery of parts of the problem and some visual imagery" (D).

4. The imagery used in the completion-test may hang over or recur during the reading of the problem, in which case the problem is sensorily and cognitively unclear or is not heard. The results of this experiment show that the conditions set are not conducive to the "carrying-over" of imagery from one situation to another; the break between the two situations is too great. What we really have here is a study of shift in attitude. There remains for consideration one case in which the imagery used in the completion-test remained during the solution of the problem. This we shall discuss in connection with the results of the third part of the experiment.

Just as in the first part of the experiment the reading of the problem was characterised by a feeling-component, so was it here. The *O*'s were "annoyed," "irritated," "bothered," when the problem was read and the set task interrupted. We are again reminded of the experiments in perception.

EXPERIMENT D. 3. THREE-WORD IMAGERY

In the third part of the experiment the procedure was more succesful so far as the setting up of a background of imagery is concerned. We have a total of 324 reports from the *O*'s. These show that the imagery set up by the 3 words (to be referred to as the 3-word imagery) may behave as follows:

1. The imagery may drop out before the problem comes, or with the reading of the first word. Number of instances=79.

Stimulus: bells—horses—fire. Report: "I was trying to hear the bells ring when the problem came. This was carried by strain in the ears. This dropped out at once and the problem was the only thing in mind" (B).

2. It may remain during the reading of the first few words of the problem; or during the whole of the reading of the problem, in which case the problem may be rendered sensorily or cognitively unclear. Number of instances=79.

Stimulus: elephant—man—sawdust. Report: "The 3-word imagery was good. When the problem began I did not attend, for I was engrossed with a visual image of the elephant and his trainer and the sawdust on the floor. Then I 'picked up' a memory after-image of the first part of the question and the visual image dropped out" (Z).

3. It may recur or it may fluctuate with (*a*) the reading of the problem or (*b*) the solution of the problem. Number of instances=46.

Stimulus: June—stars—perfume. Report: "The 3 words set up visual imagery and also some auditory and olfactory imagery. The problem at first was obscure. The imagery persisted clearer than the sensation. Then the imagery dropped out and the sensation became clear cognitively. The visual imagery came back very, very briefly. There was no connection of meaning between the visual imagery and the auditory sensation. Then the visual imagery dropped out, meaning that I could get no aid in the problem from the imagery" (H).

4. It may remain or recur (*a*) as a whole, (*b*) in part, (*c*) changed in form or meaning or both, to serve as all or part of the anchoring imagery of the problem. Number of instances=50.

Stimulus: red—fragrant—alive. Report: "'Red' and 'fragrant' called up a visual image of a red rose on a small bush. Then in verbal-motor imagery, 'alive, alive; that's alive.' This imagery remained until *E* set the second situation (Problem: A man wanted to catch a kitten but the kitten ran up a tall tree which no person could climb. How could he get the kitten without hurting it?). The reading of the problem was accompanied by a shift in imagery. The thorns on the tree became large and looked wicked, meaning 'No person can climb it.' They also meant a very large bush like a tree. The rose was still on top and the kitten was near the rose" (Z).

5. It may remain or recur (*a*) as a whole, (*b*) in part, (*c*) changed in form or in meaning or both, to serve as the material for solving the problem. Number of instances=31.

Stimulus: prison—stripes—chains. Report: "The 3-word imagery was quite good and was of a prisoner in stripes with ball and chains on his leg. Then came the problem (Problem: How do you play 'Snap the Whip?'). There was verbal-motor repetition of the problem which helped me to remember it. The visual imagery was modified and now included a lake and skaters. The prisoner stood still in the middle of the lake as if the place where he had been in the 3-word imagery was now the ice. The 'ball' became a man and the 'chain' a string of skaters. Then the string of skaters moved and the prisoner stood there" (D).

6. It may drop out as soon as the problem comes, but may recur after the answer to the problem has been given, in which case it may

be (*a*) illustrative of the problem, (*b*) non-illustrative of the problem. Number of instances=6.

Stimulus: cat—yarn—basket. Report: "The 3-word imagery came. Then the problem was read (Problem: Suppose that you have been sitting in one position for a long time and your foot goes to sleep. What is the thing to do?). I hardly know what happened in the solution. I had a visual image which meant my foot going to sleep. This was an irregular patch with bright zigzag lines. Then I said the answer and back came the 3-word imagery" (D).

7. It may serve as a background into which is projected a person or the *O* himself who solves the problem. The background may last throughout the problem-solving or drop out before the problem has been solved. Number of instances=12.

Stimulus: dew—garden—silence. Report: "A visual image of a nice flower garden in the early morning before sun-up. I was in it. When the question came that visual image was still there vividly and I was solving the problem, projected to that garden (Problem: Why is an octave so named?). I was in the garden thinking: 'octave, octagon' in verbal-motor imagery. Then came the verbal-motor 'eight' and I said, 'do, re, mi, fa,' and gave my answer. The 3-word imagery was there until the end, but I was the important part in it" (Z).

8. It may recur to serve as a means of escape from solving the problem. Number of instances=2.

Stimulus: doctor—lawyer—minister. Report: "The 3-word imagery was present sharply and the problem came and was carried partly by visual imagery which was in the lower right part of the field (Problem: If 3 oranges cost one fifth of a dollar, how much will 2 cost?). The rest of the problem was carried by verbal-motor imagery with here and there a visual tag, but the solution seemed to be in terms of verbal-motor imagery. The 3-word imagery recurred at least 3 or 4 times. It seemed to be impossible to solve the problem. Later I made a specific effort. Once I said: 'Report that you can't do it.' Then by forcing myself I got the answer. Near the end there was a recurrence of the 3-word imagery. It was very, very scrappy" (D).

9. It may persist either through part of the solution or until the end of the solution of the problem, in which case the problem-imagery is (*a*) in front of it, (*b*) at its left or right. Number of instances=10.

Stimulus: infant—manger—wise men. Report: "There was good 3-word imagery. It stayed, so that there was no break when the problem came (Problem: How was Achilles made invulnerable?). When 'Achilles' was mentioned he seemed to be at the left of the other image; larger and clearer. He was running. Then I started to give the answer" (S).

10. It may remain throughout the solution until the answer has been given, in which case the *O* (*a*) can give no report, or at least no certain report, of the experiences leading to the answer, (*b*) states that the answer came immediately or automatically. Number of instances=5.

Stimulus: chimes—snow—carols. Report: "The 3-word imagery was very good. Then came the reading of the problem and its meaning was carried in verbal-motor imagery (Problem: Where do corks for bottles come from?). The 3-word imagery was still there and almost as good in detail as before. There may have been a little tag of visual imagery which carried part of the meaning for my answer; I am not sure. About here I found myself answering. I didn't know I was going to answer until I heard myself" (D).

We are already familiar with some of the foregoing headings, so that a detailed discussion of them will not be necessary. The cases in which the 3-word imagery dropped out before the problem came included those in which the words had not evoked any imagery at all, and those in which the imagery was poor in detail and in clearness. Frequently, too, the set to solve the problem caused a clean break when the problem came, although the 3-word imagery might have been good. The cases in which the 3-word imagery remains or recurs during the reading of the problem or its solution are illustrative of the conflict between the *Aufgabe* and the 3-word imagery. The determination to solve the problem may overcome the 3-word imagery before the solution is actually entered upon or, as in the other possibility mentioned, the alternation between the problem-imagery and the 3-word imagery may continue during the solution of the problem. We find, too, that there may be a recurrence of the 3-word imagery after the answer has been given, that is, after the determination to solve the problem has been satisfied and the *O* is free. These cases are evidences that we succeeded in giving our *O*'s a background of imagery. A suggestion as to why the 3-word imagery dropped out is found in the report of an *O* who said: "The visual imagery carrying the 3-word situation came back very briefly during the reading; then it dropped out, meaning that I could get no aid in the problem from the imagery" (H). This brings us to the cases in which the *O* did secure aid from the 3-word imagery. To just what extent the imagery was used in the anchoring and the solution of the problem the illustrations will have shown. That this is the economical mode of behavior for the 3-word imagery is obvious. The *O*'s had a background of imagery, and upon this they drew in their solving of the problems. Frequently, only that part of the 3-word imagery which was relevant remained, the irrelevant part being lost; a further proof of our thesis that irrelevant imagery does not exist. We furnished our *O*'s with imagery logically irrelevant to a situation in which they might need imagery. If no aid could be got from the 3-word imagery or, in other words, if it was totally irrelevant, it dropped out. If, however, it could be used, though only in part, it so far remained. The cases in which the imagery became changed to carry the meaning are of interest. They show again the tendency to economy and, as well, the importance of relevancy. The cases in which the imagery serves as an escape from solving the problem are like those met with in Experiment A. The cases in which the 3-word imagery remains as a background during the solution of the problem are further illustrative of use pushed to its limit. The *O* has been revelling in a field of imagery from which he need not wholly withdraw; that is, the 3-word imagery remains relevant to the new situation.

Before we consider the cases included under the two last headings (9 and 10) it seems well to discuss the attitude of the *O*'s toward the instructions during the last part of Experiment D. The smallest number of observations made by any one *O* was 55, the largest number, 73. The same instructions were used throughout the experiment and the same kind of material. For the sake of convenience and somewhat as a matter of course, the material was divided into 4 groups; the first group containing 10; the second, 13; the third, 15; and the fourth, 35 three-word situations. So far as the *O*'s were concerned, the instructions underwent a process of specialization. That is to say, the reported behavior of the 3-word imagery at the beginning of the experiment is quite different from its behavior at the end. Let us, for purposes of discussion, divide our classification of the behavior of the 3-word imagery into 5 sections. The first section will include those cases in which the 3-word imagery dropped out either before or at the very beginning of the reading of the problem. The second section will include all cases in which the 3-word imagery hung over during either the reading or the solution of the problem, but in which it did not aid in the anchoring or the solution of the problem. In the third section will be placed the cases in which the 3-word imagery was used as material for anchoring or solving the problem; and in the fourth section, the special and relatively infrequent uses of the 3-word imagery listed under headings 6, 7, and 8 of our classification. There remain for the fifth section the cases in which the 3-word imagery was present throughout the entire solution of the problem. All 4 groups of the 3-word situations will be found in the first section. This is to be expected, since the adequacy of the 3-words for setting up imagery cannot be predetermined, and such a word-situation may occur in any one of the 4 groups. That the *O*'s were, as time went on, more exactly obeying the instructions to take a passive attitude is shown by the relatively small number of cases in group 4 in which the 3-word imagery dropped out before the reading of the problem. In the cases included in the second section it is clear that the 3-word imagery is of more importance than it had been before. The *O* does not succeed in getting rid of it when the problem comes. This is perhaps due to the assumption of a more passive attitude with respect to the 3-word imagery. Nevertheless, the state of affairs is not satisfactory to the *O;* for he is hindered in both his comprehension and solution of the problem by the 3-word imagery. Group 4 of the 3-word situations contains the smallest number of cases of this kind, and group 3 the next smallest. This means that as the experiment went on the *O* was making a better adaptation to the complicated situation. When we come to a consideration of the third section we see at once a change. The 3-word imagery does not drop out with the coming of the problem, nor does it remain to disturb the *O* in his solution of the problem. It is used. The *O* has succeeded in meeting the situation with a minimum of annoyance to himself. Two *O*'s did not reach this stage until the fourth group of 3-word situations, and one *O* never reached it at all.

The cases included in the fourth section we have discussed above. They are all illustrative of the uses of the 3-word imagery and, for the most part, occur in groups 3 and 4. What has been happening has been a specialisation of the instructions which is correlated with a successful adjustment to the situation. The specialisation has been in favor of the 3-word imagery. Instead of being a factor to be eliminated, it becomes one of value in meeting the situation, that is, in solving the problem.

In the fifth section, which includes headings 9 and 10 of our classification, the specialisation is continued.

In stages 9 and 10 the 3-word imagery becomes so favored that it remains through the solution of the problem, either together with the problem-imagery or to the exclusion of the problem-imagery. There occurs a shift of relevancy from the problem-solving to the experiment as a whole. What the *O* does is to attend to both kinds of imagery, the 3-word and the problem. He is interested now, not so much in solving the problem, as in giving a good report; a report that shall describe both the 3-word and the problem imagery. This sort of reaction to the experiment is confined to 2 *O*'s, and occurs in the case of one of them altogether within group 4 of the 3-word situations and in the case of the other within groups 2, 3, and 4. The reports show that in these instances a fluctuation between the 3-word imagery and the problem-imagery takes place, and that in the fluctuation the 3-word imagery does not completely disappear, though it loses in clearness and sometimes disappears for good before the problem-imagery disappears and the problem has been solved. The 3 cases of superposition of the problem-imagery on the picture-imagery referred to in part 1 of Experiment D,[39] and the one case in which the imagery used in the completion-test remained during the solution of the problem noted in part 2 of the same experiment,[40] are similar to the cases described here. They were reported by the same *O*, a fact which is further evidence of a special interpretation of instructions and of a shift in relevancy from the problem solving to the experiment as a whole.

In the 5 cases (confined to the reports of 2 *O*'s) found in part 3 of Experiment D and included under the tenth heading of the classification of the behavior of the 3-word imagery, the 3-word imagery is altogether favored. The *O* can give no report of the processes leading to the solution of the problem. In some instances he states that there was some process present but that "none came into great clearness." The other *O* stated that the answer was given automatically. In this connection we would call attention to the fact that in none of the 5 cases did the *O*'s have any doubt as to the relevancy of the imagery present. In every case they were certain that the imagery belonged to the 3-word situation and not to the problem, though they could not report on the imagery used in solving the problem. If, as Dr. Koffka thinks, we do not know whether or not our imagery in a given case is relevant or irrelevant, the *O*'s might well have reported, since they were specifically asked to give such a report, the 3-word imagery as the carrier of the solution of the problem. But this is exactly what they did not do.

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[39] Cf. p. 222 above.

[40] Cf. p. 223 above.

The results of this experiment (D) confirm the results of those preceding. We have shown that, when imagery is occupying the focus of attention, if a new situation bearing no relation to it is introduced, two things may happen; the imagery either drops out, or it remains and is used in the new situation. We have found also that there may occur a specialisation of instructions, which means a shift of relevancy from one attitude to another, but that in no case is there any irrelevant imagery reported.

General Conclusion

All of the experimental work has led us to one conclusion, that *there is no irrelevant imagery.* That at the outset we had expected to find it is clear from our experimental procedure. We hoped at first to get some evidences of it indirectly; and when we did not succeed we arranged situations in which there were irrelevant factors. The analysis of irrelevancies on the perceptive level showed that they are principally characterised by a feeling-component. This feeling-component we found in later experiments, though it was never a characteristic of irrelevant imagery, but always of a total situation irrelevant to another situation, each of which situations had its own relevant imagery. When we finally succeeded, in Experiment D, in giving our *O*'s a background of imagery logically irrelevant to a situation later introduced, we still found no irrelevant imagery. In such a case, if the imagery could be used either as a whole or in part, it remained; otherwise it dropped out. We can, then, answer Dr. Koffka's criticism that "sensory contents may be irrelevant to the thought;"[41] for we have seen that, if imagery is present, it is relevant; and accordingly, if the author to whom he refers meant by analysis "nothing else than the exhibition of the sensory contents present at any given moment,"[42] she was right in assuming that these contents were relevant to the thought.

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[41] Koffka, *op. cit.*, 219.

We have called attention to the fact that not only did all of the *O*'s use imagery but that they frequently expressed a felt need for it. We have also brought evidence that this dependence upon imagery is not the result of laboratory conditions.

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[42] *Ibid.*, 219.

AN OBJECTIVE INTERPRETATION OF MEANINGS

By J. R. Kantor

Paradoxical as it may seem, it is still true that the problem of meanings, which provided so much difficulty for the introspective psychologists, meets with a comparatively simple solution by the methods and materials of the objective psychologist. The introspective psychologist experienced great difficulty in the interpretation of meanings because, presumably, the latter were supposed to possess essentially an inner character, which could not be identified with mental content of any particular sort, meanings being considered still more central than sensations or images. Nor is this difficulty much minimized by the parallelistic behaviorists[1] who translate the specific meaning-factor into partial movements of the eye, hand, or some muscle. For a meaning is in no sense a thing or a content either mental or physiological, but rather an act or an adjustment of the person,[2] which conditions another and following reaction.

I

Our first and fundamental departure from the traditional descriptions of meaning-functions may be summed up in the statement that meanings, in common with all other data of psychology, are definite responses to stimulating objects and conditions. By meanings we understand specific characteristic differential responses to particular stimulating objects in their appropriate settings. Now as meaning-reactions these responses differ from other differential reactions in the fact that the former are not complete adaptations or final adjustments to stimulating circumstances, but rather their function is to condition the specific operation of another succeeding or consummatory reaction. Thus my perception, that is, the appreciation of the presence or the identity of the book that lies before me on the table, is a precurrent or anticipatory response conditioning the further act of picking up the book or allowing it to lie undisturbed.

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[1] From our standpoint, of course, a psychological theory does not lose its parallelistic character even though the adherent thereof rejects one or the other series of supposedly parallel phenomena.

[2] That is to say, the complete operation of a reaction system.

Let it be at once noted that a meaning-reaction differs from any other kind of psychological response only in the fact that it serves to condition a succeeding act. Otherwise, it may comprise the same number and kind of factors, such as cortical centers, neural pathways, affective components, etc. In other words, meaning-reactions differ from other reactions only in function. In consequence, they represent the acquisition of various combinations of reactions, such that when the stimulus calls out one of them, it will be a means of bringing the other or others into operation. In other words, meaning-reactions consist of specific integrations of precurrent and consummatory responses, the former of which are already linked with specific stimulating objects or conditions. Thus, when the signal light flashes into the visual field of the locomotive engineer, the perceptual response is coupled with a series of other responses which result in the stopping of the locomotive. From this stand-point the meaning-reaction derives its name from the fact that it serves as a definite means for the functioning of some given reaction, picking up the book or pulling back the locomotive lever.

Since the unit of behavior or psychological reaction is the segment of behavior, or the response to a stimulus, we can, by referring to such a segment of behavior, indicate more precisely how the meaning-reaction operates. In detail, this operation is as follows. The stimulus-object brings into function a definite reaction-system which has been developed in direct contact with the object in question.[3] When so developed, this reaction-system operates as an evaluatory response in the sense that it serves to mediate an appropriate final adaptation to our illustrative object.[4] The point here is that, when we develop a differential reaction to an object in a given setting, we have appraised and evaluated the object from the standpoint of our behavior toward it.[5] This point is illustrated by the fact that all but the most abstruse definitions of things are stateable in terms of how we react to things. A table is " to put something on," as the child describes it. These evaluatory responses are developed, of course, with reference not only to objects but also to the specific qualities and relations of objects, as well as to events and conditions of every description.

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[3] By a reaction-system we mean the series of factors; sensitivity to stimulus, receptor and effector mechanisms, neural activity, muscular and glandular functioning, etc., which make up part of a psychological response.

[4] A segment of behavior consists of a stimulus and the series of reaction-systems (when there is a series) that is the pattern of response, which constitutes an adjustment to the stimulus.

[5] This development takes place irrespective entirely of whether the person knows it is taking place.

When a quality of an object is evaluated, we must observe that in many cases the person's reaction may be much more passive, or we might say the object is evaluated more on the basis of what it does to the person than upon what the person does to the object. In such cases the evaluations of things and their qualities go back to a non-observable condition or action in the organism, possibly an electro-chemical change, which we refer to as the differential sensitivity of the organism to color, taste, temperature, etc. Although, such activities of the organism are hidden from the view of the observer, whether it be the reacting person or some other, they are none the less definite acts, precisely as are the digestive acts, the occurrence of which is unseen and may even be unknown to the food-consuming person. It is possibly not an imprudent suggestion that, while the substantive meanings of simple perceptual objects are derived from active operations upon such objects, the adjectival meanings, on the other hand, are derived from the more subtle and for the most part unobservable reactions which we call sensitivity. In passing we might suggest that our term sensitivity is used in the sense that the physicist uses the term when speaking of the deflective actions of a galvanometer. Obviously, whenever any meaning-reaction is named or brought to the notice of oneself or others, a positive observable reaction is being performed.

Impossible it is to omit in a discussion of meaning-responses the specific reactional auspices under which the meaning-acts are performed, since the stimulating circumstances are not by far the least important of the conditions for the origin and operation of meaning-responses. This means to say that the meaning or significance of an adjustment is entirely a function of its appropriateness in any given circumstance. In other words, meaning-functions symbolize specific forms of interaction between objects and the person's responses to them. The kind, number, and relations of objects to which we react force us to develop combinations of responses for adaptational purposes. What an object means is intrinsically the problem of the particular type of response which the person has developed while in contact with it. As a psychological term, therefore, meaning refers (1) to the significance which an object has for the person as indicated by his reaction to it, and (2) to the particular type of reaction which a given object brings about in the person.

From our standpoint there is nothing strange or inscrutable about meanings. The fact is that the meaning-reactions which appear as such effective adaptational aids to the person are merely the anticipatory responses which the various surrounding stimuli have caused persons to build up or otherwise to acquire. A meaning-reaction is accordingly a fitting adjustmental response which individuals acquire through the direct influence of the surrounding objects and conditions. It is in this way that a meaning-reaction becomes the means for bringing about an especially fitting adjustment of the person to his surroundings. And it is this type of precurrent meaning-reaction which permits us to say that a person foresees the consequences of an act. For it is precisely such meaning-reactions as we have described which make it possible for us to have any delayed reactions. We might even go a step further and say that it is the development and operation of these anticipatory meaning-reactions which are celebrated by the term consciousness. We may repeat, then, that a meaning is a psychological action, in no sense distinct from the many other definite responses which we are hourly performing. In other words, a psychological meaning is not anything mental or psychic, nor is it merely a peculiar muscular or glandular reaction paralleling a mental state, but rather a meaning-reaction is *any* reaction of the person which stands for or signifies a thing or condition by causing a particular response to it.[6] But of course meaning-responses differ from each other and from other members of a segment of behavior in precisely the same way as any two human reactions may differ.

II

Up to this point, namely, as long as we are discussing direct responses to present stimuli, everything seems clear and definite enough. But uncertainties appear at once when we consider the behavior in which the objects to which we adjust ourselves are not immediately present. How, we might ask, can we interpret the meanings and intentions concerning persons, things or other stimuli which are not at the moment within range of our actions? Here we have the problem of the detached meaning-reactions which are usually referred to as thought and imagery. To the objective psychologist it

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[6] Regardless, of course, of whether the person knows what is taking place. In order that the reacting person should also *know* what is going on, he must not only be determined, because of some precurrent reaction, to perform a definite final adjustment, but in addition he must be able to report to himself verbally or otherwise that such an event has taken place.

seems an extreme error to overlook the unquestioned continuity between what may be called the perceptual and non-perceptual meaning-reactions. Imagery, or non-perceptual, meaning-reactions are no less definite responses of the person than are the meaning-reactions contained in segments of behavior in which the original object is present.

As a matter of fact, every genuine perceptual response already involves a partially detached or implicit reaction, in the sense that the specific perceptual phase of the segment of behavior is a revival of a differential response to qualities and conditions of objects not at the moment in actual contact with the organism.[7] The main point here is that a differential reaction system or pattern of response which was acquired in the original contact with the object in question is upon a second or later contact with the same object put into operation in the same way or in a slightly modified form.[8] When the object which originally caused the reaction to be built up is absent, the differential reaction system can still be put into action by a substitution object. In this case, of course, the reaction system will be an implicit or an incipient operation. The response will occur only in part or in some lesser degree. There is no reason to suppose, however, that such an implicit response to an object not present, does not involve the same neural, receptor, and effector apparatus.

Furthermore, since all implicitly operating reaction systems, no matter how closely they resemble the original act, are substitutive reactions we are not surprised that it is possible for them to become symbolic. That an implicit reaction system determining a succeeding response can be very unlike the overt act which would ordinarily stimulate the succeeding response, or entirely different from the overt response which a given object elicits, is traceable directly to the fact that every implicit reaction is already in some sense a representative activity. Whenever we respond to an object not present, be it in a dream, revery, thinking, or planning of whatever sort, then we are symbolizing or representing the object or situation constituting the original stimulus to our behavior. Now although there is no limit to the degree with which the implicit response varies from the original reaction to the same object, still there are definite determining conditions which make for the specific symbolization of objects by particular reaction systems. These determining conditions are for the most part contained in the original environmental setting of the object which is being reacted to. Just why my implicit reactions to the city of Washington should invariably involve a symbol of fatigue is no doubt explained by the fact that my first contact with that city included an ennervating round of continuous sightseeing. As every one knows the implicit reaction systems of thinking processes appear in most cases as entirely unlike anything that we should expect to be connected with the original stimulating situation. No further illustration of the peculiarities of symbolized implicit behavior is necessary than a reference to the facts of individual imagery differences.

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[7] Cf. Kantor, Suggestions Toward a Scientific Interpretation of Perception, *Psychol. Rev.*, 27, 191 ff.

[8] Such modification depends upon the setting, or the total stimulating situation.

Although it may not at all reinforce our conviction that an implicit meaning-reaction is a detached response to an object, it is well to observe just how a reaction system or response pattern can be detached from a situation in which it was first acquired. We have here really two problems. Not only must we account for the detaching of reaction systems, but we must also describe the mechanism by which such detached reaction systems are put into operation by some substitute for the original stimulating circumstance. Naturally enough these are reciprocal problems and the solutions are closely intertwined.

In general, the possibility of detaching reactions from their original settings goes back to the elementary fact that human persons are from the psychological standpoint organizations of response systems. A psychological fact consists of the operation of one of these reaction systems resulting in the adjustment of the person to an object or condition. Of the most elementary of these adjustments are the connate reflexes and the sensitivity to colors, sounds, etc. By contact with objects these primitive responses may become integrated into more complex adjustments to objects or into reaction patterns; so that the person will respond not only to color and shape, but also to the complex objects as a whole. Nor does the reaction to objects define the limit to human reactions. Suffice it to suggest that the next hypothetical step is the organization of the reaction to include the setting of the object. Mark well our point, namely, that the whole of the individual, psychologically speaking, is an enormously expanded series of such potential responses.

Now it so happens that such reactions are sometimes called out when the original objects responsible for their existence are absent. That this operation of the reaction system can occur without the instrumentality of the original stimulus is accounted for by the existence of some common factor in the present and past situation. And so the infant may proceed to suck any object, although it may only very remotely resemble a nipple. In more complex situations the common element responsible for a given reaction may be the setting of the stimulus, and not any quality of it. Illustrative of the latter situation are the mistaken reactions resulting when we respond to a stranger in a way in which we have accustomed ourselves to react to a friend, who usually is found in the place which the stranger now occupies.

Again, a person is stimulated by wants, organic necessities, and desires, to reinstate some reaction system previously built up under entirely other auspices. A hunger reaction may stimulate us to reach for and perform other incipient reactions to food-objects, although no food-objects are to be found in the vicinity. To a certain extent, also, we might consider that the process of detaching reactions from the original stimulating circumstances in which they occur is merely a process of continuing an action once the original stimulus for it has passed. As an example we might refer to the tense organic strain and reverberation of the person who virtually is unable to stop running or rowing for some time after the actual contest is over.

We may conclude, then, that the different mechanisms for detaching reactions from their original settings involve in some sense a rearousal or a functional continuance of a reaction system. When the rearousal occurs we may trace the stimulus back (1) to some object or condition other than, but previously connected with, the original stimulus object, or (2) to some present reaction of the person serving as a stimulus to such reoperation of a reaction system. No matter how the rearousal takes place, the detached reaction may become a means and a determiner of the operation of an associated succeeding response. What this associated response is depends upon the specific experiences of the person. Plainly, it is extremely important for the process of detaching reactions from their original stimuli-response situations that there be some similarity and resemblance between the various objects and situations concerned; so that they can substitute for each other as stimuli.

III

Thus our discussion brings us up squarely against the problem of the image, since it is pretty clear that imagery reactions are detached vestiges of sometime overt reactions to concrete objects. Imagery responses, therefore, must be considered as one of the types of the individual's reactions, along with overt responses and others which determine the immediately succeeding reactions to things. What the proposed hypothesis demands of us to believe is that every psychological process is a specific operation of the person to given objects or situations. To accept this theory means to believe that any perceptual activity is a particularized action, performed when stimulated by any given object with all its qualities, as it comes into direct contact with the person. That is to say, the theory demands that we do not assume that the qualities of things or the things themselves exist as central material, or *in consciousness,* as the textbooks put it. Lest there be any question as to our meaning at this stage of our discussion, we reiterate that, in every case of psychological reaction, perceptual responses by no means excepted, the person reacts to a thing in which inhere all of its qualities. Hence we are not obliged in the case of imagery reactions to account for central qualities. The difference between perceiving and imagining a book lies in the fact that in perceiving it we react to it with its qualities, while, when we imagine a book, we must supply the qualities and relations of things, at least when the imagery is vivid, by means of verbal or language substitutions. This fact accounts for the usually greater vividness of the perceptual reactions. And here we might suggest that the traditional difficulties with imagery reactions are born of the prejudice that the image which is presumed to be the carrier of non-perceptual meanings is a peculiar central process absolutely independent of receptor systems and muscular mechanisms, in addition to being otherwise related than are sensory processes to cortical centers. From our standpoint an image response is a reaction system, in principle precisely like any other, involving exactly the same factors, but differing from other reaction systems in that the former are in some manner distantly removed from the primary stimulus-response situation in which they originally developed.

And what precisely is an image? Why simply this, a vividly repeated reaction system or pattern of response to some specific situation, plus the speech reactions descriptive of the objects and events reacted to. In many cases the nature of the image depends upon its verbal purveyance, in the sense that the reported qualities of imaged things are supplied by the person himself. This fact is clearly demonstrated when we attempt to imagine an object which we have looked at some time ago or perhaps have never actually seen. The infidelity of testimony illuminates the possibilities in the way of adding materials to past objects and events. To be entirely specific at this point, we must indicate that much of the image experience is really a verbal self-analysis of how we respond to an absent object. Thus, contrary to popular conception, the most intense imagery reactions are those in which the person repeats verbally the scene he has just witnessed or has otherwise partaken of. Consider the young lawyer just returned from his first important legal argument, or the lover rehearsing before himself as auditor the scene just preceding the capitulation of the most adored one. The amount and intensity of the imagery depends upon the impressiveness and the intensity of the original situation.

Although much of the image reaction is verbally supplied and verbally stimulated, yet the basic fact in any imagery experience is the actual responsiveness and organic functioning which is involved in the implicit repetition of a person's activity. Anyone attempting to recall what happens when deeply suffering from the pangs of remorse, or writhing under the lash of insult or the sting of disappointing dejection, will have no trouble in appreciating the definite activity of the person. Impossible it is to overlook the shuddering of the entire person, the palpitation of the heart, the activity of the salivary, lachrymal and other glandular processes which take place, when we remember or think about some distressing or very pleasant experience. Above all, we must bear in mind always that we are attempting to describe the behavior of a tense superactive organism, and not the products of a cold logical analysis. What happens when any of our imagery experiences become blunted, as is practically always the case with the passing of time, is that the pulsation and quavering of the organism have subsided.

From the fact that the core of imagery reactions is a definite liberation of the person it follows that by far the most vivid imagery is that in which the individual is himself living over a profoundly impressive experience. So vivid at times is such imagery that one may appear to be in a certain place and actually to speak and otherwise react to persons, when as a matter of fact one is at great distance from both the person and the place which are now being implicitly reacted to. Moreover, an implicit reaction to a past experience may involve almost as much actual expenditure of energy as an overt adjustment to a less intense situation. Less vivid, naturally, are the imagery responses which constitute implicit reactions to conditions with which one is not personally concerned. In general the energy with which our implicit reactions operate depends upon our capacity to relive the original situation. And so the warmth with which one sympathizes with another person who suffers some tragic experience is dependent upon the fact whether the sympathizer has himself played a part in a similar event, and is consequently able to relive it, to image it better. Here we find the psychological basis for the emphasis which the employer places upon *experience* as a qualification for employment. It is this, that having previously made reactions to a type of situation, one is now better equipped to react implicitly to the same or a similar situation and thus be more resourceful in the present circumstances.[9]

Because the implicit meaning-reactions are so easily performed and occur so much more readily and quickly than the explicit type of response, and moreover because the former are so subtle and representative, they serve as the most capable and efficient of meaning-reactions. Indeed, even though the older psychologists did not fully recognize the character of imagery, they hit upon images as the exclusive type of meaning-responses. Not even the mistake of making images the only type of meaning-reactions can rob those psychologists of the credit for their insight into the character of imagery reactions. Just how efficient these implicit reactions are may be gathered from the consideration of a thought or a planning segment of behavior, in which the compelling stimulating circumstances induce a very intricate interplay of implicit reactions, serving on the whole as backward references to events in the life of the person important to him in the particular circumstances, besides enabling him to anticipate future possibilities of action. Of course in any serious problematic situation the person will combine such implicit responses as we have attempted to describe, with explicit handling of maps, statistical tables, drawings, slide rule, books, and other such instruments of complex human behavior. The explicit acts serve as stimuli to actions as well as being themselves adaptive responses. We hasten to add that, by considering the actual complex of implicit and overt reactions constituting a planning behavior, we gain insight into the actual continuity of such actions throughout all their variations.

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[9] We can no better illustrate the repetitive nature of an implicit reaction than by pointing out the necessity of repeating reactions in order to recall or understand them, or to reproduce facial expressions in order to appreciate their significance or to name them.

IV

If our hypothesis concerning the nature of meaning-reactions is valid, it follows as a matter of course that the implicit meanings cover a wide range of precurrent responses. Now as a matter of fact, all of these responses may be roughly grouped between two limiting classes, which we will name representative and substitutive. By a representative meaning-reaction we understand a reaction system similar to one performed upon a previous occasion and now serving as a determiner of a succeeding reaction. Thus the representative meaning-reaction is a direct vestige of a previous reaction situation, and consequently is morphologically a fairly overt response, in the sense that it incipiently repeats a former response. Such representative reaction systems, therefore, stand upon the borderline between the overt and implicit reactions. We may consider as examples of the representative meaning-reactions all of the imagery responses that we have discussed; remembering, however, that other representative meaning-reactions involve the elaborate movement of external skeletal muscles.

At the opposite pole from these representative reactions stand the substitutive responses, which, though functioning as determiners of reactions, are themselves morphologically completely at variance with the overt act producing the same result. These substitutive responses, while of course definite acts of the person, do not in any sense resemble the reactions to the objects for which they substitute; they may be totally symbolic; and so difficult is it to seize hold of such meaning-responses that their operation in many cases is frankly inferred. The fact is that the symbolic reaction may involve such an act on the part of the person as he himself has no notion of. In the literature of psychology the substitutive or symbolic reactions are denominated concepts or thoughts. So distantly removed are the substitutive responses from the original conditions to which the person is adjusting himself, that the stimulus to thought action is a total situation or problem. Now, the detailed facts of the stimulating situation are supplied to the person by drawings, writings, or verbal stimuli. The general problem, however, may be looked upon as the directing and controlling stimulus.

Between the fully representative and the substitutive reactions we find interpolated the language responses which constitute a most efficient form of meaning-reaction. The marvellous effectiveness of the verbal reactions to determine behavior lies in the fact that they are not only completely overt morphologically, but they represent the most facile of all our performed actions and at the same time they are capable of infinite modification. In consequence, verbal responses are among the most satisfactory substitutes for all sorts of objects and acts. Finally, the language reactions constitute so pervasive a form of human activity, that they connect with and bring to the surface the deep seated conceptual responses. Thus, the conceptual responses, for example, are most serviceable for reactional purposes when they are associated with language acts. This fact is evident when we consider that ideas are simple concepts which, because of their attachment to verbal reactions, serve to induce responses in oneself and others.

V

Before proceeding to a brief description of the conceptual reaction we might raise the question how the implicit reaction, which resembles in no way the original object or the original reaction, can be said to be a reaction to that object at all. The answer to this question is found in the consideration of the manner in which the reaction operates. The substitute reactions operate as precurrent or anticipatory responses to some other final reaction, and this is exactly why they are meaning-reactions. As we have already indicated, the entire significance of a meaning-reaction lies in the fact of its operation as a determiner of a succeeding final response to a given stimulating object or condition. Whether a given reaction system is a response to a particular stimulus depends entirely upon its functional connection with that stimulus.

Concepts are reaction systems which operate when it is necessary for us to make immediate use of large segments of our past experienecs in rapid and effective ways. The mechanism for this activity is as follows. Some problem presents itself to us, the construction of a bridge, let us say. It is necessary for us to correlate this problem immediately with other problems of a similar sort, in order to make plans for the new structure or to draw up an estimate of cost. In terms of the old work, old conditions of all sorts, we must project and create the new object or condition. For this purpose we have a stock of concepts or ideas representing our past experience of a particular sort, which now conditions the actions of making plans, submitting estimates, and directing the actual bridge construction.

The concept is therefore a vestigial remnant from a previous situation or rather a series of situations; for a concept is a standardized and definite implicit response which substitutes for and sums up the person's experiences in a form useful for present purposes. Plainly, the capacity to develop concepts depends upon verbal aids, since only through such symbolic means can we build up such meaning-functions as concepts are. Not only are language functions necessary for the development of concepts or standardized meaning-reactions, but the use of them depends very closely upon word responses. It is through words (spoken, written or printed) that concepts are primarily stimulated to action. Hence we may look upon a treatise as an extension of the person's implicit reactions to things. By means of a treatise we are enabled to sum up and record the significant facts of our past experience.

Incidentally, there is brought to our attention the distinction between different types of concepts, namely, those standard implicit reactions summing up our own experience, and those concepts which are derived from our indirect contact with things, from our reading and hearing of bridges built, for instance. Obviously, the largest number, by far, of our more important concepts are derived in the indirect way, although it is an essential factor in the development of a concept that the person must have had some personal experience with the fact or conditions represented. The degree to which we are unable to grasp a concept or have one communicated to us is a direct function of our lack of experience with the facts and conditions represented. When we have had no actual contact with a certain object or condition we can have no concept of it, and therefore we can only have verbal substitutes. Every teacher, it is almost safe to say, has met with persons who function as students entirely on the verbal level, or almost so.

In considering the differences between a scientific concept and one developed in everyday life we cannot overlook the deliberate operations which the scientist performs upon the materials with which he deals. We might look upon this deliberative activity as in part a direct manipulation of materials and apparatus and in part an implicit handling of these materials with direct reference to previous experiences of the same sort. All of this activity is performed with the constant aid, and by means of, communicative language activity, both stimulative and responsive.

The distinction between scientific and everyday concepts marks a difference in the levels of our behavior. These levels may be arranged in series from the mere implicit contact of the person with things to a thorough understanding and manipulation of objects for certain definitely appreciated purposes. The actual basis for the distributive arrangement of the levels of behavior lies in the intimacy of the contacts of the person with the objects and conditions to which he is adapting himself. Clearly, the intimacy with which one is in contact with surrounding things is not in direct correlation with the overtness of the response. For obviously we may produce more important effects upon things by the indirect responses of thinking than by most direct contacts of a perceptual sort. Now in point of fact, the scientific concepts operate in a more remote way upon objects than do the everyday concepts. As we have previously suggested, we may consider scientific concepts to be reactions operating upon a level of deliberated and motivated action and therefore very different in degree from everyday concepts. From the fact that scientific concepts are practically identical with ideas, we may infer that the difference in degree between concepts parallels such crystallization of reactions as to make them available as stimuli to actions.

In a sense, a conceptual act is a self-stimulating reaction to further implicit behavior, so that a thinking activity which is essentially a manipulation of concepts is a continuous activity of the person with respect to some object or condition. When the conceptual reaction becomes so standardized and identified with a term or name that it may serve as a common stimulus to various persons, then we may call it an idea. The intimate connection between the concept and the communicative language form makes the concept a definite object of scientific technique, much as a piece of apparatus is. Absolutely essential is it for the functioning of ideas that they be embodied in language forms (names, etc.) and it is true, as a matter of fact, that such ideas are indistinguishable from the expressive language which serves as their medium of circulation. Thus language appears as an indispensable tool for both the operation and the expression of thinking.

VI

The development and functioning of language responses indicate most excellently the facts of meaning-reactions. For language is essentially a determiner of action, whether in the vocalizing person or in some other. In the fact that action is determined in others we find a basis of division of language mechanisms into mere vocalization and communicative speech. The latter type of meaning-reactions are not, however, confined to communication with others, for obviously one may also communicate with oneself. The differentiation between verbalization and true language activity, or between mere speech and communication, involves a difference in the total behavior of the person at the time. Essentially the total situation includes not only the responses of the person but the stimulating circumstances also. As to the latter, they must be such as to interest or challenge the reacting person, interest him in the form of satisfying his wants, as the mother or nurse functions to the desiring child. Or the stimulus may arouse or awake the person to a response which will in turn induce in the stimulator another response. Here we have of course ordinary conversation. As contrasted with mere verbal utterance as a determiner of behavior, communicative speech involves a definite attention set and an attitude of serious expectancy which puts the person in close *rapport* with the stimulating person.

All language in the sense of action determiner or verbal stimulus goes back of course to the articulate function of human beings, and we might assume that speaking or the use of words, both as mechanism and as psychological acts, derives directly from crying through the path of vocalization. All the observable facts of human language are constituted by the processes in which the crying mechanism and action develop to the stage of definite communication.

Much light may be obtained concerning the development and use of language from a study of the reactions of infancy. The growth of language may begin as a mere manifestation of a need. That is to say, the observer notes the infant acting in a particular way when he is apparently stimulated by some exteroceptive or interoceptive stimulus. This reaction may appear to the observer as a stretching out of the hand or a cry. As it happens, the reaching or crying occurs when the infant is stimulated by objects he cannot reach or otherwise adequately make responses to. The reaching or crying is, then, a gesture or stimulus to the mother or nurse to complete the necessary reaction, and in this manner the reaching or crying of the child becomes a means of finally attaining some result. For a long time the child's activity has no significance to him in the sense that he has no manner of appreciation of its operation. At this stage the action of the child is a bare meaning-act or a means to some other reaction, playing with the ball or drinking the milk.[10] Because of the particular situation this reaction of the child to be sure is pregnant with potentiality to develop into genuine language.

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[10] We ignore absolutely any standpoint of an observer.

Another stage in the development of language involves the appreciation by the child of the stimulating character of his reaction. This appreciation arises from the observation of the close connection between his own beginning and final reaction, and that of the mother, since the latter's action is a necessary part of the total situation. In many cases this appreciation may merely amount to the fact that the mother has trained the child to substitute a true stimulus word (name) for the original crying or reaching stimulus.[11] Since the development that we are describing consists mainly in the child's learning that his act is a stimulus, the use of a name is not an essential part of the development. Any sort of gesture will do. The entire criterion for the description of the developmental stages in the language reactions lies naturally in the specific ways in which the person is in contact with his surroundings.

Distinctive as a stage in the development of language reactions is the performance of verbal actions as stimuli-determiners of the actions of others. We have already seen that this stimulating act may be merely the expression of a need which is satisfied through the act of another. Later, this stimulus is uttered as a deliberate means of achieving some definite end. Obviously, the best means of accomplishing such purposes is by the use of connected speech-stimuli. "Get-me-this" may be considered as a typical illustration of such a connected speech determiner of action. And it is very important to observe that we have attained here a stage beyond the mere use of name words. In fact we may think of this communicative-stimulus or deliberate, transmissive reaction as a phrasing or speech-expressing reaction which transplants simpler need and want reactions. These connected speech reactions are definitely made to exhibit or express a need, desire, or some other condition of the person, or an implicit response of quite another sort. Such language reactions essentially involve segments of behavior including reaction systems of at least two individuals, either as stimuli or responses.

In this stage of language development words or their equivalents are not mere substitutes for objects or acts but are definite stimuli for one's own reactions or the responses of others. A clear example of communicating with oneself is the use of language in the formulation of plans or the application of pencil marks upon paper during the solution of a mathematical problem.

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[11] That is, stimulus for the mother.

By far the most elaborate use of communicative language reactions is found in the informational responses which are stimulated to action by another person or his presence. Especially is this the case in informational reactions in which one person apprises another concerning objects or events which have long ceased to be actual factors in the surroundings of one or both persons. By means of communicative language reactions, not only can old events be revived and brought again to the surface, but in general one person may inform another of comparatively new objects or events by way, of course, of stimulating in the other person implicit responses representative of similar conditions.

In general, we might look upon language reactions as instrument reactions; they enable the person to extend his activities and to reach across all sorts of barriers. The use of language reactions puts the human individual upon a definite and characteristic level of action. As instrument reactions, language represents one of the highest developments of the meaning-responses. Not only is language a determiner of overt and implicit reactions of all sorts, but it also serves at the same time as a complex means of developing newer and more capable reactions. We have already suggested that the great efficiency of language acts lies in their capacity to integrate with other reactions and thus form a part of all planning, thinking, and choosing. To a considerable extent language responses constitute the primary factors in the higher types of delayed reactions. We might add in passing that it is the capacity of verbal responses to integrate readily with other reactions, and not any mysterious power, which makes of language such a distinctly human form of behavior.

To obviate any misunderstanding, let us state at once that our remarks concerning language do not apply to printed words or signs, nor perhaps to written words. Since, obviously, printed words or sentences, not being reactions at all, cannot be meaning-responses. They are stimuli pure and simple, although the act of producing written words may be considered to be a meaning-reaction of a highly sublimated sort. In this connection it is well to note that there are distinct levels of action, and that a distinct difference exists between the spontaneous meaning-responses of a personal sort and the deliberative institutional acts constituting the publication of a theory by a scientist or the formulation of a law by a legislature. The latter acts are means to some other acts, of course, but since they function much less directly they belong to a different level of behavior. Such acts are means to other acts through the instrumentality of institutional or common stimuli, and are not themselves personal stimuli. Written and printed language are common stimuli, and hence are psychological data in no other sense than is any other type of stimulating object. The acts producing these common stimuli are just as indirect means to reaction as are those which result in the construction of a house, or driving an automobile into the path of traffic.

VII

We conclude, then, that the problem of meanings in psychology involves no other factors than those which are dealt with and described by the ordinary methods of objective science. Not only the simple reactions of the person while in direct contact with objects, but also the most complex thought and memory responses, are definite integrations of responses and stimulating conditions. In other words, the most intricate intentional action may well be considered as an elaborate organization of the person's actions under definite conditions of stimulation. Incidentally, we observe that meaning-responses are not limited to thought processes, but are parts of reaction patterns including all types of reactions. From this list of reactions, habit responses are not exceptions and in fact in every segment of behavior in which there are two or more reaction systems, one serves as a precurrent determining response for others, and therefore answers to our description of a meaning-reaction.

Since precurrent reactions are both overt and implicit, we shall find both types of responses operating as anticipatory determiners of action. For these two types of responses merely represent the different ways in which the person responds to his surrounding stimuli. Our discussion has indicated that the list of precurrent reactions includes not only concepts and images, but also language reactions and other more definitely direct operations upon stimulating objects, namely, those involving the skeletal musculature.

A QUALITATIVE INVESTIGATION OF THE EFFECT OF MODE OF PRESENTATION UPON THE PROCESS OF LEARNING

By Francis J. O'Brien[1]

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History

The relation of the mode of presentation of a material to the process of learning it has been approached experimentally from various points of view. Most investigators have sought, with a pedagogical interest, simply to determine the mode of presentation most advantageous in learning; a few have concerned themselves chiefly with the psychological aspects of the problem and undertaken to determine the qualitative changes in the complete mental process that arise when the material to be learned is presented to different senses. Our present problem is psychological in this latter sense. In most studies investigators have been content to deal merely with the quantitative aspects of learning and recall, supplementing such data only occasionally by introspections. They have made the analysis of the learning-process almost entirely with reference to the mode of presentation of the material—whether it is visually or auditorially presented, or reinforced by vocimotor repetition, for example—and little with respect to the actual sensory terms with which the learner represents the material to himself. For this latter information, as well as for a complete understanding of the problem, an introspective study is essential.

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[1] From the Psychological Laboratory of Clark University. This work was performed under the direction of Professor J. W. Baird.

Münsterberg and Bigham[2] published the pioneer investigation in this field in 1894. They presented digits and colors to their observers in visual, in auditory, and in combined visual-auditory fashion, and tested the learning immediately by a method of reconstruction. They concluded that visual memory is superior to auditory and that material presented to both these senses at the same time is more easily reproduced than material presented only to the one or the other.

Cohn[3] took especial account of the motor factor in learning. Using lists of consonants as material, he compared learning with emphasis upon the vocimotor processes with learning with vocimotor processes inhibited. He sought to secure this inhibition by having the *O* press his tongue against the roof of his mouth, count "1, 2, 3, 1, 2, 3," etc., count backwards, or count by 2's or 3's. He concluded that learning was better when there was no attempt to interfere with vocimotor processes. It is possible, however, that his results were due to the distraction of attention by the inhibiting task.

Quantz,[4] in a study of the psychology of reading, performed memory experiments with visual-vocimotor, auditory, and visual-auditory-vocimotor modes of presentation. He concluded that lip movement is a serious hindrance to the speed of reading and that a combination of the visual and auditory modes of presentation has little advantage for recall over either mode of presentatoin separately.

Lay[5] studied the mode of presentation most effective in learning spelling, and demonstrated that visual presentation is much more effective than auditory and that the introduction of motor imagery is of considerable assistance in the learning processes. Fuchs and Haggenmüller[6] and Itschner[7] repeated Lay's experiments with certain variations and in general substantiated his findings.

Smedley[8] investigated the memory span of children for five different modes of presentation. He found that he obtained the greatest span with the visual-auditory-vocimotor mode, and that his other modes could not be completely realized because his subjects were unable entirely to repress their vocimotor processes.

Finzi[9] worked upon observational noting and retention as conditioned upon presentative modes. He worked without consideration of the imaginal types of his *O*'s, but concluded that vocimotor and manumotor imagery are least efficient for retention. Some of his *O*'s, however, remembered best when motor processes were introduced in addition to the others in the learning.

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[3] Cohn, J. Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses. *Zsch. f. Psychol.,* 15, 1897, 161-183.

[4] Quantz, J. O. Problems in the psychology of reading. *Psychol. Rev. Mon. Sup.,* 2, No. 1, 1897, 51 pp.

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[6] Fuchs, H., and Haggenmüller, A. Studien und Versuche über die Erlernung der Orthographie. *Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie,* II, 1898, 63 pp.

[7] Itschner, H. Lay's Rechtschreib-Reform. *H. Jahrbuch d. Vereins f. wissenschaft. Päd.,* 32, 1900, 206-234.

[8] Smedley, F. W. *Report of the department of child study and pedagogic investigation.* Chicago, No. 3, 1900-01, 63 pp.

[9] Finzi, J. Zur Untersuchung der Auffassungsfähigkeit und Merkfähigkeit. *Psychol. Arbeit.,* 3, 1900, 289-384.

Kemsies,[10] in a study of school children, concluded that visual presentation was more advantageous than auditory for learning German-Latin vocabularies. Fränkl[11] studied the same problem with more modes of presentation and concluded that Kemsies' conclusion holds for learners of the visual type but that auditory presentation is more efficient for learners of the auditory type. Schuyten,[12] on the basis of a similar experiment, concluded that 'pure auditory' presentation is superior to 'visual plus auditory' presentation. His 'pure auditory' presentation was, however, presumably auditory-vocimotor, and his experiment was made without reference to the imaginal type of the school children.

A more thorough investigation of this problem is Pohlmann's.[13] He worked with six modes of presentation and three intervals of recall. His general conclusions are that auditory presentation is slightly more advantageous than visual for such familiar material as meaningful words, that visual presentation is superior to auditory for nonsense syllables, and that retention is not improved by the addition of the auditory mode or the vocimotor mode of presentation to the visual.

Segal[14] worked with trained *O*'s and with visual and auditory presentation. In both cases he allowed the learner in certain series to use the visual-auditory mode with any imaginal supplementation that he desired, while in other series he required him to avoid vocimotor processes by carrying on simultaneously with the visual or auditory presentation some other irrelevant vocimotor process. He concluded that the same individual may be said to belong to different imaginal types when reproducing different materials, that optimal conditions for reproduction consist of the presentation of the material in the mode that corresponds to the *O*'s type, and that, when presentation is made in a mode different from the *O*'s type, the *O* either may learn less efficiently using the terms in which presentation is made or may immediately transpose the presented material into imagery corresponding to his type. The necessity for taking strict account of the *O*'s type under the particular experimental conditions and the need for a persistent appeal to introspection become apparent in this study.

Von Sybel[15] reinforced Segal's conclusion by an experiment involving six presentative modes. He noted especially that the imaginal type of the learners had to be considered before the most effective mode of learning could be established, and that imaginal type is not fixed, but subject to change under different conditions.

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[10] Kemsies, F. Gedächtnisuntersuchungen an Schulkindern. *Zsch. f. päd. Psychol.*, 2, 1900, 21-30; 3, 1901, 171-183.

[11] Fränkl, E. *Ueber Vorstellungs-Elemente und Aufmerksamkeit. Ein Beitrag zur experimentellen Psychologie.* 1905, 256 pp.

[12] Schuyten, M. C. Experimentelles zum Studium der gebräuchlichsten Methoden im fremdsprachlichen Unterricht. *Exper Päd.*, 1906, 3, 199-211.

[13] Pohlmann, A. *Experimentelle Beiträge zur Lehre vom Gedächtnis.* 1906, 191 pp.

[14] Segal, J. Ueber den Reproduktionstypus und das Reproduzieren von Vorstellungen. *Arch. f. d. ges. Psychol.*, 1908, 12, 124-236.

[15] Sybel, A. von. Ueber das Zusammenwirken verschiedener Sinnesgebiete bei Gedächtnisleistungen. *Zsch. f. Psychol.*, 53, 1909, 257-360.

Abbott[16] studied the nature of the mental processes involved in learning to spell unfamiliar English words. Her work was undertaken in order to test the results of Lay, Fuchs and Haggenmüller, and Itschner. She found, under her conditions, that the initial recall came typically as visual imagery of the letters of the word no matter what the mode of presentation, that the subject would proceed to pronounce the word only as soon as a clear visual image was obtained, that auditory presentation gave rise, by way of visual imagery, to an even more purely visual learning than did visual presentation, and that the mode of presentation appears, therefore, to determine the imaginal terms of the learning in only a small degree.

Meumann's[17] conclusions support the general trend of the foregoing experiments: it is more advantageous for a learner to use imagery corresponding to his imaginal type than for him to attempt the use of imagery in other modalities. Meumann holds that learning is dependent more upon the formation of strong associations than upon the formation of many associations and that therefore the material which is most readily impressed should be used. Frankfurther and Thiele[18] also came to this same conclusion, although they noted especially that the addition of other modes to the natural mode for the *O* increased efficiency of learning. For them the best results were obtained with visual-auditory-vocimotor learning.

MATERIALS, APPARATUS, PROCEDURE AND OBSERVERS

In the first half of the investiagtion we used significant words; in the second half, nonsense-syllables.

Significant Materials

This material consisted of four-letter English words,—nouns, pronouns, adjectives, verbs and adverbs,—which were arranged in 36 lists of 20 words each of approximately equal difficulty. The lists were divided into three series, *a*, *b*, and *c*, of 12 lists each.

Twelve modes of presentation were employed and in the order given below. The following symbols are used in this paper in abbreviation of the names of the modes of presentation: V=visual, A=auditory, M=vocimotor, m=manumotor.

1. *Auditory* (A). *E* pronounced the words, one every 3 sec., to the beat of a metronome. The learner was asked to inhibit vocimotor imagery during the learning.

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[16] Abbott, E. E. On the analysis of the factor of recall in the learning process. *Psychol. Mono.*, 11, 1909, 159-177.

[17] Meumann, E. *Vorlesungen zur Einführung in die experimentelle Pädagogik und ihre psychologischen Grundlagen*, II. 1907, vii+467pp.

[18] Frankfurther, W., and Thiele, R. Ueber den Zusammenhang zwischen Vostellungstypus und sensorischer Lernweise. *Zsch. f. Psychol.*, 62, 1912, 96-131.

2. *Auditory-vocimotor* (AM). The same procedure as in the purely auditory presentation (A), except that the learner was allowed the use of vocimotor imagery.

3. *Visual* (V). The series as they were presented visually on an exposure apparatus, one word every 3 sec. The learner was asked to inhibit vocimotor imagery.

4. *Visual-vocimotor-auditory* (VAM). The words were presented to the learner as in the purely visual presentation (V), and as soon as he perceived each word he was required to pronounce it aloud, definitely and distinctly.

5. *Auditory-manumotor* (Am). The same procedure as in the purely auditory presentation (A), except that the learner was required in addition to write the words. The learner was blindfolded, so that he could not see the word he had written.

6. *Visual-auditory-manumotor* (VAm). The procedure here is a combination of the purely visual (V) and the auditory-manumotor (Am) modes of presentation, except that the learner was not blindfolded. (See 7, VMm below.)

7. *Visual-vocimotor-manumotor* (VMm). The learner saw the word and was permitted to repeat it in vocimotor imagery but not aloud; and then wrote it. The *O*'s hand was under a screen, so that he could not see the word that he had written.

8. *Visual-auditory* (VA). The learner saw the word, and at the same time heard it pronounced by *E*. He was required to inhibit vocimotor imagery in the learning.

9. *Visual-vocimotor* (VM). The learner saw the word and was allowed to repeat it in vocimotor imagery but not aloud.

10. *Auditory-vocimotor-manumotor* (AMm). The procedure is the same as in the auditory-manumotor mode of presentation (Am), except that the learner was allowed the use of vocimotor imagery.

11. *Visual-manumotor* (Vm). The procedure is the same as in the visual-vocimotor-manumotor mode of presentation (VMm), except that the learner was asked not to employ vocimotor imagery in the learning.

12. *Visual-auditory-vocimotor-manumotor* (VAMm). The learner saw the word as it appeared in the aperture, pronounced it aloud definitely and distinctly, and then wrote it; he did not perceive visually the word that he had written.

In the visual presentation the list of words was typewritten on paper fastened to a revolving drum which presented one word every 3 sec. A space indicated the end of the list.

In the auditory presentation a 3-sec. interval was allowed at the end of the list before repetition of the list.

A list was re-presented until the *O* signalled that he had learned it. The *O* would then immediately recall the series to *E*, who would record the recall, the number of presentations required for learning, the time required for recall, and the number and the nature of the errors made.

We sought in this investigation both *a quantitative determination* of reproduction after a few seconds and after 24 hours, and *an introspective account* of the mental procedure in the learning and in both the immediate and delayed recalls. Thus we hoped to obtain a clearer insight into the act of learning itself, and to clear up some of the differences of experimental results that are to be found in the literature.

Seven graduate students in experimental psychology served as *O*'s. They were all highly trained in introspection. Four of them took part in the entire investigation.

Non-significant Materials

In the second half of the investigation we used 33 lists of nonsense-syllables. Twenty syllables constituted a list. The procedure was identical with the procedure of the first part with two exceptions. In the first place, the visual-auditory-manumotor presentation (VAm) was not employed because the learner's procedure was almost the same as with the visual-auditory-vocimotor-manumotor presentation (VAMm). In the second place, the *O* could see the word as he wrote it. An apparatus was arranged so that the syllable written by the learner was drawn out of sigh simultaneously with the coming-in of the new syllable. We employed for this purpose an endless band of paper which passed under an aperture cut in the table and was actuated by electrical contacts controlled by the exposure apparatus, a Marx machine. Thus we prevented continued visual stimulation.

Ideational Equipment of Observers

Observer P. *P* was of mixed type, with preference for vocimotor imagery, but he was able to use visual imagery readily. When vocimotor imagery was prohibited by the instructions, he reported that it was nevertheless present in most of the learning process, although under certain conditions it could be inhibited. His recalls were always in vocimotor imagery.

Observer C. *C* was of the mixed type, but dominantly visual. He used vocimotor-auditory imagery, especially when the material was difficult. In anticipating and recalling, he relied mostly on visual imagery, both of the printed word and in his own handwriting. *C* also employed a visual schema of the list of words frequently in learning and always in recall.

Observer Fe. *Fe* was of extreme motor type; in the entire investigation he reported only one or two visual images and these were subsequent upon vocimotor imagery. In no instance was he able to inhibit vocimotor imagery. He always recalled the material by means of vocimotor-auditory imagery or by vocimotor innervation. Frequently he employed a kinaesthetic schema of localization.

Observer T. *T* was of the mixed type, but preferred vocimotor imagery. He possessed, nevertheless, a good command of visual imagery and recalled many words in visual-verbal terms. When instructed to inhibit vocimotor imagery, he never succeeded; and he always anticipated the material in vocimotor terms. In cases of difficulty of recall, he employed either a visual or a kinaesthetic schema of localization. All the recalls, regardless of the mode of presentation, occurred mostly in vocimotor and vocimotor-auditory terms, accompanied often by visual-verbal or visual-concrete imagery or meaningful associations.

Observer Fi. *Fi* was of the balanced type; she habitually employed vocimotor (auditory) imagery alone in learning but she sometimes used visual imagery alone, especially under the *Aufgabe* to inhibit vocimotor processes. In these latter cases, however, the vocimotor processes were present during the first few presentations. She formed many meaningful associations during the learning. The syllables were generally recalled in vocimotor (auditory) imagery; but visual imagery was present in a few of the immediate recalls. She also employed a visual schema of localization. One olfactory image was reported by *Fi,* the only such image reported in the investigation.

Observer W. *W* was of the extreme visual type and in learning and recalling he employed a greater amount of visual imagery than of any other kind. He used vocimotor and vocimotor-auditory imagery to no great extent. Even when permitted vocimotor imagery, he employed it only in the first few presentations. He also employed a visual schema of localization. He found it impossible to inhibit vocimotor imagery in the first few presentations of a new material.

Observer S. *S* was of the mixed type with a slight preference for the vocimotor imagery. He was able to use visual imagery in learning, but it appeared only after the vocimotor imagery had been present in the first few presentations. In spite of the use of several contrivances, he found it impossible to inhibit vocimotor imagery under instruction. He employed a visual schema of localization.

RESULTS

Summary of Introspections[19]

A survey of our introspections brings to light the following points:

I. Vocimotor imagery can not be inhibited, at least not in the first part of the learning.

P, VA. " During the presentation I focused attention on your voice, adjusting my right ear toward you and holding the auditory stimulus as long as possible; I attempted to anticipate, but when I succeeded the words always appeared in vocimotor imagery."

T, V. " In the fore-period there were sensations of pressing my lips together, pressing the tongue against the teeth, kinaesthesis of strains in the muscles of the stomach, and general muscular inhibitions throughout the whole body. This tense state seemed to be an attempt to inhibit vocimotor imagery; but in spite of it all the first presentation of each syllable was followed by a vocimotor-auditory repetition of it."

C, A. " In the first part of the learning I found that if I attended focally to the perception of each word and then to auditory imagery of it afterwards, I could not help having vocimotor imagery of it; so in the first 5 presentations at least I had rapid vocimotor imagery of each syllable after it was presented."

W, VA. "As soon as the first word was presented I found myself repeating it in vocimotor imagery, then awareness of the *Aufgabe,* present in visual imagery of you. . . . When the next word was presented I found myself repeating it again in vocimotor imagery."

Fi, A. " In general I would repeat the syllable and the associated word in vocimotor-auditory imagery, and at the same time I was keenly aware of tensions about my tongue and throat, tensions which would increase at that moment. In some cases the vocimotor image was very slight."

S, VA. "As soon as you said, 'Inhibit vocimotor imagery,' I was conscious of a numb feeling in the vocal apparatus. In the first presentation I was aware of a vocimotor image of the word as soon as I received the visual percept. . . . This vocimotor image after the presentation of each word was present, I am certain, during the entire first presentation and occurred very many times during the second presentation."

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[19] In the following excerpts from the protocols, the letter at the beginning of the paragraph indicates the *O,* and the symbol following the mode of presentation: see p. 252.

II. Observers of motor type never wholly succeed in eliminating vocimotor imagery in the learning, even when instructed to do so.

T, Am. "After the presentation of each syllable I had vocimotor and auditory repetition of it throughout the whole learning; I prevented movement of lips and actual throat movement, but I could not repress a sensation of movement in the tongue, and auditory imagery often accompanied this vocimotor imagery."

Fe was never able to inhibit vocimotor imagery; it was present in all the learning with all materials.

III. With *O*'s of visual type, the words are anticipated in vocimotor imagery; gradually the words appear to consciousness more and more in visual and less and less in vocimotor imagery; finally the words are anticipated in visual imagery only and no vocimotor imagery of the words is present.

C, V. "In the first part of the learning I was unable to rule out vocimotor-auditory imagery; it immediately followed the visual perception of the word. But as the learning went on the amount of vocimotor imagery became less and less. In the last 6 or 7 presentations there was almost no vocimotor-auditory imagery; the words were anticipated almost wholly by means of visual imagery."

W, VAm. "Vocimotor imagery appeared very seldom after the first 3 or 4 presentations, the anticipation being done mostly in visual imagery."

IV. If *O*'s of visual type attempt to inhibit vocimotor imagery, visual imagery in the learning sometimes becomes less clear; in the recalls the visual image develops more slowly than it does when vocimotor imagery is not interfered with.

C, A. Delayed Recall. "All the syllables came in visual imagery but very slowly and in no definite order. . . . Only 4 or 5 of the syllables were definitely localized. Strains in my eyes and consciousness that the words were coming very slowly."

W, AMm. "None of the visual imagery was very clear. Accompanying the learning were great strains and unpleasantness; I could not give the words fast because they came in so slowly."

W, Am. *W* gave up his attempt to learn the series after it was presented to him 18 times; he said, "I could not learn this series because I was unable to visualize the words as this is the medium I use in learning."

V. In addition to the help obtained from meaningful associations, there is another aid which comes from the vocalization of the words and which we may call a 'motor or kinaesthetic help': on pronouncing the first word the *O* may find his vocal apparatus set to pronounce the following word.

T, VM. Delayed Recall. "The syllables before SEC came in very quickly, one after the other, in vocimotor imagery; that is, after pronouncing one word I would find my vocal apparatus set to pronounce the next, and this word was then immediately vocalized. As soon as I vocalized VIT I found my vocal apparatus about to pronounce CAY."

C, V. "As soon as I recalled POM I was conscious of eye-movement downward, and vocimotor and auditory imagery of POM; before I finished vocalizing POM the vocal apparatus was adjusted for the pronunciation of TER, and POM-TER were pronounced very quickly, one after the other."

Fe, A. Delayed Recall. "The vocal apparatus was set for the pronunciation of a hard *c*-sound. Then I found myself pronouncing COCO."

Fi, VAM. Delayed Recall. "WAB flashed in mechanically in vocimotor innervation; after the vocalization of VOQ (which immediately preceded WAB) my vocal apparatus formed for the vocalization of WAB."

S, Am. Delayed Recall. "After vocalizing TALL I was conscious of the adjustment of the vocal organs for the *ive*-sound; this was followed by a visual image of FIVE in my own hand-writing."

VI. If given the *Aufgabe* to inhibit vocimotor imagery, the *O* sometimes sets up for himself a new *Aufgabe* to use imagery of another modality.

P, VA. "I tried to anticipate, but when I did I used vocimotor imagery, so I stopped anticipating; then I set up the vocimotor-auditory *Aufgabe* to retain the words in visual concrete images."

T, VA. "I tried to anticipate, but I found myself using vocimotor imagery. . . . I set up the vocimotor and auditory *Aufgabe* to anticipate in visual imagery but I did not succeed; vocimotor imagery always coming in."

C, VA. "There was little tendency to use vocimotor imagery after the presentation of each word; I set up the vocimotor-auditory *Aufgabe* to anticipate in visual imagery; I just sat and looked as the words were being presented, attending very little to the auditory presentation."

W, VA. "I then found myself seeking for a way to fulfil the *Aufgabe* [to inhibit vocimotor imagery]; I had a visual image of a sheet of paper and myself; then a vocimotor and auditory, 'Attend to the visual image of the words; never mind the vocimotor image.'"

S, A. "I was using vocimotor imagery in learning these words; I then set up the vocimotor-auditory *Aufgabe* to substitute the exhaled breath for the vocalization of the word; the vocimotor imagery became less, and I had a second *Aufgabe* to use visual imagery after you spoke the word."

S, Am. "I set up the vocimotor-auditory *Aufgabe* to visualize the words in my own hand-writing; this I did, but I was not able to anticipate in this visual imagery."

VII. Any attempt to inhibit vocimotor imagery makes auditory imagery of the word more clear and intensive.

T, Vm. "Following the visual percept of the word there was a vocimotor-auditory image of it. The vocimotor image was much less intensive than the auditory. The auditory image of the word became much more intensive as I was able to suppress the vocimotor."

C, Vm. "Later on in the learning the vocimotor seemed to drop out and the auditory image seemed much louder; the vocimotor image almost completely dropped out in the last half of the learning."

Fi, VA. "I am quite sure that I inhibited vocimotor innervation of each word, but there was slight vocimotor imagery and very intensive auditory imagery."

VIII. In a visual-auditory presentation an *O* seldom attends to both the visual and the auditory stimuli; he attends either to the visual alone or to the auditory alone.

P, VAm. "During the presentation of the words I paid the least possible attention to your pronouncing . . . concentrating attention almost wholly upon the visual perception."

C, VA. "During the middle of the series I was not attending to your voice at all but to my own anticipatory processes and to my retaining, in visual imagery, of the last two words which I had just perceived visually."

Fe, VA. "My attention was directed much more to the auditory perception than to the visual perception of the word; in fact, once or twice I found that my eyes were actually closed, while I was perceiving the words in auditory fashion only."

W, VAm. "For 4 or 5 presentations I paid little attention to your voice; it was by no means in focal perception for at least 4 or 5 presentations; I attended mostly to the visual perception."

IX. In an auditory presentation all *O*'s had great difficulty in distinguishing syllables containing such letters as *d, t, q, c, k, etc.*

T, Am. "After you pronounced QAZ I wrote it KAZ; the *k* held my attention. Vocimotor-auditory imagery, 'That must be a *q* because there is a *k* later.'" *T* had to reason, therefore, as to the correct spelling of a syllable; the auditory perception made possible at least two spellings.

C, AMm. "During the first few presentations the auditory perception was not instantaneous; not definite or clear; that is, not so much the clearness of the sound but rather a lack of definiteness of the letters pronounced. For example, for one syllable I write it at least two ways: BER and BUR."

Fe, AMm. "In the first presentation several words presented were capable of two interpretations; *e.g.,* BARN-BARM. This caused nervousness."

W, A. "I had great difficulty with the syllables containing either the letters *c* and *q, d* and *b;* after perceiving a syllable containing such a letter there was confusion as to what the exact spelling was; this confusion consisted mostly in a fluctuation of the two possibilities in visual imagery."

Fi, AMm. "Sometimes I had difficulty in perceiving the word definitely; I would find my vocal apparatus set to pronounce but for no definite syllable; this setting of the vocal apparatus only appeared when I was not able definitely to perceive a syllable.

S, AM. "In 3 cases it was very difficult for me to get a distinct perception of the words, and I was not able to vocalize them easily; there were elements in my auditory perception of your voice in pronouncing the words which I was unable to reproduce."

X. If, in recalling a series which had been presented to the *O* in auditory fashion, auditory imagery is present, it is seldom of *E*'s but usually of *O*'s voice.

C, AMm. "The words were recalled in vocimotor-auditory imagery of my own voice; then a visual image of each word in my own handwriting."

Fe, A. "In the immediate recall I made use, as far as I am aware, only of vocimotor-auditory imagery; the auditory imagery was of my own voice."

W, AMm. Delayed Recall. (What was the nature of your auditory imagery? Was it of your voice or of my voice, or of any one else's voice?) "The auditory imagery when it appeared was always of my voice."

Fi, A. "I anticipated every syllable in auditory imagery of my own voice, perhaps a little vocimotor, sometimes 2 or 3 ahead of the actual presentation."

S, VA. Delayed Recall. "I gave the first 3 words from visual imagery, but the word SOIL (the second word of the three) was followed by auditory imagery of my own voice."

XI. Manumotor imagery does not help either in learning or in recalling a list of words or nonsense-syllables.

P, VAm. (Did any manumotor imagery appear in the learning or the recalling of this series?) "I had absolutely no manumotor imagery at all."

T, Am. "The immediate recall of this series was almost wholly in vocimotor-auditory imagery, the auditory being of my own voice; there were no visual and no manumotor imagery present."

C, Am. "I anticipated far ahead of the drum, not attending to your pronunciation until I failed to anticipate; there was no consciousness of any manumotor imagery whatever."

Fe, Vm. (Did any manumotor imagery enter into the learning or the recall of this series.) "No, none at all."

W, VAm. "In anticipating I always set up the *Aufgabe* to call up the words visually; if a visual image of the word failed to appear I attended to my arm; but no manumotor image of a syllable ever appeared."

Fi, Vm. Delayed Recall. "There was no consciousness of any manumotor imagery; in fact, there was no consciousness that I ever wrote the material."

S, Am. "Before I wrote a word I was never conscious of either a visual or a manumotor image of it."

XII. In recalling words between which associations have been established in the learning, the imagery of these words may appear very quickly, and the association is present only inasmuch as the words appear more quickly.

P, AMm. "The first word of an associated group would appear and the remaining words would come more readily with very little attention, one after the other."

T, AM. "All the syllables came very quickly, one after the other, but there was no consciousness of their meaningful connection; there seemed to be no meaning attached to the words excepting the fact that they came very quickly."

C, VA. Delayed Recall. "There was no visual concrete imagery with the word NUNS; *i.e.*, the visual concrete imagery that appeared to consciousness during the learning process; but as a result of the association made with NUNS and CAPE, the word CAPE came more quickly in auditory imagery after the word NUNS, than was the case where two succeeding words in the series were not associated."

Fe, Vm. Delayed Recall. "The meanings were present in the words with which associations had been formed in the learning, as far as I can see, only inasmuch as these words came together."

W, VMm. Delayed Recall. "The meaning which was present in the learning of the groups was not clearly present, the effect being that these words appeared very quickly, one after the other."

Fi, VA. Delayed Recall. "After a few presentations the associations did not come in with the syllables; the syllables themselves seemed to function just as the meaningful words would have done in carrying meaning."

XIII. When associations have been formed with the words of the series the following phenomenon often appears in the recall: The meaningful association comes first; the actual words of the series come later.

P, Vm. (Did any associations appear in the immediate recall?) "Yes. With the words DOTH-KLINK-GLOW the meaningful content came first and the words themselves later."

T, AM. Delayed Recall. "The feeling of familiarity was present with most syllables and the meaningful association came in before the nonsense-syllables."

C, VM. Delayed Recall. "I set up the vocimotor-auditory *Aufgabe*, 'Try to call up an association and see if the words will come;' immediately 'wigwam' flashed in in visual imagery, but the 'wig' faded out of consciousness very quickly and WAM was clear and distinct."

W, Vm. Delayed Recall. "The associations which I had in the learning came in before the words and were followed in each case by visual imagery of the word; the associations always came in in visual imagery."

Fi, VAM. Delayed Recall. "I had a vague visual image of some books; this meant for me language-books. Then came the vocimotor-auditory image 'Latin;' immediately QAH appeared in visual imagery."

S, VA. Delayed Recall. "In anticipating the series the concrete imagery came in first; *e.g.*, I had visual imagery of some soil and a consciousness, which I can not describe, that the next word was related to soil; following this I had a visual image of a tub of clams; immediately I had an auditory image of the word CLAM; the association immediately dropped out."

XIV. In adding an extraneous syllable to a syllable of the series,—as in the case of making the word 'Berlin' from the printed syllable BER,—a remarkable phenomenon occurs in that, while the intruding syllable may come to consciousness during the act of recall, the *O* never fails to recognize that it does not belong to the series. Although it is present as among the contents of his recalled consciousness of the series, in the recall he never reports the formed word but always reports the syllable which was presented to him.

T, Vm. "With the syllable JIT I formed the association 'jitney;' JIT was completed by a vocimotor 'ney,' very faint and less intensive than the vocimotor JIT."

C, VAMm. "With the syllable FEK I had the association 'Fechner;' that is, after the visual perception of FEK there was vocimotor imagery of it; then I wrote it; then I had visual perception of the word I had written with a visual image of NER after the FEK, with vocimotor-auditory imagery of 'Fechner;' the FEK was more clear and definite in the vocimotor-auditory image than the NER."

Fe, VM. "The syllable JOS was recalled in vocimotor-auditory imagery as 'Joseph,' with the emphasis on the JOS part."

W, VAMm. "I had several verbal associations present mostly in auditory imagery; for example, LIB='liberal;' the syllable LIB was more intensive than the rest; the latter syllables dropped out very early in the learning."

Fi, VMm. Delayed Recall. "After I had given XOL-JIT I had rapid vocimotor imagery of 'funny rector;' the nonsense component was emphasized and the completing part of the word was very indefinite." (The syllables were FUN and REC.)

XV. The size of the group which is formed by an *O* in the learning is sometimes determined by the immediate memory span of the *O*.

C, VM. (What determined the number of words which would constitute a group?) "The number of words which I was able to retain in the amount of time I had left after the presentation of one word and before the next one was presented determined in most cases the size of the group."

W, A. "I tried to retain the words in visual imagery in groups of 5, but in the interval between the presentations of the words I was not able to retain that number, so I set up the *Aufgabe* to divide them into groups of 3; this I did."

S, AM. "Between the presentations of the words I would repeat about 4 of the immediately preceding words; I could not carry any more in memory. Then I started off with the fifth one as the first of the second group, and so on, until I got 4. This grouping took place in the first presentation."

XVI. The size of the group of words formed by an *O* in the learning may be determined by the number of words that the *O* is able to anticipate in the interval between the presentations of two successive words.

P, A. "I began to combine the words, as many as I was able, between the presentations of two successive words."

C, VAm. "Some of the words were learned in pairs, because in joining them together I had time to go over only the word now being presented and the preceding one."

S, VAM. "In the first presentation I attempted to recall all the words I had passed up to about the fifth, as I found I was able to retain only this number of words. This threw the series into two groups of 5 each. This I did for 2 or 3 presentations."

XVII. The *O* may set up an *Aufgabe* to divide the series into definite groups. This course makes the size of the groups arbitrary.

P, VMm. "I set out with the vocimotor-auditory *Aufgabe* to group the words in pairs."

T, AM. (Why was it that your groups contained 4 syllables?) "In the beginning I set out with the *Aufgabe* to group the syllables in fours; during the presentation of the series I just found myself doing it."

C, AM. "After I passed the first 4 words I held them in visual imagery and in vocimotor-auditory imagery; this was the result of an *Aufgabe* set up in the foreperiod of the learning."

Fe, VMm. "The series were grouped in 5 groups of 4 each in the order in which they occurred." This grouping seems to be the result of an *Einstellung.*

W, VAMm. "After you had given me the signal 'Ready, now,' I had vocimotor-auditory imagery of 'Get them by fives right off.' Then I had a very faint visual image of an entire sheet of paper with a schema of 4 groups."

Fi, VM. "The words were divided into groups of 4." As this grouping took place in the first presentation, it seems evident that it was the result of an *Einstellung* carrying over from experiences in other investigaitons in which lists of nonsense material were used.

XVIII. During the act of anticipating, and later in recalling a series, a visual schema of the series is sometimes present, especially for *O*'s of the visual type.

T, Vm. Delayed Recall. "A visual schema of a vertical column in front of me appeared in consciousness; the schema became clear as there was eye-movement from the bottom up. The upper part remained very focal, especially the third place from the top. Then MIH came clearly in visual imagery (printed)."

C, VAMm. Delayed Recall. "Each word came first in vocimotor imagery, followed by auditory and then by visual imagery; the visual image of the word appeared in a visualized column, and as visual attention moved up the column each word came in clearly."

W, A. "TINT-ANAL came in in visual imagery on a visualized sheet of paper in front of me, one word below the other."

Fi, Am. Delayed Recall. "I had a vague visual schema in which the first 4 syllables stood out more clearly than the rest."

S, A. Delayed Recall. " The first word came on the top of a visualized sheet of paper, the visual image being of the mimetic sort (∫) and accompanied by vocimotor and auditory imagery of 'ent.' Immediately DENT was vocalized."

XIX. During the act of anticipating, and later in recalling a series, a kinaesthetic schema of the series of words or syllables is sometimes present, especially with *O*'s of the motor type.

P, AM. " Sometimes I stopped anticipating between the pairs and attempted to get a clear percept of each word, localizing them on my fingers; sometimes I did this for two successive presentations."

T, AM. " The immediate recall was as usual in vocimotor imagery with kinaesthetic imagery of tapping in the tempo with which each syllable was presented in the learning."

Fe, VAMm. " There was also present my localizing kinaesthetic schema, present in kinaesthetic imagery of pointing from left to right with my right hand, with eye-fixation at the place where I was pointing. In this process the eye-fixation was much more focal and intensive than the kinaesthetic imagery of pointing."

W, VAM. " Eye-kinaesthesis. I had a visual image of a sheet of paper; then unpleasantness and a sudden shift of visual attention to the bottom of the paper, which meant to me that the words which I had just now recalled did not belong at the top of the list."

Fi, AM. Delayed Recall. " For the first time in the recall I was aware of a vague visual schema which was different from the one I generally use; that is, I was aware of certain regions in the air in front of me, more of a spatial reference to something that was not filled in. This was followed by a kinaesthesis in the neck and eyes of turning slightly to one side in order to fixate this region; perhaps there was actual eye-movement. . . . Then DOY came in in visual imagery."

S, A. " I was conscious of eye-movement up and down this visual schema; the lower part of the schema was focal and the first 3 words were anticipated in visual imagery."

XX. Words which are not recalled promptly are generally preceded, when they are recalled, by a schema which mediates their advent into consciousness.

P, VMm. " When a word did not appear I would focus attention on the finger with which that word was associated; and the word when it appeared would come slowly in vocimotor and auditory imagery."

T, Am. Delayed Recall. "Attention as focused on the bottom of a visual image of a vertical list. Eye-movement up several times, which meant to me 'Start at the bottom and go up.' While fixating this point, QIW finally came in in vocimotor imagery, but there was no visual imagery."

C, A. Delayed Recall. " Before I recalled KITE and SOFA, I had a visual blank in which there was room for 2 words; then vocimotor-auditory imagery, 'Two words associated together;' then both words appeared in visual imagery, localized on this visual schema, one over the other."

Fi, A. "There was a hesitation in which no words came; great tensions in the body; there had been gradually developing a visual schema in which I saw the series in a grayish outline extending toward the south-west from the floor."

XXI. If the word is recalled in imagery of the modality corresponding to *O*'s type and is not clear, it is often clarified by a coming in of imagery of a modality corresponding to the mode of presentation.

T, VA. Delayed Recall. "DEJ appeared in vocimotor-auditory imagery, followed by a visual image, localized to the left of the aperture; the *d* was especially clear."

C, A. Delayed Recall. "In recalling DEAD, DEA appeared first in visual imagery, and this was completed by an auditory image forming the complete word DEAD."

W, Am. Delayed Recall. "CARD came in in visual imagery, localized at the top of the visual schema; not more than the *AR* and a very faint *d* were present. Immediately I had auditory imagery of the whole word, with emphasis upon the *c;* I repeated the whole word to myself in vocimotor-auditory terms several times."

S, VAMm. Delayed Recall. "I had a faint vocimotor-auditory image of TOLA. This was followed by a very clear visual image of it in print."

XXII. The recalled word may appear first in the imagery corresponding to the mode of presentation and may be completed or clarified by the coming in of imagery corresponding to *O*'s imaginal type.

T, AM. "After a long pause I had a clear visual image of the letter *a;* then a vocimotor image of FAZ; then I vocalized it."

C, AM. Delayed Recall. "Very often I had a visual image of the first part of the word which would be completed by a very clear distinct visual image of the whole word."

W, A. "XUT appeared first in indistinct auditory imagery, I think of your voice; this was completed by a visual image of the word on a sheet of paper in front of me; the word appeared in type."

Fi, Vm. Delayed Recall. "All the words came in. There was perhaps a very vague visual image first, in printed type, accompanied by a distinct vocimotor-auditory image."

S, VA. "Most of the syllables were usually completed or filled out by a vocimotor image, although the syllable first appeared in a visual image."

XXIII. Visual imagery is not suited for a rapid anticipation or recall. If the *O* recalls very rapidly, it is found that the visual imagery does not develop quickly enough, and that for this reason the *O* often has recourse to vocimotor or auditory imagery or to both.

C, VAM. "I began anticipating the second presentation in auditory imagery, then in visual imagery. About the middle of the presentation I started rapid anticipation and this was done in vocimotor-auditory imagery, the visual imagery not coming in at all; I anticipated so rapidly that it seemed there was not time for it to develop."

W, VMm. "In the immediate recall I was surprised to find myself repeating the first 5 words in vocimotor imagery with no visual; I gave them very rapidly from the vocimotor image and only one vague visual image (of the word WAGE) came in."

Fi, V. "I then attempted to visualize the words, but the attempt always failed me and I abandoned it. The auditory-vocimotor anticipation was much more rapid than the visual."

XXIV. The mode of presentation does not determine the modality of the imagery which the learner will employ in learning a given material.

P, A. Delayed Recall. "The first pair of words that appeared was ERGO-VAMP; they appeared in vocimotor imagery while pressing the fourth finger."

T, V. "The immediate recall of this series was mostly in actual speech, many of the words being preceded by vocimotor-auditory imagery."

C, AM. "Most all the words appeared in visual imagery, followed very often by vocimotor-auditory imagery; the words were localized on my visual schema."

Fe, V. "CALF, SOFT, HUMP, URNS and BARK all came, one at a time, in vocimotor-auditory imagery, most of the auditory being of my own voice, but the vocimotor image seemed to be more intensive and clear."

W, AMm. Delayed Recall. "The first 7 words came in visual imagery; no vocimotor or auditory imagery was present."

Fi, V. "The immediate recall came in vocimotor imagery; no visual imagery."

S, A. Delayed Recall. "This recall was made from a visual image of the list about the size used in the presentation; I started at the bottom of the list and went up. HASH came in vocimotor imagery; then in visual imagery. MILT-CUBE came in visual and vocimotor imagery. Which came first I can not say."

XXV. If the *O* fails in his attempt to anticipate a forthcoming word, he sometimes attends to the preceding words which he had just anticipated, and then he waits for the desired word to come into consciousness.

P, VM. "After a few presentations I was able to anticipate the first pair and also the last 5 pairs. After this I attended wholly to the words which I was not able to anticipate. As soon as I perceived them I repeated them over and over until the next word was presented. . . . During this time I was neglecting the last 5 pairs which I had anticipated early in the learning. Later on, when I perceived these words focally as they were being presented, they appeared as new words."

T, VAM. "After I was able to anticipate the syllables I would hardly look at them . . . as if I were anxious to get to the place where I could not anticipate."

C, VMm. "In the third stage of the learning I attended to the drum only when I was not able to anticipate a syllable or when I was not sure when the word which I had anticipated was correct."

Fe, V. "Toward the end of the learning I had visual perception only of those words which I was unable to anticipate."

W, A. "Toward the end of the learning process I did not attend to your presentation of the words until I was able to anticipate. In the last presentation I anticipated all the syllables."

Fi, VAM. "The syllables were anticipated mostly in vocimotor-auditory imagery; next I would look at the drum and, if I had anticipated correctly, there would be a pronunciation of the syllable and then a turning away of my visual line of regard from the drum; then I would anticipate the next word."

S, AM. "In the third presentation I actually anticipated every syllable in the series; this anticipation was made with but little attention to you; I attended only to my own anticipatory processes."

XXVI. If an *O* is unable to anticipate a word he often repeats the preceding words until the desired word appears.

P, VA. "All the words came easily up to the seventh pair; then a blank; that is, no imagery came; then I had imagery of the next pair, so I gave it; I then went back to the sixth pair and repeated it in vocimotor-auditory terms; immediately the second pair followed right on."

T, VAM. Delayed Recall. "PIW was repeated several times, mostly in auditory imagery. Immediately CUG-QAR came in vocimotor imagery, no schema being present."

C, VAM. Delayed Recall. "The first 8 words appeared first as very definite and intensive auditory imagery, followed by vocimotor imagery; then I had a visual image of a blank, to which I attended. Finally NEXT appeared in vocimotor imagery."

Fe, AMm. Delayed Recall. "'Crop' came very easily in vocimotor-auditory imagery; auditory imagery of my own voice; then no imagery came but there was great unpleasantness and strains. 'CROP, —un, un' was repeated several times in vocimotor imagery; finally a vocimotor, 'Gee, don't know these.'"

W, VM. "When the words did not appear promptly in the immediate recall there was a period during which there was no imaginal content in consciousness; then my eye moved up and down that part of the visual schema to which the desired word belonged; usually visual imagery of the words preceding and following the desired words would come in; then suddenly these words would appear in visual imagery in their correct place."

Fi, A. Delayed Recall. "I had a visual schema in which was a blank space for about 2 words; I attended to the schema for awhile, conscious of strains in the whole body; then the syllable NAF came in vocimotor-auditory imagery and the remaining words of the series followed."

S, VAM. "I had a visual image of my schema with a visual image of IDOL at the top of the list; then blank spaces for about 3 words below which meant to me that there were words left out; then vocimotor-auditory *Aufgabe,* 'Try your vocimotor; you can get it that way;' then I had vocimotor-auditory imagery of IDOL several times and the remaining words came in finally."

XXVII. If an *O* comes to a part of the series which he is unable to anticipate, he very often stops his attempted anticipation until the word desired is presented to him in the ordinary course of the presentation of the series; he perceives this word, if the presentation is visual, or calls up imagery of it, if the presentation is auditory, until the next word is presented.

C, VMm. "In the next presentation I started to anticipate; in the middle of the series I had great difficulty; when they were presented I looked at them very hard and wrote them down, and looked at the word I had written for a long time."

W, V. "In the last presentation I adopted the same procedure as formerly, anticipating ahead of the actual presentation. When I could not anticipate I had recourse to the retention method; that is, as soon as the desired word was presented it was held in consciousness for a long time."

XXVIII. When an *O* could not recall a word, he sometimes set up the *Aufgabe* to go through the alphabet in an attempt to get the word desired; *i.e.,* commencing with *a,* then *b, etc.,* he would pronounce each letter or call up a visual image of it, excepting that, when he came to the letter which was the initial letter of the desired word, the word itself would come to consciousness.

T, VAMm. "After I had given CEJ there was a blank; visual schema came in and the last 3 syllables stood out focally and I gave them to you; the fourth last space became focal but no syllable appeared; then I set up the vocimotor-auditory *Aufgabe* to go through the alphabet; when I came to M, MUN came in vocimotor-auditory imagery; with this there was also a visual image of MUV, with the V much more clear than the MU."

C, Am. Delayed Recall. "JOQ-HAJ was recalled in a sort of a logical procedure. A vocimotor-auditory *Aufgabe,* 'Go through the alphabet.' . . . When I came to *j* I had vocimotor-auditory imagery, 'Some syllable had a *j* in it;' then a visual image of a *j,* accompanied by pleasant affective toning, and HOJ came in in visual imagery.

XXIX. In the learning, some of the words formed 'corner stones' on which the remainder of the series was learned.

W, VA. "My procedure was to retain in visual imagery the first and last of each group of 5, and then to add one word each time in each presentation."

W, Vm. "I spent the rest of this presentation in attending to the first and last word of each group; that is, looking at them more attentively and keeping them longer in visual imagery than I did the rest of the group. In the next presentation these words so retained were always recognized as the first and last word of a group. . . . Later I commenced to attend more closely to the middle of each group. Then my procedure changed to one of anticipation; I could anticipate the first and last of each 5, then the middle. The rest of the procedure consisted in anticipating the remaining words of each group."

The Process of Learning

The process of learning was found to consist of three distinct stages; and these three stages recurred throughout the experiments, whatever the mode of presentation, or the nature of the materials to be learned, or the ideational type (or learning-type) of the learner.[20]

I. *The Orienting Stage.* This initial stage of the process of learning extends over only the first few presentations of any given series to be learned. Whether it is confined to the first presentation alone, or whether it extends over several presentations, depends less upon the ideational type of the individual than upon the nature of the material itself (*i.e.,* whether meaningful words or nonsense-syllables) and the nature of the series (*i.e.,* whether difficult or easy for the particular individual).

During the presentation of the material in this orienting stage, the attitude of the learner is one of passive receptivity. He makes no attempt either to group or to anticipate the words which are being presented; he merely perceives them. There are rare exceptions. Occasionally an *O* manifested an incipient tendency during this initial stage to group the materials into larger units, a tendency apparently consequent upon his having set up for himself during the foreperiod an *Aufgabe* to divide the list of material into groups of definite size. *W* sought groups of five and *Fe* groups of four.

If the mode of presentation is a combined visual-auditory one, the learner in most cases attends focally to either the auditory or the visual stimulus alone, and very seldom to both equally. A feeling of familiarity for the series tends to develop during this initial stage, and marks the progress of the learner's orientation in his task.

It appears that the perception of the words, no matter how they may be presented, is invariably followed, at least in the first few presentations, by a reflex reproduction of them by the learner. This reproduction occurs in vocimotor or in vocimotor-auditory terms. Even the instruction to inhibit vocimotor imagery fails to prevent the vocimotor reproductions, at least during the first few presentations of the list.[21]

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[20] Meumann, *Vorlesungen,* II, divides the learning process into four stages: (1) the orientation or adaptation stage; (2) the stage of passive recptivity; (3) the stage in which the material is tentatively recited or checked by the learner; (4) the stage in which the learner finally fixes the uncertain parts of the series. The first and second stages of learning as given by Meumann differ but little, if at all, and therefore can easily be combined into one stage. If this is done our classification agrees exactly with that of Meumann.

[21] Secor, W. B. (Visual Reading, A study in mental imagery, *Amer. J. Psychol.,* 11, 1900, 225-236), in his experiments on reading, reported that vocimotor movements could be inhibited. Curtis, H. S. (Automatic movements of the larynx, *ibid.,* 11, 1900, 237-239), using a different type of laryngograph, obtained graphic records of vocimotor movements when his *O*'s were mentally reciting a poem or a selection of prose. If Curtis' results are to be taken to mean that vocimotor movement was present in all cases, it is probable that Secor's learners employed vocimotor imagery, if not the actual vocimotor innervation. Abbott (On the analysis of memory consciousness in orthography,

When a learner of the visual type is fairly successful in inhibiting vocimotor processes, his visual imagery is usually less clear and definite than when the vocimotor processes are allowed to function in natural fashion. When the learner of the motor type attempts to inhibit the vocimotor processes, auditory imagery frequently makes its appearance and plays a more important rôle than when the vocimotor processes are not interfered with. Auditory imagery may thus be enhanced even when the learner does not succeed in completely inhibiting his vocimotor processes.[22]

In rare instances learners succeeded in almost wholly eliminating the vocimotor processes from the very start of the series; but in every such case they found that it was impossible completely to memorize the materials presented.[23] Even though the list were presented a great many times (in one instance 39 times), the process of memorizing still remained far from complete, and the learner eventually always gave up his attempt to memorize the material. A subsequent attempt to recall the list would show that not more than 5 or 6 words out of a list of 20 had been memorized, and that these words the *O* recalled with but a slight degree of subjective assurance and with no definite consciousness as to their exact position in the list. Every attempt to complete the act of memorizing without the participation of the vocimotor processes thus ended in failure and the abandonment of the effort to learn on account of fatigue or lack of time. Learners of the motor type were especially unsuccessful.

With the *O* of visual type, the vocimotor image, though essential at first, tends to disappear very early in the learning process. It is seldom present after the fifth or sixth presentation of the series. This rule holds even when the learner is not instructed to inhibit vocimotor imagery. We hold, consequently, that, whatever the imaginal type of the learner may be, vocimotor imagery or vocimotor innervation is absolutely necessary for an individual to begin to learn a series

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Psychol. Rev. Mon. Sup., 11, 1909, 127-158) found that vocimotor processes were always an aid, and Smedley (*op. cit.*), concluded that it was impossible to test a single sense-modality because vocimotor imagery could not be repressed. The results obtained by Mould, Treadwell and Washburn (The influence of suppressing articulation on the favorable effects of distributing repetitions, *Amer. J. Psychol.*, 26, 1915, 286-288) show that the recall is twice as efficient when the *O* is allowed to use vocimotor imagery in the learning as when he is told not to use it, and accordingly attempts to inhibit it. The statistical data of these investigations indicate that vocimotor imagery was never wholly eliminated, and that the repression of vocimotor imagery was always a great distraction to the learners.

[22] This phenomenon has also been reported by Abbott and Secor, *opp. citt.*

[23] Kline, L. W. (A study in the psychology of spelling, *J. Educ. Psychol.*, 3, 1912, 381-400) found that any interference with the dominant receptor mechanism results in a greater impairment of the learning process than does an interference with the preferred form of expression. Müller and Schumann (Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Zsch. f. Psychol.*, 6, 1893, 81-190, 175-339) found that the repression of rhythmic vocalization renders learning almost impossible for some *O*'s. Cohn (*op. cit.*) also found that vocimotor processes were important to the learning and that learning was less efficient when the learner attempted to inhibit vocimotor imagery.

of words or nonsense-syllables. In these early stages it can rarely be eliminated by instruction, and its elimination, when it does occur, prevents learning.

When the *O* is given the *Aufgabe* to inhibit vocimotor imagery, he reacts in either of two ways. (1) He may accept the *Aufgabe* and actively attempt to inhibit it; if he is successful to a great degree, very little learning, if any, takes place. (2) He may comply with the instructions by setting up for himself a new *Aufgabe,* usually in vocimotor terms, to use imagery of another modality; *e.g.,* 'use visual imagery.' This latter procedure is more efficient than the first, but is effective only for *O*'s of the visual type and after the first few presentations.

When the *Aufgabe* required that the *O* pronounce the material aloud, vocalization often proved a hindrance to the learning. The chief fault of vocalization lies in the fact that it fills up so much of the time-interval (3 secs.) between the presentation of successive syllables that little time is allowed for the learner to anticipate the next syllable, and such anticipation is the learner's method of testing his knowledge of the series. As a matter of fact vocalization of the material, when required, becomes less intensive in the later stages of the learning.

II. *The Stage of Attempted Anticipation.* In the second stage of the learning process the *O* is very active in his attempts to anticipate the forthcoming members of the series. The power to anticipate the series becomes in every case the *O*'s criterion that he has learned the material.

The anticipation takes place either in visual or vocimotor (auditory) terms, depending upon the learner's type. In this stage the learner actually succeeds, however, in anticipating only a very few words, for he is concerned chiefly in obtaining a clear perception of each word during presentation and in attempting to combine the word at hand with the preceding and subsequent words.

In rapid anticipation the learner of visual type often makes use of vocimotor-auditory imagery, reporting that visual imagery does not develop quickly enough and that he therefore finds himself using vocimotor imagery.[24]

The first two or three and the last two or three words of a series are usually the first to be anticipated. There is no definite order in which the remaining words begin to be anticipated. Only one of our 7 *O*'s (*S*) did not invariably resort to grouping of the words, and even he occasionally used this expedient, although by no means so frequently as the other *O*'s who always grouped the words.

The number of words or syllables which constitute a group is determined in one of five ways. (1) *The time intervening between the presentation of two successive words.* As soon as a word was presented, the learner would repeat it, and then repeat as many of the preceding words as possible before the following word was presented. The maximum number of words that he was able to repeat in this time would constitute a group. (2) *The immediate memory-span of the O.* As many words as an *O* was able to repeat to himself from immediate memory without confusing their order or forgetting a word would constitute a group. If he attempted to add another word to such a group, he would be unable to recall one or more of the words, and this failure would mean to him that he had exceeded his immediate memory-span. (3) *A meaningful association.* If a certain number of words form a meaningful association, then this number of words may form a group. (4) *The Aufgabe.* In the fore-period, or during the learning, the *O* may set up an *Aufgabe* to group in fours or fives, thus forming a purely arbitrary group. The size of the groups chosen is frequently determined by the *O*'s experience, either during the experiment or in previous experiments, by which he knows what size of group is the most efficient for him to work with in learning a series of words. (5) *Difficult words.* A difficult word often marked the beginning of a group; if a learner had especial difficulty either in perceiving a word or in anticipating it, this difficult word would become the initial member of a group. The next difficult word would be the first word of the next group; thus the number of words which would constitute such a group varied.

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[24] Binet and Henri (La mémoire des mots, *Année psychol.,* 1, 1894, 1-23) report material recalled in vocimotor-auditory terms, because recall was so rapid that a visual image did not have time to develop. Von Sybel (*op. cit.*) found that when an *O* of visual type became fatigued he had recourse to vocimotor imagery. Von Sybel also found that the visual learner again employed vocimotor imagery when the presentation of the material was very rapid. Pohlmann (*op. cit.*) observed that an *O* of visual type is hampered in his learning by a too rapid auditory presentation.

A group formed through the medium of a meaningful association is least apt to be forgotten in the recall. An entire group thus formed may fail to appear to consciousness in the recall; but, if the first word of the group can be recalled by the *O,* the others follow very quickly.

Meaningful associations are present in the first few presentations only and then drop out, seldom to reappear in the learning. This rule holds no matter in what modality of imagery the association appears. The effect of such association is manifest in anticipation and recall, for the meaningfully associated words come into consciousness more quickly, one after the other, than the other words.

The temporal relations between the appearance of the words of the series and their meaningful associations are as follows. (1) The words to be recalled come into consciousness quickly and clearly in the imagery of the *O*'s type; the meaning is present in the words themselves and in the fact that they come together. The *O* was unable to find any other introspective evidence to explain the meaning. (2) The meaningful content comes into consciousness in imagery of any modality; the words of the series come later and very quickly, one after the other, in the imagery of the *O*'s type. If the association is present in a visual-concrete or an auditory-concrete image, it is followed by verbal imagery of the desired word in the imagery of the *O*'s type. (3) The words themselves come first in the imagery of the *O*'s type, and the meaningful content comes later, either in verbal or concrete imagery of any modality. (4) If a meaning was attached to a syllable by adding one or two syllables to the presented syllable, thereby making it a meaningful word, the *O* 'knows' what part of the made-word is the syllable desired by the fact that the desired syllable is more clear and definite, if a visual image, and more intensive and distinct, if the image is motor or auditory or auditory-motor, than the added or associated part.

III. *Anticipatory Stage.* In this final stage of the process of learning, the *O* is concerned chiefly in anticipating the syllables. He may anticipate as rapidly as he is able with no reference to the words which are being presented, or he may anticipate one word at a time just before it is presented to him. As soon as the word is presented he perceives it focally, and again anticipates the next word just before it is presented to him. Vocimotor imagery may or may not be present if the *O* is of visual type; if he is of motor type he repeats this stimulus-word several times and then links it with the preceding or subsequent words, or with both.

In the case of an *O* whose anticipation of words or syllables is far in advance of their actual presentation, an interesting phenomenon is seen when the *O* comes to a point in the series where anticipation is impossible. Then the process of anticipating ceases until, in the actual presentation, that word is reached which he failed to anticipate.

This procedure of anticipating and linking up the words which he can not anticipate, a learner continues until all the words have been anticipated at least once. In many instances, after correctly anticipating the series once, he will set up the *Aufgabe,* 'Go through and anticipate the series once more, to make certain;' and then, having again anticipated successfully, he will signify that he has learned the series.

When the material is presented to the learner in a purely auditory fashion, he very often finds it difficult to obtain a definite percept of words or syllables containing such letters as *c, k, d, q,* and *t* occurring either as initial or final letters, and *w* and *h* occurring as final letters in a syllable.[25]

Some *O*'s, as soon as they perceive the auditory stimulus, decide upon a definite spelling of the word, especially in the latter part of the investigation. In this way the uncertainty as to the correct spelling of the word, if it enters at all, is very slight. In most instances such words are not spelled by the *O*'s in the form in which *E* had them in his lists.

Greater attention, it appears, may be secured by material presented in auditory fashion than by material presented visually. This difference is due to the nature of the auditory presentation itself. Individuals find it more difficult to obtain a definite auditory perception than a definite visual perception, especially with nonsense material, hence greater attention is required when the material is presented in auditory fashion. Nevertheless in spite of increased attention auditory presentation does not increase the efficiency of learning. To a large extent this heightened attention is aroused by the inherent indistinctness of the auditory percept, and the *O*'s alertness is expended in decisions about the material and not in further impression of the material.

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[25] Henmon, V. A. C. (The relation between mode of presentation and retention, *Psychol. Rev.,* 19, 1912, 79-96) found that material was learned more efficiently when the words were presented in purely auditory fashion, but he excluded all syllables ending in *c, q* and *h,* and all syllables beginning with *x.* The excluding of words containing these letters from his material did away with the main difficulty connected with the auditory mode of presentation. Henmon claims he was investigating the relative efficiency of the different modes of presentation, but by omitting words containing such letters as *c, q, k,* he eliminated one of the difficulties necessarily connected with the auditory mode of presentation. The auditory mode of presentation, therefore, was given an advantage which was not given to the other modes of presentation, since many letters like *e, a, b, l,* look alike in the visual presentation, and may give rise to an indefinite visual perception.

Pohlmann (*op. cit.*) found that auditory presentation is better than visual for young children. This result can be explained by the fact that children are more familiar with spoken language than with written language. Pohlmann also found that auditory presentation is not efficient for the learning of nonsense-syllables because the learner is often uncertain as to what the exact sound is.

Abbott (*op. cit.*) also confirms our findings, for her *O*'s recognized only about one-half of the words when they were pronounced to them.

In both the immediate and delayed recalls the material is recalled always as individual words, though the words may have been grouped in the learning. Words which were grouped in the learning came to mind more quickly in the recall one after the other and with a slightly longer pause after the last one, than do words which were not grouped. Those *O*'s who employ a visual schema often in grouping visualize a part of this schema, equal to that which the number of words in the group would require if they were printed in the same fashion as the material used in the visual presentation. The words themselves then come to consciousness, one at a time, usually in visual imagery. Words not grouped in the learning come to consciousness in the recalls, one at a time, but much more slowly than the grouped words.

In many cases an *O* is subjectively certain that the recalled material is correct, but the structure of this subjective assurance is not the same for all *O*'s.[26] The following items, arranged in order of importance, may contribute to this state of consciousness, although not more than two or three of them need be present at any one time. (1) The *O* after recalling a word was able to attempt the recall of the subsequent word without the first word reappearing during the attempted recall of the second word; (2) the imagery of recall comes rapidly to consciousness; (3) the recalled words are pronounced with positiveness; (4) the affective tone is pleasant.

When the words do not come to consciousness quickly either in the anticipating or in the recall, all *O*'s, regardless of their imaginal type, usually recall the words in vocimotor imagery.[27]

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[26] Finzi (*op. cit.*) noted that subjective assurance depends upon the distinctness of the memory image; the more distinct the image the more convinced is one of its fidelity. He also found that certain organic sensations are present when the learner is subjectively certain that his recall is correct. Frankfurther and Thiele (*op. cit.*) pointed out that an important requisite for this state is the presence of a spatial schema for localizing the material. Kühn, A. (Ueber Einprägung durch Lesen und durch Rezitieren, *Zsch. f. Psychol.,* 68, 1914, 396-482) noted the following factors as making up the consciousness of subjective assurance: (1) clearness of the visual image, (2) number of helps, and (3) smoothness of recall. Meyer (Bereitschaft und Wiedererkennen, *Zsch. f. Psychol.,* 70, 1914, 161-211) emphasized two factors: (1) quickness of reaction time and (2) definiteness of localization. Pederson (Experimentelle Untersuchung der visuellen und akustischen Erinnerungsbilder, angestellt an Schulkindern, *Arch. f. d. ges. Psychol.,* 4, 1905, 520-534) held that (1) a good perception of the material and (2) a highly concentrated attention were the requisites for a consciousness of subjective assurance.

[27] Von Sybel (*op. cit.*) also found this to be the case. He reported that his learners used more vocimotor imagery and less visual imagery when the series was difficult to acquire.

A schema of localization is usually employed by *O*'s in the recall, especially when the material to be recalled does not come to consciousness quickly. There are three types of schemata: (1) a visual schema; (2) a kinaesthetic schema; (3) a rhythmic schema.

(1) *Visual Schema.* This schema is employed mostly by *O*'s of the visual type,[28] and to a less extent by some *O*'s of the motor type. It consists of a visual image of a sheet of paper with words printed on it. It is usually localized directly in front of the *O* with the words appearing in a vertical column, the first word at the top. In one instance this schema appeared in a horizontal plane with the words running from left to right. When a word does not come to consciousness quickly, the schema appears first. The part of the schema to which the word belongs is most focal, and the rest of the schema is present in a very indefinite non-focal fashion. The word desired then appears in consciousness in visual imagery, usually localized in its proper place in the schema. Some *O*'s are conscious of eye-movement up and down the series localizing the words in the visual schema.

(2) *Kinaesthetic Schema.* This schema is used by the *O*'s of the motor type who employ very little visual imagery. It consists of a kinaesthetic movement or imagery of movement of the hand or head pointing to that part of the series to which the particular word belongs. The extreme left of the *O* represents the first word of the series and the extreme right the last of the series. No visual imagery is present, although there is the kinaesthesis of eye-movement accompanying the manumotor or arm-motor imagery of pointing.

(3) *Rhythmic Schema.* This schema is used by *O*'s of all types, and consists of a vocimotor consciousness of rhythmic sounds. In some instances no definite words or syllables are present.[29] The rhythm, up to the point where the word fails to appear, is repeated by the *O* until the required word is obtained or until the learner gives up his attempt to recall the word.

When the words do not appear in consciousness some *O*'s often set up an *Aufgabe,* usually in vocimotor-auditory terms, to go through the alphabet; *i.e.,* start with *a* and pronounce each letter, expecting that when the correct initial letter is pronounced the word itself will come to consciousness.

The clearness and definiteness with which any schema appears in consciousness is in direct relationship to the difficulty with which the words appeared. If the words come to consciousness after a short pause, the schema, if it appears at all, is non-focal; if the word or group does not appear until a relatively long time has elapsed, the schema is focal and definite in consciousness. Since the schema is more focally present when the recall of words is more difficult, it is therefore more focal in delayed recall than in immediate recall.

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[28] Kuhlmann, F. (On the analysis of the memory consciousness, *Psychol. Rev.,* 13, 1906, 316-348) reports that the schema of localization generally precedes the recalling of the material.

[29] Müller (*Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, Zsch. f. Psych.,* Ergbd. V., 1911, xiv+403) emphasized the importance of rhythm, especially in the first stages of the learning. Müller and Schumann (*op. cit.*) also found that rhythm was a very important factor in learning inasmuch as the syllables which had once formed a part of a metrical foot tended to be associated more closely than syllables not bound together in this way.

The *O* is apt to be uncertain in recall when a word is recalled with difficulty.[30] Two or more of the following factors, listed in order of their importance, usually constitute this consciousness of uncertainty. (1) The first syllable in an associated pair keeps repeating itself; even after the recall of the second syllable the first continues to recur in consciousness. (2) Images of the two syllables alternate or rival in consciousness. (3) A word fluctuates in its position in the schema of localization. There is (4) hesitancy in vocalization, (5) a questioning intonation in vocalization, (6) an unpleasant affective tone.

If in the learning an *O* forms a group which is recalled in motor terms, a motor trend sometimes appears as an aid in recall: as soon as a learner vocalizes one word of a group he 'finds' his vocal apparatus automatically set to say the next word. The group thus becomes a 'motor unit' which runs its course automatically once it is initiated.

In anticipating and recalling the words of a series, the visual imagery of a word may appear (1) typewritten, (2) in the *O*'s own handwriting, or (3) in a form that can not be recognized as any specific writing or printing. There is no conclusive evidence that explains the occurrence of one of these forms rather than another. Most of the visual imagery is of the typewritten form and is derived doubtless from the presented material. It is when the *O* is required to write the words in the learning or in the recall that he has many visual images of his own handwriting. Especially does he seem to visualize his handwriting if he has a characteristic way of forming certain letters.

The *O* of visual type, when presented with material in either auditory or visual fashion, always recalls in visual imagery.[31] The *O* of motor type, when presented with material in visual or auditory fashion, almost always recalls in vocimotor imagery. It appears, therefore, that, regardless of mode of presentation, an *O* recalls material predominantly in imagery of his own type, although supplemented at times by the imagery corresponding to the mode of presentation. Certainly the mode of presentation is in no way indicative of the modality of imagery that an *O* will employ in recalling that material.

When material is recalled with difficulty, the imagery may first appear in the *O*'s own type, and then be completed by imagery corresponding to the imagery of the mode of presentation; or the difficult word may first appear incomplete and unclear in the imagery of the mode of presentation, and then be completed by the imagery of the learner's type. Observers of all types in recalling in auditory terms a material presented auditorily by *E* usually have imagery of their own voices rather than of *E*'s. Occasionally, however, if *E* pronounces a word in a manner which seems odd to the *O*, the recall appears in auditory imagery of *E*'s voice.

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[30] Kuhlmann (*op. cit.*) and von Wartensleben (Ueber den Einfluss der Zwischenzeit auf die Reproduktion gelesener Buchstaben, *Zsch. f. Psychol.,* 64, 1913, 321-385) found subjective uncertainty when there was rivalry between two images for the center of consciousness. Meyer (*op. cit.*) found that the greater the lack of subjective assurance the longer the reaction-time, and that indefinite localization conditioned subjective uncertainty.

[31] Abbott (*op. cit.*) points out that a visual image is invariably substituted at once for the heard letters. Frankfurther and Thiele, Meumann and many other investigators have shown that the image of the reproduced word is primarily determined in the ideational type of the learner, and is influenced only secondarily by the mode of presentation.

In no instance, either in learning or in recall, did any *O* report the presence of a manumotor image.[32] It has been assumed that learning is more efficient if the material to be acquired is written during the act of learning, especially when the material is dictated to the learner as it may be in learning to spell. In the light of our data it appears that the writing-movement, the motor sensations *per se*, do not help at all in the learning.[33] The help comes rather from the *O*'s seeing what he has written, and it is this visual percept only that helps the learner. The act of writing is important and necessary because it makes possible this visual percept and because attention is thus retained longer upon the word than is the case when the word is written by another person.

If the series is difficult to learn and the *O* requires a great number of presentations to learn it, the delayed recall is apt to be relatively poor. This situation arises because successive presentations yield diminishing returns. In most of the later presentations the *O* passes over the words which he has already anticipated; he assumes that they are learned and tries neither to perceive them clearly nor to anticipate them. Thus the later presentations do little to strengthen the associations of many of the words, which after a time pass below the associative threshold as readily as if the final presentations had not been made.

Meumann's rule is that the greater the number of presentations, the greater the strength of the association, everything else being equal. But in no series unfortunately are all things equal. Even nonsense syllables are of unequal difficulty, and the various positions within the presented series are variously favorable. Hence in learning a series it is inevitably true that some parts are learned first and that the final presentations are of greatly diminished value in the further impression of these parts.

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[32] One observer, *T*, Am, reported with great uncertainty what he thought was a manumotor image, but at no other time was a manumotor image reported by any *O*.

[33] Smith, T. L. (On muscular memory, *Amer. J. Psychol.*, 7, 1896, 453-490) found that errors in recall were reduced 16% by allowing the learner to write the letters; but his subjects were deaf mutes and employed the deaf-mute alphabet. The nature of this material lends itself more readily to motor reproduction than does printed or spoken language. Lay (*op. cit.*) concluded that the writing *per se* is an aid to learning; Fuchs and Haggenmüller and Itschner (*opp. citt.*), who obtained results similar to Lay's, explained their findings differently. Fuchs and Haggenmüller pointed out that the material is seen twice when it is written, once when it is presented and a second time after being written. Itschner calls attention to the fact that the presentation-time is longer for the series when the *O*'s write the words and that writing the words destroys an illusion that the material is learned, which occurs prematurely in visual presentation.

R. Dodge (*Die motorischen Wortvorstellungen,* 1896, 78 pp.), although of extreme motor type, never had a manumotor image.

Objective Data

In securing the objective data we arbitrarily determined to make the following deductions for each of the possible errors in recall. The deductions are in arbitrary units based on the assumption that perfect recall involves 1,440 units.

	Three-letter syllables	Four-letter words
For a wrong letter....................	32	24
For an omitted letter..................	24	18
For a misplaced letter.................	16	12
For each letter of an unplaced word.....	8	6
For each letter of an interchanged word.	4	3
For an interchanged letter..............	4	3

The deductions were determined in the following manner. The correct recall of a single letter was assumed to count 6 points, and its omission in recall to necessitate a deduction of 6 points from the maximal score. For giving a wrong letter 8 points were deducted, since it is a greater error to give a wrong letter than to give none at all; in this case not only is the correct letter forgotten, but the *lacuna* is also filled in by false data. Since, when a wrong letter is given, more than the total value of a letter is deducted (8/6), it is theoretically possible to obtain a negative score if more than three-fourths (6/8) is positively wrong in recall. Such a situation, however, if it occurred, would indicate a positive tendency for mislearning and not merely a failure to learn, and should properly be represented by a negative value. For each letter of a misplaced word or syllable, 4 points were to be deducted; for each letter of an unplaced (unlocalized within the series) word or syllable, 2 points; for each letter of interchanged words, 2 points. In the foregoing table of deductions these points have been multiplied by 4 for three-letter nonsense-syllables and by 3 for the four-letter meaningful words, in order that the total values of the two materials might be the same. Thus the score-value of 60 three-letter syllables is $60\times3\times4=1440$; and the score-value of 60 four-letter words is $60\times4\times3=1440$. The 1440 units represent perfect recall; deductions are made from 1440 in accordance with the foregoing table, and the remainder is expressed as a percentage of perfect recall (1440 points).

TABLE I

RANK ORDER OF EFFICIENCY FOR EACH MODE OF PRESENTATION. V = VISUAL, A = AUDITORY, M = VOCIMOTOR, m = MANUMOTOR. MODES OF SAME RANK ARE CONNECTED BY SIGN OF EQUALITY

RANK-ORDER

Obs.	Material:	Most Efficient								Least Efficient			
		1	2	3	4	5	6	7	8	9	10	11	12
C	Meaningful	AMm	VAMm	Vm =	A =	Am	VMm =	V	VAm	VAM	VA	AM	VM
	Nonsense	V	VMm	Am	VAMm	VA	VM	AM	AMm	VAM	A	Vm	
Fe	Meaningful	VM =	VA =	VAM	VAm	Am	V	A	VAMm	Vm	VMm	AM	AMm
	Nonsense	VM	AMm	VAMm =	A	VMm	Am	AM	VA	Vm	V	VAM	
S	Meaningful	VM	AM	Vm	A	VA	VAMm	VAM	Am	AMm	V	VAm	VMm
	Nonsense	VM =	AMm	VMm	AM	V	VA	Am	Vm	A	VAM		
W	Meaningful	VM	V	A	Vm	VAMm =	Am	AMm	VAM	VMm	AM	VAm	VA
	Nonsense	VAM	Vm =	AM	VA =	V	A	VAMm	VMm	VM	AMm	Am	
Fi	Nonsense	VM	VAM =	AM	VMm	VA	A	Vm	V =	Am =	AMm	VAMm	
P	Meaningful	VM	AM	VAMm	Vm	V	VAm	Am	VMm	VAM	AMm	VA	A
T	Nonsense	VAM	VM	VMm	AMm	VA	V	AM	Vm	VAMm	A	Am	
All	Meaningful	VM	Vm	VAMm =	V =	Am	A	VAM	AM	VAm	AMm	VA	VMm
	Nonsense	VM	VMm	VAMm =	AMm	VA	V	AM	VAM	Vm	Am	A	

Table I shows that there is no one mode of presentation which is the best for all *O*'s. Visual-vocimotor presentation, VM, is the most efficient in 7 cases: it is second in efficiency once, fifth once, ninth once, and last once. Nor is the same mode of presentation best for the same *O* with different materials. *W*, for example, finds visual-vocimotor presentation, VM, the most efficient mode in learning meaningful words, whereas it is only eighth best for him in the learning of nonsense material.

TABLE II

Average Efficiency for Each Mode of Presentation as Measured by Average Number of Presentations Required for Learning; All Observers. V = Visual, A = Auditory, M = Vocimotor, m = Manumotor. Last Column Arranged in Order of Efficiency with Least Efficient Mode at the Top

Order of Presentation	Mode Presentation	Meaningful Words	Nonsense Syllables	Average Two Materials
7	Am	5.90	7.83	6.87
3	VA	8.10	5.58	6.84
10	AMm	7.80	5.92	6.86
2	A	6.00	7.33	6.67
11	VMm	9.10	4.17	6.64
8	VAM	7.00	5.83	6.42
5	Vm	4.80	7.58	6.19
6	AM	7.20	5.00	6.10
12	VAMm	4.00	6.90	5.85
1	V	5.90	5.25	5.58
4	VM	3.40	3.42	3.41
9	VAm	8.00	....	....

Table II again shows that the combined visual-vocimotor mode of presentation is by far the most efficient. The amount of variation between the other modes of presentation is so slight that no significant differences are apparent.

These objective data do not properly afford an answer to the problem of the most efficient mode of learning, because in the first place the determinations are too few to allow of a significant statistical treatment, and because the general averages fail to take account of the imaginal type of the *O*'s. If statistics are to tell the true story, an average must represent not a single mode of presentation but a single mode of learning. The learning-process must be introspectively controlled or at least viewed in the light of the previously determined type of the learner, and averages must be found for similar modes of learning, even though they occur with dissimilar modes of presentation.

SUMMARY AND CONCLUSIONS

1. The process of learning consists of three distinct stages: (*a*) the orienting stage (pp. 269 ff.); (*b*) the stage of attempted anticipation (pp. 271 f.); and (*c*) the anticipatory stage (pp. 272 ff.).

2. All of our *O*'s found it necessary to employ vocimotor imagery in learning a series of meaningful words or nonsense-syllables; no *O* was able to learn a series of words or nonsense-syllables if he succeeded under instruction in inhibiting vocimotor imagery from the start. The *O*'s of visual type in most cases did not employ vocimotor imagery after the first few presentations of the material in the learning, but the vocimotor imagery was present during the initial presentations and did not lapse until the visual imagery had become clear and definite. The *O*'s of the motor type are never able to inhibit vocimotor imagery and yet learn the material.

3. If the *O* is instructed not to use vocimotor imagery during learning he responds (*a*) by attempting actively to inhibit vocimotor imagery, thus interfering with or preventing learning, or (*b*) by setting up for himself a new *Aufgabe* to use imagery of another modality (*e.g.,* visual imagery).

4. The *O* in learning usually groups the words or syllables; and the number of words or syllables which constitute a group depends upon (*a*) the time intervening between the presentation of two successive words, (*b*) the immediate memory-span of the *O,* (*c*) the meaningful associations between the words or syllables, (*d*) the presence of an *Aufgabe* for grouping that the learner himself may set up, and (*e*) the position of difficult words within the series.

5. Manumotor imagery does not aid either the learning or the recall. When the material to be learned is presented to the *O* in auditory fashion, the learning is in most cases more efficient if the *O* is required to write the material than if he does not write it; and the increased efficiency occurs especially when the materials are isolated words or syllables or when auditory perception is less definite than visual perception. In these cases the *O* must decide upon a definite spelling in order to write the word pronounced to him, and the writing thus definitizes the perception. The advantage of writing, therefore, comes not from manumotor processes but from the visual percept of the written word.

6. The mode of presentation is in no way indicative of the modality of imagery that an *O* will employ in learning or

recalling a series of words or syllables. The modality of the imagery which a learner employs is determined primarily by his ideational type and only secondarily by the mode of presentation.

7. All *O*'s find it difficult to obtain a definite auditory perception of some syllables, especially those syllables containing the letters *c, q, k; d, t; etc.*

8. The recall of difficult words or syllables is in most cases preceded by the appearance in consciousness of a schema in one of three types: (*a*) visual schema (p. 275), (*b*) kinaesthetic schema (p. 275), (*c*) rhythmic schema (pp. 263, 275).

9. In addition to the words themselves and their associations there may occur as an aid a 'motor trend.' This 'motor trend' is present in the acts of anticipation but more frequently in the recalls.

10. Extraneous associations with the words to be learned are formed by all the *O*'s. There seems to be no well-defined chronological order in which the words and these associations appear in recall. The word appears sometimes in the imagery corresponding to the learner's ideational type and sometimes in the imagery corresponding to the mode of presentation; and the association similarly may come either in the imagery of the learner's ideational type or of the mode of presentation.

11. Words which have these extraneous associations are in most instances retained better than words not thus associated.

12. The first two or three and the last two or three words of a series are the first words of the series to be learned.

13. Visual imagery is not efficient for a rapid anticipation and usually gives way to vocimotor imagery when rapid anticipation is acquired.

14. In visual-auditory presentation the learner seldom attends equally to both the auditory and the visual aspects of the presentation. He attends usually almost wholly to the one or to the other according to his ideational type.

15. The recall of a series is sometimes accompanied by subjective assurance and sometimes by subjective non-assurance.

16. The statistical data obtained in this study are significant in scarcely a single instance for the reason that, although the objective conditions were kept constant in accordance with the rules for such investigations, the subjective factors could

not be brought under control. At best mere objective data will do little more than indicate the most efficient mode of presentation for a particular *O,* until account is taken of the ideational type of the *O*'s, the attentive selection that they exercise among the various presentative aspects of a material, and the manner in which one sensory mode is subject to translation into another. Some introspective procedure is a necessity.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE.

XLII. VOLUNTARY CONTROL OF LIKES AND DISLIKES; THE EFFECTS OF AN ATTEMPT VOLUNTARILY TO CHANGE THE AFFECTIVE VALUE OF COLORS.

By MARGARET FLOY WASHBURN AND SARAH LOUISE GROSE.

Marcus Aurelius said: "Everything is opinion, and opinion is in our power." It is the second part of this comfortable aphorism that most arouses our distrust. When we test it critically, it seems to resolve itself into the much less satisfactory statement that one of our opinions is in the power of another of our opinions when the two come into conflict; the most encouraging feature of the case is that the victory in such a conflict may through the operation of certain mental processes be conferred on that opinion which seemed at the outset weaker. Such a conflict is especially vital when the opinions are not merely intellectual but emotional; when they are desires. Practical life often confronts us with the advisability of 'changing our desires rather than the order of the world,' to use another Stoic phrase; of liking the things we at first disliked, and regarding with aversion the things that were originally attractive to us. This feat can actually, by a person with normal mental inhibitions, be performed; and one of the most important problems in practical psychology concerns the methods by which it can be performed.

If we reflect on the process as it occurs in experience, we find that, in the first place, the nature of our organism itself has furnished us with a powerful aid in the overcoming of desires. Through the law of affective fatigue or adaptation an emotion tends naturally to pass over into its opposite; violent delights have violent ends. This natural tendency, which operates on good and bad desires alike, may be utilized by the individual who wishes to overcome a desire, and may probably be somewhat accelerated in its action by the other methods which we will proceed to mention.

A second method by which our affective attitude to certain stimuli may be changed is that of fixing attention on different elements in the stimulus. The object desired is usually sufficiently complex to be not all desirable or all undesirable. One who wishes to change his affective attitude towards a given object may turn his attention from its merits to its defects, or *vice versa,* provided of course that the existing emotional reaction is not so strong as to make attention immovable.

Thirdly, we can alter our likes and dislikes by transforming reality through the aid of imagination. That is, the object may be surrounded by a set of ideas unlike its actual setting, and thus become much more or much less desirable. A person who wishes to overcome a passion for another may imagine that other placed in different social conditions, where his or her defects of character or training would be more apparent, or may ask himself how the individual in question would 'wear' as an unescapable daily companion. This device, in proportion as the imaginary circumstances are improbable, if it succeeds in altering the emotional attitude, does so by virtue of the mechanism which the Freudians have called phantasy; a form of withdrawal from reality.

Finally, it is possible to change the affective attitude by deliberately performing the movements and reactions which belong to the opposite attitude, so far as these reactions are controllable. This is the Freudian mechanism of compensation: whistling to keep up one's courage; declaring in words and acts as strongly as possible that one loves the thing one really hates, or did hate in the beginning.

We may call these four devices by the following terms respectively: (1) affective fatigue; (2) shift of attention; (3) imaginary context; (4) compensation.

The present study undertook to observe the process of deliberately altering the affective reaction to color, presented in the form of small pieces, 3 centimeters square, pasted each in the middle of a card 2½ by 3 inches. One of these cards was laid on a table before the observer, who was asked to express her judgment of the pleasantness of the color by using one of the numbers from 1 to 7, 1 meaning 'very unpleasant,' 7 'very pleasant,' and 4 'indifferent.' When the judgment had been expressed, the experimenter said "Now I want you to see if you can dislike that color," if the judgment had been favorable; or, "I want you to see if you can like that color," if the judgment had been unfavorable. The original judgment and the altered judgment were recorded, and the observer was then asked how she had effected the change. This proceeding was followed with each of eighteen Bradley colors, namely, saturated red, orange, yellow, green, blue, violet and the lightest tint and darkest shade of each of these six. About two months later each observer was asked to judge the pleasantness of the colors again; this was done in order to study the permanence of the changes voluntarily produced.

The total number of observers from whom complete returns were obtained was 53; since each judged eighteen colors, there were in all 954 experiments in each of the two series, the earlier and the deferred series.

In 6.3% of these experiments, the observer was unable to change her judgment of the color.

Of all the	1 (very unpleasant)	judgments made,	14%	could not be changed.
" " "	2 (moderately unpleasant)	" "	5.7%	" " " "
" " "	3 (slightly unpleasant)	" "	3.2%	" " " "
" " "	4 (indifferent)	" "	1.9%	" " " "
" " "	5 (slightly pleasant)	" "	5.4%	" " " "
" " "	6 (moderately pleasant)	" "	6.7%	" " " "
" " "	7 (very pleasant)	" "	8.8%	" " " "

As was to be expected, it was thus found that *extreme judgments are harder to change voluntarily than moderate judgments.* More interesting is the result that *judgments of extreme unpleasantness are decidedly harder to change than judgments of extreme pleasantness.*

Where the effort to change the judgment was successful, we may note certain points with regard to the extent of the change produced. Evidently the judgments 1 and 7 had equal chances to be altered by six points. The results show that in 9% of the 1 judgments the effort to find the colors more agreeable resulted in raising the judgment to 7; while in 32% of the 7 judgments the effort to find the colors less agreeable altered the judgment to 1. It seems clear that under our conditions *it is much easier voluntarily to lessen the pleasantness of a color by six points than voluntarily to raise its pleasantness by six points.*

A similar relation, though less marked, holds between raising and lowering pleasantness by five points. The judgments 2 and 6 have equal chances to be changed five points. Of the 2 judgments, 36% were by the effort to find the colors more pleasant raised to 7; of the 6 judgments, 48% were by the effort to find the colors less pleasant lowered to 1. *It is noticeably easier voluntarily to lessen the pleasantness of a color by five points than voluntarily to raise its pleasantness by five points.*

In a number of cases where the effort to change the affective value of a color succeeded, the original judgment recurred after the two months' interval; that is, the pleasantness of the color was estimated after the interval just as it had been estimated when first shown, before the effort was made to alter it. Of the 7 judgments, 29% thus recurred; of the 6 judgments, 26.9%; of the 5 judgments, 28.3% of the 4 judgments, 25.7%; of the 3 judgments, 18%; of the 2 judgments, 25%; of the 1 judgments, 26.7%. It thus appears that *pleasant judgments, though more easily changed than unpleasant judgments, have a somewhat greater tendency to recur later.* The average percent. of recurrence for pleasant judgments is 28; for the unpleasant judgments it is 23.2 Part of this difference is caused by the very low tendency of the judgment 3 to recur; a fact for which we have no explanation to suggest.

In a number of other cases the judgment made after the two months' interval coincided not with the original judgment, but with the judgment as altered by the observer's effort. Here it is interesting to observe the relation between the amount and direction of the change effected, and its permanency.

Of the changes where pleasantness was	raised	1 point,	24%	were permanent.
" " " " " "	lowered	" "	32%	" "
" " " " " "	raised	2 points,	16%	" "
" " " " " "	lowered	" "	8%	" "
" " " " " "	raised	3 "	14%	" "
" " " " " "	lowered	" "	12%	" "
" " " " " "	raised	4 "	11.8%	" "
" " " " " "	lowered	" "	1.6%	" "
" " " " " "	raised	5 "	2.6%	" "
" " " " " "	lowered	" "	2.3%	" "

Obviously accidental variation would often be responsible for slight changes from the original judgment, so one would expect that one point changes would have the greatest tendency to recur. Where the amount of change was more than one point, it is noticeable that *changes in the direction of increased pleasantness are more likely to be lasting* (av. per cent of permanence of former, 11.1, of latter, 5.9).

On the whole, these results might be interpreted as suggesting the existence of *a superficial pessimism operating on a deeper-lying optimism.* The pessimism is indicated by the fact that it is easier to pass from strong liking to strong disliking than to go in the reverse direction, and by the fact that it is harder to change at all a judgment of extreme disliking than a judgment of extreme liking. *At the moment, our observers were more ready to abandon their likes* *than their dislikes.* But *in the long run, the tendency was towards optimism;* the observers inclined to recur to their original likes more than to their original dislikes, and to be more lastingly influenced by the favorable changes which they had effected in their reactions to the colors than by the unfavorable changes.

We may next consider the methods which they used to bring about the changes. *By far the most frequently used method was the one which we have called 'imaginary context.'* The color was thought of in an imaginary setting different from the actual one. In 32% of the entire 954 experiments, altering the context produced a change in the affective judgment. For example, the shade of red was judged 5 at first; imagining it in wall paper lowered its pleasantness to 2. Where this method of altering the affective judgments was used, *it produced marked changes.* Of the changes amounting to 6 points, 50% were due to altered context; of those amounting to five points, 46%; of those amounting to four points, 40%; of those amounting to three points, 39%; of those amounting to two points, 30%; of those amounting to one point 24%. It was a little more effective in raising than in lowering the affective values, but the difference was not marked (average, 40% of raising, 36% of lowering).

A particular form of altered context, applicable only to color sensations, is *the imagining the color in combination with some other color.* In 17.7% of the 954 experiments, the affective value of the colors was changed by this method. It was less effective in producing six point changes than other forms of altered context: 21% of the six point changes were due to this cause, 29% of the five point changes; 20% of the four point changes, 22% of the three point changes, 20% of the two point changes, and 5.9% of the one point changes. It was somewhat more effective in lowering the affective value of the colors than in raising it (average, 20.6 of raising, 26.9% of lowering).

Still another form of altered context consists in imagining a greater or less amount of the color. In 4.2% of the total number of experiments the change of affective value was due to imagining the color in greater quantity; in only one of these 41 cases was the effect thus produced that of raising affective value. *Imagining the quantity of a color to be increased nearly always lowers its pleasantness.* This influence is most effective in producing slight changes of affective value; it caused no six point changes, 4.8% of the five point changes, 12.8% of the four point changes, 9.6% of the three point changes, 10% of the two point changes, 18.7% of the one point changes. In only two percent of the total number of experiments was the affective value of a color altered by imagining less of it; obviously because of the small size of the bits of color shown. The effect of lessening the amount of the color in imagination was only in one case an increase of the affective value, the change then amounting to one point. The greatest effectiveness of this device was in raising the pleasantness two points, 10% of the cases of such raising being due to its influence.

The *effect of shift of attention,* the voluntary direction of attention to some disagreeable aspect of the color, was shown almost entirely through a *direction of attention to some agreeable or disagreeable association* with the color. Although this involves the use of elements not actually presented in the color itself, it cannot be said to constitute a transformation of reality in the sense that altered context does. It is a recollection of actual experience. Recall of associations with the color was the controlling influence in 14.1% of all the experiments, and had its maximum effectiveness in the case of large changes in affective value, and it was *markedly more influential in raising than in lowering pleasantness.* Of the six point changes towards pleasantness, 33% were due to associations; of the six point changes towards unpleasantness, 33%; of the five point changes toward pleasantness, 15.8%; of the five point changes toward unpleasantness, 7%; of the four point changes toward pleasantness, 31%; of the four point changes toward unpleasantness, 11.2%; of the three point changes toward pleasantness, 25%; of the three point changes toward unpleasantness, 18%; of the two point changes toward pleasantness, 25%; of the two point changes toward unpleasantness, 10%; of the one point changes toward pleasantness, 17%; of the one point changes toward unpleasantness, 12.5%. The average percentage for increasing pleasantness was thus 24.4; for decreasing pleasantness, 15.3. This fact, that *it is easier to recall pleasant than unpleasant associations with colors,* is in accord with a point established previously[1] in our laboratory, namely, that when the affective value of a color changes spontaneously during fixation for one minute, increase of its pleasantness is more likely to be due to association than is decrease of its pleasantness.

The colors, being such simple objects, did not in themselves present enough variety of aspect to allow the shift of attention from agreeable to disagreeable features or the reverse. The nearest approach to such a process was shown in the case of 17.6% of the one point drops in pleasantness, which the observers ascribed to finding colors 'insipid.' It is hard to decide whether these cases do not come under the next head, that of *affective adaptation.*

This influence was exercised in 2.2% of all the experiments. It had *very little power to produce marked changes* in judgments of affective value, and what power it had in connection with such changes was to *lower pleasantness.* In 2.3% of the six point drops in affective value it was the cause at work, and in 1.17% of the five point drops. Its effectiveness was oftener shown *in the case of the slight changes,* where it brought about *both increased and lowered pleasantness;* of the two point drops it was responsible for 5.6%: of the one point rises, for 13.3%; of the one point drops, for 14.7%. Probably the slight influence of affective adaptation in these experiments is due to the mildness of the emotional reactions involved.

Finally, *true compensation,* the deliberate assumption of the opposite affective attitude, *was used by only one of our observers,* and by her only three times. She raised the value of one color from 1 to 3 'just by trying suggestion,' and lowered that of two from 6 to 5 by 'concentrating on getting them down.' It is easy to conjecture that *this method,* so useful in ordinary life, *would naturally play little part in conditions where the affective state is not only mild, but accompanied merely by simple motor expressions.* One may assume hatred in order to counteract love, for example, because hatred may be expressed by a great variety of movements, by torrents of words, by forcible actions, and when these voluntary movements are set in operation, there is a fair chance that the deeper organic movements associated with them may come into play and the emotion really be transformed. But expressing one's like or dislike of a color is so mild and simple a motor process, that its voluntary performance can have no very profound effect.

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[1] "The tendency of associated ideas is to raise the pleasantness of a color." Washburn and Crawford: Fluctuations in the Affective Value of Colors during Fixation for One Minute. This Journal, 22, 1911, 579-582.

BOOK REVIEWS

Der Geruch. By HANS HENNING. Leipzig, Barth, 1916, viii, 533 pp.

Henning's notable contribution to the psychology of smell first appeared in the *Zeitschrift für Psychologie.* Its parts are scattered through various numbers, beginning with the one dated October, 1915 and ending with that dated September, 1916. The appendix of the book contains not only a special study of the sense of smell in ants but also a very important discussion of taste-qualities. Both these papers appeared in the number of the *Zeitschrift* dated February, 1916. The bound volume contains a few additional notes, a name-index and an index to the scents discussed.

In the opinion of the reviewer, Henning's work marks the beginning of a new era in the study of smell and probably also in that of taste. In his treatment of his predecessors, Henning is a ruthless—in fact, a very uncivil—iconoclast. To change the figure, he forgets that if the work of Zwaardemaker, Nagel and Aronsohn had not been done, he would have had to break some quite rocky ground which, as a matter of fact, he has had only to plough over and to plant anew. Nevertheless, one is reluctantly compelled to admit that the authors of our standard text-books will have to rewrite *ex radice* their chapters on smell, chapters based for the most part on the conclusions of Zwaardemaker in his *Physiologie des Geruchs,* published twenty-six years ago. Twenty-six years make more than half the life-time of experimental psychology, and Henning's criticisms not only are searchingly destructive but also carry full conviction. This conviction is half dismaying and half comforting to anyone who has tried faithfully but unsuccessfully to reproduce the results of Zwaardemaker's compensation and exhaustion experiments or to obtain definite evidence for the Linnaean classification of odors—adopted by Zwaardemaker (be it said) not as final but as one feature of an experimental programme.

The number of Henning's experiments and the expertness of his subjects, in which he has the better of all other experimenters in his field, lend great weight to his positive conclusions. The writer confesses that, when she reviewed early in 1916 (in the *Psychological Bulletin*) the first instalment of his work, she was antagonized by his derisive treatment of earlier experimenters. She also confesses to lacking the wide and exact knowledge of biology and organic chemistry which are necessary for a reliable evaluation of some of Henning's most important findings. But from the experimentalist's point of view, she cannot forbear saying again that one looks almost in vain in his pages for precise details of the procedure employed in particular groups of experiments. One canon of scientific investigation Henning certainly violates to some extent; he seldom makes it possible for convert or critic to reproduce the minutiae of his experimental conditions. To this criticism on his work in classification he has replied by saying (p. 360): "Ich finde, zum Experiment genügt eine Flasche mit Riechstoff, die jeder kennt." With this remark anyone who has worked much with olfactometers must have some sympathy.

Another peculiarity, which seems unfortunate in so polemic a writer as Henning, is that he rarely reduces the introspective data obtained from his subjects to statistical form and does not mention, except to explain away, instances in which their reports conflicted with his conclusions. For the most part, his method of procedure is to state his conclusions dogmatically, but amply to illustrate them from the reports of his observers. He says very truly (p. 360) that the critical introspection of trained psychologists [such as he numbered among his observers] is more valuable than statistics taken on all the students in the University, and that the statistical procedure, about which science in America has so far raved (*immer noch krankt*), has by no means the precision of a qualitative analysis. But the point is not that his experiments were deficient in number or faulty in method—the writer's belief is quite to the contrary—but that he gives us little or no inkling of ever obtaining inconvenient data, and that he thus fails to create an impression of taking a dispassionate attitude toward his own work in comparison with that of others. Nevertheless—whatever omissions of matter or faults of manner Henning's book may show—it not only represents an enormous amount of work both in laboratory and in library, but also represents the application of a keen and original mind to a line of research which has long badly needed and really merited more enthusiastic pursuit.

Even though a large part of Henning's work consists in a critical examination of the 'literature,' it is impossible in the space allotted to summarize his book at once completely and clearly. It seems best, therefore, to confine the remainder of this review to indicating in the briefest possible fashion the character of his subjects, material and apparatus, to stressing two or three important points which he makes in regard to method in smell experiments, and to stating intelligibly those of his conclusions which most strikingly contravert the findings of earlier authorities.

His experiments were made at Frankfort on the Main, where he was *Privatdozent* at the time his book was written. He made more or less extended series of individual experiments on thirteen grown persons and three children. He also made certain group-experiments on forty-six university students. His really most expert subject was his wife, a trained psychologist, who conducted experiments in which he himself had the chance to serve as observer. Of the fourteen chief subjects (including Henning), four were psychologists by profession, six in all were trained psychologists, and ten in all had at least an elementary knowledge of psychology. Of the professional psychologists two—Henning himself and Professor Cornelius—were thoroughly grounded in chemistry, the latter being especially well-acquainted with odorous materials. Of the other subjects, one had considerable knowledge of chemistry, and another was familiar with clinical laboratory-work.

In his chief series of experiments Henning used 415 different scents, selected to represent the whole qualitative range of natural odors and including in about equal numbers chemically pure substances (such as essential oils) and natural scents (such as dried herbs). He also submitted the odors of daily life to systematic examination and even made excursions with his subjects to the Zoölogical Garden and to other places rich in smells in order that no natural odor essentially different from the 415 specimens might escape him. He also made some use of fifty-one odorous trade-articles, such as perfumes, officinal preparations and ink. He kept many of his scents at hand in five different concentrations. On the average, in his qualitative experiments, about ten smell-exposures were made to a subject at a sitting. For olfactometric purposes he used six different devices, including various forms of Zwaardemaker's olfactometer (which he criticizes severely).

Not the least valuable part of Henning's contribution consists in the following methodological points. First, the quality of a scent cannot be fully appreciated by "monorhinic" smelling, smelling with one nostril only, which is unnatural. From this it follows that mixture of qualities by "dichorhinic" smelling—smelling one with one nostril and another with the other—is unsatisfactory. "Dirhinic" smelling alone gives clear-cut perception. Secondly, reliable judgments with regard to smell-similarities can be obtained only from observers who do not know the nature of the scents with which they are dealing. Henning distinguishes between the true odor (*Gegebenheitsgeruch*), which is obtained by the observer who is smelling with closed eyes and is ignorant of the nature of the scent, and the object-smell (*Gegenstandsgeruch*), which (like color) is projected upon the object from which it is known to come and is apt to be distorted by associative supplementing. Both upon this point and upon that of monorhinic smelling such experience as the present writer possesses is fully in accord with Henning's.

The most revolutionary of Henning's conclusions have to do (1) with the interrelation of smell-qualities, (2) with the phenomena of smell-mixture, (3) with the phenomena of smell-exhaustion, and (4) with the qualities of taste. In the discussion of these conclusions some of his other findings will incidentally appear.

(1) He holds that smells, like colors, constitute a tridimensional manifold, although smell-qualities are related to one another in quite another fashion than are color-qualities. In the first place, the groupings of odors must be represented by a prism rather than by a double pyramid. Of this prism, the triangular faces are equilateral and the rectangular faces are squares. At the angles should stand the most typical smells of his six fundamental classes. At the corners of one triangle should stand respectively the most typical flowery, fruity and putrid smells; at the corners of the other should stand the typical spicy smell on the same edge with the flowery smell on the other triangle, the typical resinous (*harzig*) smell on the same edge with the fruity, and the typical burning (*brenzlich*) smell on the same edge with the putrid. He regards violet as the most typical smell of the flowery class, lemon of the fruity, sulphuretted hydrogen of the putrid, nutmeg of the spicy, frankincense of the resinous, and tar of the burning. Transition-smells lead from every class into every other; the classes which stand diagonally opposite to each other on the square faces of the prism are to be regarded as connected with each other by diagonal lines across these faces. Instances of transition-smells are as follows: (1) between flowery and fruity, geranium and sandalwood; between flowery and putrid, the smell of decaying flowers, and between fruity and putrid, the smell of decaying fruit; between flowery and spicy, thyme and vanilla; between fruity and resinous the various piney odors; between putrid and burning, the ammoniacal animal odors; between flowery and resinous, the smells of the fragrant gums; between fruity and spicy, the mints; between putrid and spicy, garlic; between putrid and resinous, fish-scales; and between burning and all the remaining classes (theoretically at least) the smells obtained by burning scents of these classes. But, in the

second place, odors differ from colors in the fact that likeness between smells does not correspond with an overlapping of the physiological processes concerned. Henning sets his face against specific energies in the realm of smell, against original smells comparable to the *Urfarben* of the color theories. It is only by courtesy that we can say that any natural smell is counterfeited, that the smell of violets, for example, is counterfeited in the perfume industry. The smells at the angles of the prism are indeed related to one another like the colors at the angles of the pyramid, but the smells on any one edge or diagonal are related to one another like the tones in the tonal series. A mixture of two such smells does not result in a simple blend like the orange obtained by mixing red and yellow. In fact, the prism cannot be made to illustrate *both* the transition from one simple smell to another through other simple smells (for example that from lemon to nutmeg through certain minty odors) *and* also the transition from one to the other as produced by mixing the two in varying proportions. For if mixtures are transitional between two smells on the surface of the pyramid, they cannot logically be placed upon a line drawn through its substance. The whole surface of the prism, however, and not merely its edges and diagonals, must be thought of as occupied by simple smells localised according to their two-sided, three-sided or four-sided resemblances. Thus, all simple inorganic scents would stand somewhere on the fruity-putrid, burning-resinous face. The inside of the prism may be vaguely imagined as reserved for unblended mixtures of smells on different faces.

Henning bases his classification primarily on the descriptions given, and the confusions made, by his subjects in extended series of experiments in which they were instructed carefully and without haste to describe the qualities of scents presented. These experiments were supplemented by others in which the observers were given a number of scents and were required to arrange the similar ones in series and to throw out the others. He finds secondary (but probably very real) support in organic chemistry. Assuming that the quality of an odor depends not on the composition but on the pattern of the scent-molecule (which is probably true), he points out a striking sameness of pattern in the benzol rings of the aromatics belonging to any one of his six classes, and also the transitional character of the patterns corresponding to transitional smells. Henning scouts any connection between odor and the periodic system. Odor is as much a constitutive property of the molecule as is fluorescence.

The replacing of the linear or one-dimensional grouping of the Linnaeus-Zwaardemaker classification by a tridimensional grouping appeals strongly to the present writer. An instance of the difficulites which crop up out of the former classification is this: a short-cut apparently exists between the camphor-turpentine and the lemon-rose subgroups (under the "aromatic" group) beside the recognized but longer path through the spices and the mints. Years ago, in personal conversation with the writer, Zwaardemaker noted the resemblance between oil of orange and pure turpentine, and also the difficulty created by the many-phased smell of oil of juniper, which Henning places on the flowery-fruity-resinous-spicy face of his prism.

(2) Henning denies any analogy between the phenomena of color-mixture and those of smell-mixture, which are rather parallel to the phenomena of tonal fusion. When certain smells are mixed, the result is indeed comparable to the most perfectly welded chords, which Stumpf admits are simple in direct sensory experience. The best

instances are such natural blends as one encounters in oil of juniper and such stable blends as one finds in the best artificial perfumes. When other odors are mixed, one may have rivalry or the suppression of one smell (or smells) by another. Compensation, the cancellation of one smell by another, is a myth. Henning experimented on forty-six university students with Zwaardemaker's olfactometer and never obtained a single instance. Alleged cases are explicable by fatigue or by exhaustion in the first inhalations of all the free scent-particles in the olfactometer. The phenomenon certainly never occurs in free air. Another possibility, which is best realized in dichorhinic smelling, is the "coincidence-smell," in which the two odors are held apart by a strain of attention (*Aufmerksamkeitsspannung*) and yet have a certain unitary character. Still another possibility, which of course depends on dichorhinic smelling, is the "duality-smell," in which the two components are clearly localized, the one in the right nostril and the other in the left. Blends which are perfect at the first instant of mixture are not necessarily stable. In a little while one may get the coincidence-smell or the duality smell, or one may succeed by shift of attention in making now one and now another smell stand out on a background made up of the rest. This is the phenomenon of the "successive smell." In general, the more similar the smells are, the more perfect their fusion is; within limits their intensity makes little difference.

(3) Henning holds that the phenomena of smell-exhaustion have been exaggerated. He urges with justice that the nervous apparatus of smell should be no more subject to fatigue than is that of the eye or ear. The terminal apparatus of smell may indeed be subject to fatigue, but strong smells cannot be made to disappear merely by exhaustion. Cases of apparent exhaustion are largely explicable by failure of attention to weak and persistent stimuli. Moreover, the effect of fatigue upon the sensory epithelium cannot as yet be distinguished clearly from the toxic effect, local and general, of long continued smelling. Henning describes in detail the toxic effects, marked and lasting for days, produced on one of his subjects by smelling in quick succession 150 different scents, from which substances known to be poisonous had been excluded. The observation was confirmed by experiences with other subjects. The present writer has made similar observations in the case of subjects (particularly herself) who were memorizing long series of smells. But in her opinion, Henning makes too pathological a matter of smell-exhaustion, so-called, and also exaggerates the rôle played by the failure of "sensory attention" in producing insensitiveness to a scent to which one is long exposed. May not smell-exhaustion be comparable with adaptation in the case of other senses? And may not this adaptation be of peripheral origin?

Henning maintains that when sensitiveness to a given smell is dulled by exhaustion, this dullness exists for *that particular odor only;* although, if attention has weakened, it will be poor also for any very similar odor. Aronsohn's method of attempting to arrive at a physiological classification of smells through the effect of exhaustion by one scent upon sensitiveness to another is absolutely valueless. If Aronsohn (says Henning on p. 267) had known the chemical composition of the scents he was using he would never have published his results, for in some instances in which he declared that he could smell one scent but not another, the odorous principle of the two was exactly the same. The differentiation of different parts of the olfactory membrane to correspond with different smell-qualities is rendered highly improbable by the patchy and asymmetrical distribution of the membrane in the smell-clefts of the two nostrils. Henning points out, it may be said in passing, that the scent-molecules must penetrate into the epithelium and there quickly suffer a chemical decomposition such as to make them odorless, else every smell would persist indefinitely.

(4) No account of Henning's revolutionary findings would be at all complete without at least a brief notice of his taste-tetrahedron. He holds that sweet, salt, sour and bitter are not the only simple tastes, but stand related to one another as do the colors at the angles of the pyramid and the smells at the angles of the prism. Instances of transition-tastes are these: between salt and sour, bicarbonate of soda; between salt and sweet, the alkaline tastes; between salt and bitter, potassium bromide; between sour and sweet, acetate of lead; between sour and bitter, potassium suphate; and between sweet and bitter, acetone. The phenomena of taste-mixture are closely comparable to those of smell-mixture.

This review does scant justice to the wealth of material in Henning's book, material which must be of great interest alike to the psychologist, the biologist and the chemist. It should give new impetus to experimenters who have put away the scent-bottle and the olfactometer in despair of reproducing or in any way confirming authoritative results which they have not had Henning's (somewhat unhallowed) courage to reject.

Wellesley College E. A. McC. Gamble

The following books have also been received:

W. Bruhn. *Theosophie und Anthroposophie.* Leipzig and Berlin, B. G. Teubner, 1921. Pp. 108.

J. Cohn. *Führende Denker. Vierte Auflage.* Leipzig and Berlin. B. B. Teubner, 1921. Pp. 117.

B. Erdmann. *Grundzüge der Reproduktionspsychologie.* Berlin and Leipzig, W. de Gruyter & Co., 1920. Pp. viii., 186. $1.80.

S. Freud. *Drei Abhandlungen zur Sexualtheorie. Vierte Auflage.* Leipzig and Wien, F. Deuticke, 1920. Pp. vii., 104.

J. L. Des Bancels. *Introduction à la psychologie: l'instinct et l'émotion.* Paris, Payot & Cie., 1921. Pp. 286.

D. I. Bushnell, Jr. *Native Cemeteries and Forms of Burial East of the Mississippi.* Bureau of American Ethnology, Bulletin 71; Washington, D. C., Government Printing Office, 1920. Pp. 160.

A. Gemelli. *Le dottrine moderne della delinquenza.* Milan, Società Editrice 'Vita e Pensiero,' 1920. Pp. xvi., 212.

C. Read.—*The Origin of Man and of his Superstitions.* Cambridge, The University Press, 1920. Pp. xii., 350.

M. Hamburger. *Vom Organismus der Sprache und von der Sprache des Dichters.* Leipzig, F. Meiner, 1920. Pp. vii., 189.

C. Platt. *The Psychology of Thought and Feeling.* New York, Dodd, Mead & Co., 1921. Pp. x., 290.

C. Baudouin. *Suggestion and Autosuggestion.* Translated by E. and C. Paul. New York, Dodd Mead & Co., 1921. Pp. 349.

L. Vivanti. *Principii di Etica.* Rome, P. Maglione & C. Strini, 1920. Pp. viii., 314.

A. S. Edwards. *The Fundamental Principles of Learning and Study.* Baltimore, Warwick & York, 1920. Pp. 239.

BOOK NOTES

PSYCHOANALYSIS IN 1920, BY G. STANLEY HALL

The International Journal of Psychoanalysis. Directed by SIGMUND FREUD. Edited by ERNEST JONES. London, Internat. Psychoanalytic Press, 1920. Vol. I, Part I, 124 pp.; Pt. II, 97 pp.

The most important recent publication in the field of psychoanalysis is the above journal, which appears at a time when, owing to the presumed disturbed economic and political condition of Austria, the *Zeitschrift, Imago,* and *Jahrbuch* show signs of languishing in quality, quantity, and frequency of appearance. And as Freud himself appears as co-editor and contributor, and especially as England, since the appearance of Trotter's publication, has such a galaxy of able and original devotees of the cult, the center of the movement may henceforth be gradually transferred to London. This journal will be a godsend to all English readers interested in this cult if it can maintain the high level on which it has been begun.

The first two parts contain an excellent review of J. J. Putnam's contributions, and two articles by Freud, in one of which he amplifies the thesis that there have been three great scientific movements: the first marked by Copernicus; the second by Darwin; and the third by the discovery and exploitation of the unconscious. There are very good reviews of literature, especially of Jones' "Recent Advances in Psychoanalysis;" accounts of the proceedings of societies, etc. The journal is well printed and so well edited that it cannot fail to be a helpful and stimulating competitor not only of the *Zeitschrift* but also of our own excellent *Psychoanalytic Review.*

A General Introduction to Psychoanalysis. By SIGMUND FREUD. N. Y., Boni and Liveright, 1920. 406 pp.

Freud here, in a course of twenty-eight lectures, attempts to present the outlines of his system to laymen, and divides his material into three parts: I. The Psychology of Error; II. The Dream; III. The General Theory of the Neuroses. The first part is, to our thinking, excessively and sometimes tediously elemental and prolix; but as the author advances into the subject his interest increases, and the latter part of the book will be found illuminating even to adepts. It is especially significant as being the first attempt the author has made to state his conclusions in a systematic and coherent form; for his voluminous previous publications have been scattered, so that it has been difficult to find some of them, and a few are out of print. Particularly interesting in this publication are the full and careful definitions of the points of difference between Freud's views and those of both Adler and Jung and the Zurich school. It is gratifying to note that the spirit of this discussion is well-tempered and philosophic, and without the bitter personalities that marked the inception of these divergences, which were so fortunate for the science of psychoanalysis but such a strain upon the early friendship of their respective leaders. It would be a calamity if Freud's repudiation of his more independent disciples should be as bitter as Wundt's was of his followers who established the so-called Würzburg school, which he was as unable to follow as Freud is to accept the bold and often vague speculations of Jung. A recent writer has celeverly attempted to psychoanalyze these three leaders, making Freud the feminine; Adler, the masculine; and Jung the combining type,—and with much plausibility, so far as the first two are concerned.

Psychopathology. By EDWARD J. KEMPF. St. Louis, C. V. Mosby Co., 1920. 762 pp.

This indefatigable worker, the most prolific and original American thinker in this field, here brings together his own *aperçus,* hitherto scattered through various publications, into a more or less systematic whole, with very copious case-histories and eighty-seven illustrations. In its original form, Freudianism was developed from purely clinical data, with no more implications of a physiological background than introspection itself. But Kempf has attempted to supply this in what he calls the autonomic functions and gives us, with great cleverness, a correlation of these activities, including of course those of the endocrine glands, with the Freudian mechanisms. Higier in his "Vegetative Neurology," Eppinger and Hess in "Vagotonia" and others have been working in the same direction; but Kempf was bolder and went far beyond these men in applying the findings in this field to psychopathology. Everyone interested in the general field of psychoanalysis will find this volume indispensable.

The Elements of Practical Psychoanalysis. By PAUL BOUSFIELD. London, Paul, Trench, Trübner Co., 1920. 276 pp.

Of all the introductions to Freudianism which deal with elements, this is the latest and the best. The author dissents from Freud only with regard to his complete determinism as opposed to free will, his assumption that all dreams have the same causative factors, and thirdly his theory "that the sexual is the fundamental desire underlying all desires and emotions." He gives us a convenient glossary, but no index of his chapters, which are as follows: The Unconscious Mind, Desires and Psychic Energy, The Evolution of Erotic Desire, The Fate of Erotic Impulses and Aims, Parental Complexes, Narcissism, Dreams, The Fundamental Desires, Technique of Psychoanalysis (with an excellent account of the word-reaction method), Analysis of a Case of Compulsion Neurosis with Paranoid Symptoms, Criticisms of Psychoanalysis, Its Scope. Although this work is chiefly addressed to physicians who are laymen in psychoanalysis, it is of great interest to the general reader, and even the specialist will find much help in the judicious perspective and in the relationships which the author brings out.

Mysticism, Freudianism, and Scientific Psychology. By KNIGHT DUNLAP. St. Louis, C. V. Mosby Co., 1920. 173 pp.

The author groups Freudianism, spiritism, and Christian Science, "a siren trinity," as kindred forms of mysticism, and an "assault upon the life of the biological sciences which psychology alone is capable of warding off." In Chapter II, pp. 44 to 111, he gives a résumé of some of the Freudian positions, with quotations that show that he has made more effort than most critics of the system to understand it, which would serve as a good introduction for the beginner to a very limited portion of the field. In the long concluding chapter he presents the "foundations of scientific psychology;" the chapter is largely expository of his views of awareness, apart from its objects, as the essential thing. We are told practically nothing about any of the Freudian mechanisms except repression and *Verschiebung,* and something of course of the Oedipus and Electra complex; he does not discuss infantile sexuality, sublimation, ambivalence, compensation, introversion and extroversion at all. Nor is there any delineation of the profound differences between Freud, Adler, and the Zurich school. There is little allusion to the later literature, and almost nothing of the applications of psychoanalysis to religion, literature, history, biography, etc. Evidently the author calls everything that deals with the unconscious, mysticism; just as the critics of Weismann called his constructions of metamicroscopic biological units, mysticism. What the latter, Christian Science, spiritism, etc. really are and mean, a subject which he rightly admits is a part of his programme, he unfortunately defers to a later publication.

In this book, but particularly in his also very well written "Personal Beauty and Racial Betterment," the careful reader cannot fail to see that the author has himself been not only greatly stimulated but also profoundly influenced by psychoanalysis. Beauty in woman he conceives as the possibility of motherhood; and in the second part he characterizes various anti-eugenic tendencies in the present, such as the withdrawal from the function of child-bearing of women who enter gainful occupations and those who go on the stage, etc. These views are effectively and very wholesomely presented; but the author dos not seem to be aware of the fact that he is simply amplifying what is implicit in the whole psychoanalytic position, viz., that the chief function of the race is to transmit the sacred torch of life.

To our minds these booklets are distinctly the best of the author's always meritorious contributions to psychology, and he ought to recognize that Freudianism has been to him a very helpful mental stimulus. Moreover, in the last part of his *Mysticism* book he certainly points out diversities in what he calls scientific psychology which are as many and great as those in the system he criticizes; so that the thoughtful reader will close the book with the impression that "scientific psychology" is yet far from being scientific, because lacking a consensus even in so fundamental a thing as the definition and use of terms. Consciousness, which is his muse, has itself often been called first of the hetaerae; and those who worship at her shrine conceive everything unconscious not only as entirely outside the pale but as dangerous to scientific orthodoxy, as Bolshevism is thought to be to all forms of well-organized and effective government.

The Adolescent Girl. By PHYLLIS BLANCHARD. N. Y., Moffat, Yard and Co., 1920. 242 pp.

The psyche of the budding girl (*Backfisch, tendron,* "flapper") has seemed about the very most unknown of all the great domains of psychology. Woman has played a great rôle in culture history, from the days of the Pythonesses down to the Fox sisters, who gave the initial momentum to spiritualism in this country, and the Creary girls, who were the chief theme of investigation in the early years of the English Psychical Research Society. The author writes with a very wide knowledge of the literature of the subject, and has had much personal contact with girls in the pin-feather stage of their development. The book is therefore in some respects unique in its field, and is not only an excellent summary of what has already been done but also makes important original suggestions at many points. The chapter headings are: The Broader View, The Sexual and Maternal Instincts of the Adolescent Girl, The Adolescent Conflict, The Sublimation of the Libido, Pathological Manifestations of Libido in Adolescent Girls, The Adolescent Girl and Love, and The Adolescent Girl and Her Future.

Probleme der Mystik und ihrer Symbolik. By H. SILBERER. Wien, H. Heller, 1914. 283 pp.

Here again, as in his great work on symbolism, the author takes his text from an old Rosicrucian manuscript on the parabola, and on the basis of its exegesis gives us a restatement of his views on the meaning of dreams and *Märchen;* and then attempts a specific psychoanalytic interpretation of his text, discussing more fully alchemy, the hermetic art, Rosicrucianism, free-masonry, introversion and rebirth, mysticism, and the royal art. This volume, although written in 1914, has only just reached this country, and so is included among the books that have appeared in 1920.

Man's Unconscious Passion. By WILFRID LAY. N. Y., Dodd, Mead and Co., 1920. 246 pp.

The author here gives us his third book, which is, like his preceding volumes, interesting and instructive; but while it contains much suggestive material, it presents no essentially new points of view. The chapters are as follows: The Total Sensation, Conscious and Unconscious Passions, Affection Is Not Passion, Insight, The Transfer of Passion, The Emotion Age.

The Problem of the Nervous Child. By ELIDA EVANS. N. Y., Dodd, Mead and Co., 1920. 299 pp.

The author is an experienced social worker who has for years come into close contact with childhood and has studied with Jung (who writes a very appreciative introduction to the book). She here gives us a picture of a Freudian child, with copious and very interesting illustrations. It is far and away more insightful, not only into the life of the child normal and abnormal, but even into psychoanalysis, at least from the Zurich point of view, than the child-psychology of Hug-Hellmuth which appeared a year or more ago. Despite the delicacy of some of the topics treated, there is little or nothing that could shock the most sensitive reader, so that we have in Mrs. Evans' pages a presentation of child-psychology which not only all parents but all psychoanalysts as well will profit by reading.

Sex and Life. By W. F. ROBIE. Boston, Richard G. Badger, 1920. 424 pp.

This book, by the author of "Rational Sex Ethics" and "Further Investigations in Rational Sex Ethics," is the largest and most important of his works. While not specifically Freudian, it would probably never have been written, and certainly never printed or read, but for the greater freedom of discussion and the fructifying new ideas that have come to us from Vienna and Zurich. The author begins with a very frank sex autobiography, advocates a correspondence school of sex education, and then proceeds to discuss rational sex ethics for parents, for young men, for young women, and for married people. There are copious and well-chosen references to suitable literature for every class, and plenty of case-histories and glossaries. Dr. Robie in all his writings minimizes the evils of self-abuse. He paints very attractive pictures of the felicity of happy and fecund marriages. His own experience has been long, rich, and very varied, and he has learned how to draw lessons from it. His cases are not only interesting in themselves but are all the more so because drawn from typical New England communities with two characteristics that seem rather salient: first, the scrupulosity of the New England conscience; and secondly, infertility. The author's method of analyzing his cases is far simpler than that of the Freudians and seems generally to have been extremely effective. It may not perhaps be improper to add here that in his own personal family life he illustrates an exceptionally high type of living, and certainly seems to have found the way to a happy life and pointed it out to many others.

Repressed Emotions. By ISADOR CORIAT. N. Y., Brentano's, 1920. 215 pp.

This is an interesting but rather light work which contains some interesting case-histories and some excellent generalizations. Coriat compares Freud's discovery of the unconscious to that of Harvey of the circulation of the blood, which made modern physiology possible. His chapters are: The Meaning of Repressed Emotion, Repressed Emotions in Primitive Society, Repressed Emotions in Literature, The Sublimation of Repressed Emotions, The Development of Psychoanalysis, The Depth of the Unconscious, A Fairy Tale from the Unconscious.

Psychoanalysis: A Brief Account of the Freudian Theory. By BARBARA LOW. N. Y., Harcourt, Brace, and Howe, 1920. 199 pp.

This epitome, prefaced and commended by Ernest Jones, is the briefest yet of the many introductions to Freudianism. The author treats, in successive chapters, the scope and significance of psychoanalysis; mental life, conscious and unconscious; reversions; the dream; social and educational results. The work is clearly and admirably written.

Friedrich Hebbel: ein psychoanalytischer Versuch. By ISADOR SADGER. Wien, Deuticke, 1920. 374 pp. (Schriften zur angewandten Seelenkunde, Heft 18.)

Sadger has added another to the nearly two-score psychoanalyses of great men in the above volume on Hebbel, the German poet and mystic. Psychoanalysis almost from the first has found in this writer's explorations into the depths of his own soul much material for quotation and comment; and Sadger's painstaking work here shows that in both his life and literature Hebbel furnishes some of the most striking illustrations of the Freudian mechanisms, and this in copious detail, so that few individuals yet analyzed come as near as he to the ideal case of Gradiva; while, on the other hand, the rather distinct stages through which he passed serve almost as well for this purpose as Maeder's study of Dante.

NOTES

THE ROAD TO EN-DOR[1]

This book tells the story of the plans laid by two British officers for escape from a prison camp in Asiatic Turkey. The first scheme, which took shape casually from some jesting trials of a home-made ouija board, was to convince the Turkish Commandant that the officers in question possessed mediumistic powers. It succeeded beyond expectation; not only the Turks, but also the fellow-prisoners of the pretended mediums were convinced. "In the face of the most persistent and elaborate efforts to detect fraud," writes the author, "it is possible to convert intelligent, scientific, and otherwise highly educated men to spiritualism, by means of the arts and methods employed by 'mediums' in general." More than that, these men may remain converted. "Eighteen months later, . . . I had told [one of the converts] all our work had been fraudulent, and had quoted [an instance] to show how it was done. . . . The Convert smiled pityingly at me"—and proposed an explanation by unconscious telepathy! No wonder, then, that the Turks were impressed. Things went so far that the camp was to all intents and purposes governed by the 'spook,' who secured many privileges for the inmates. And the plan finally failed, not by its inherent weakness, but through the over-credulity of its victims: a counter-spook, brought on the scene in the interests of escape, grew by an unhappy accident all too powerful; and it was the Commandant's fear of this opposing power that wrecked the whole elaborate device.

If, however, the first part of the book reads us an excellent lesson in applied psychology, the second part is even more instructive. The original plan, perforce abandoned, passed smoothly into another, a plan of release by way of pretended madness; the one officer became a 'furious,' the other a 'melancholic.' Here the risks were, of course, much greater than before, since the simulation must run the gauntlet not only of the local Turkish medical officers but also of the Paris-and-Vienna-and-Berlin-trained psychiatrists of Constantinople. The two men had been well coached at Yozgad by a fellow-prisoner, a physician of wide experience. They showed an astonishing endurance and persistence, and kept up an unremitting guard on their own conduct and on that of their visitors and attendants. The case-histories were also prepared with the utmost care and foresight. All the same, one wonders what might have happened if Mazhar Osman Bey had been less busy! Perhaps a psychiatrist may be persuaded to go over the data and tell us. This plan succeeded, and the officers were freed—just about a fortnight before the armistice with Turkey was signed. They had, nevertheless, the full satisfaction of success, and the record of their adventures is a valuable psychological document. E. B. T.

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[1] *The Road to En-Dor: Being an Account of How Two Prisoners of War at Yozgad in Turkey Won their Way to Freedom.* By E. H. Jones, Lt. I. A. R. O. With illustrations by C. W. Hill, Lt. R. A. F. New York, John Lane. MCMXX. Pp. xiii., 375.

JOURNAL DE PSYCHOLOGIE

The *Journal* of Pierre Janet and Georges Dumas, interrupted by the war, began to appear again in January, 1920. With the reissue, the editors have taken a new departure. Without any thought of hostility to British and American psychologists, they desire to make their magazine "a Latin journal, appearing at the same time in all the Latin capitals, publishing articles written by Latin psychologists, and addressing itself to all Latin readers." They have accordingly brought together an international staff, consisting of J. Ingenieros (Argentine), Van Biervliet and Decroly (Belgium), Austregesilo, M Bomfim, J. Moreira, A. Peixoto, F. da Rocha (Brazil) ; G. Marañon, A. Pi Suñer, S. Ramon y Cajal, R. Turró (Spain) ; H. Bergson, Ch. Blondel, Chaslin, Delacroix, Lalande, Lapicque, Piéron, Rabaud, Revault d'Allonnes, Séglas, H. Wallon (France) ; Boreas, Catsaras (Greece) ; Ferrari, Gemelli, Kiesow, Morselli, Ponzo, Rignano, Sante de Sanctis, Tanzi (Italy) ; Marinesco, Obregia, Radulesco-Motru (Rumania) ; and Bovet, Claparède, Larguier des Bancels (Switzerland). Most of these names are already familiar to us ; of the rest we shall hope to learn from the pages of the *Journal.* E. B. T.

ARCHIVIO ITALIANO DI PSICOLOGIA

We have received the first (double) number of this journal, which is edited by Professor F. Kiesow of Turin and Professor A. Gemelli of Milan with the co-operation of V. Benussi (Padua), L. Botti (Turin) C. Colucci (Naples), S. de Sanctis (Rome), E. Morselli (Genoa) and M. Ponzo (Turin). The staff is both strong and representative, and the *Archivio* has our best wishes for the success that it will undoubtedly achieve. According to a prefatory Note to the Reader, the *Rivista di psicologia,* edited by Professor G. C. Ferrari of Bologna, will devote itself to the application of scientific psychology and to the popularisation of psychological results among the students of neighboring disciplines, while the *Archivio* will publish strictly scientific articles. The contents of the present issue are: F. Kiesow, Observations on the relation between two objects viewed separately by the two eyes ; A. Gemelli and A. Galli, Researches on attention : i. A new method for the study of fluctuations of attention ; V. Roncagli, Experimental investigations by the method of the maze ; G. A. Elrington, The expression of the musical intervals ; L. Botti, Psychological observations on the concept of the 'last ;' F. Kiesow, A phenomenon of central representation (assimilative illusion) ; F. Kiesow, A forgotten experiment (Fechner's rivalry between the dark field of a closed and the light field of an open eye) ; A Gemelli, G. Tessier and A. Galli, The perception of the position of the body and of its derangements : a contribution to the psychology of the aviator. General Review : A. Gemelli, The application of psychological methods to the study of aesthetics. Notes. E. B. T.

SUBCUTANEOUS SENSATIONS

In the spring of 1920 I met with a minor accident that gave opportunity for observation of the sensations localized in the muscles when directly stimulated. The thumb and first finger of the left hand were cut almost completely through at the first phalange and the second or middle finger was completely severed through the second phalange. No pain was connected with the occurrence. As a matter of fact several seconds elapsed before the injury was called to my attention by the loss of the usual sensitivity of the tips of the thumb and fingers. The lacerations were cleansed with sterile water before the experimentation, in order that the sensations might not be clouded by the effects common with the use of most germicidal preparations. The flow of blood was effectually stopped and the surfaces of the wounds were kept fairly free during the series of observations.

It was anticipated that any manipulation of the exposed surfaces would be painful to some extent; but pain was not observed, and adaptation was rapidly made to the new conditions. It was a rather excruciating feeling that was experienced, especially when large areas were stimulated by contact. The resultant sensations were at once recognized as possessing some of the qualities peculiar to the subcutaneous sensations, and it was attempted to take advantage of the circumstances to make an investigation of the nature of the sensations originating in the muscles.

The sensations resulting from the stimulation of the stump of the second digit and the proximal surfaces of the other two lacerations were very similar to the feeling ordinarily experienced when the skin is anaesthetised and pressure applied; but the resemblances were scarcely more noticeable than the differences. To compare the sensations directly does not convey an adequate conception of the nature of the sensations experienced when the muscle itself is stimulated. The sensations obtained in this manner were to some extent more intense, more noticeable, more distinct, and qualitatively somewhat different. The skin of the right forearm was anaesthetised and the two sensation-complexes were directly compared. Those arising in the bared muscles seemed to be more intimately related to the body, while the others were, comparatively, more aloof, more external, and seemed to be less a part of my ‘ self ’ than the former. The sensations originating in the anaesthetised area were qualitatively more complex; although, when a large area was stimulated on the bared muscles, the quality seemed to become increasingly complex with an increase in area or pressure or with a violent manipulation. The sensations arising from light stimulation of certain small areas on the exposed surfaces by the tip of a probe were of a nature that leads me to believe that in them I experienced isolated muscle-sensations simple and uncomplicated. There were only certain points at which these apparently elementary sensations could be aroused; but whenever these points were stimulated the unique quality was clearly perceived. Stimulation was found to be most efficient when the point was irritated by bringing a slight pressure to bear, at right angles to their axis, on neighboring fibres. The sensation can be described only by reference to the entire cutaneous and subcutaneous complexes. As a matter of fact its quality almost evades description.

The dull, deadened feeling characteristic of the sensations underlying the anaesthetised area does not carry over to the description of what appear to be the clean-cut sensations found by manipulation of the muscles themselves. The latter sensations, while not acute, are not heavy or torpid, but rather are keen and brisk, though accompanied by a ‘ smouldering ’ feeling. They are sharp and distinct, but not in the least painful or unpleasant, unless it be at first when they are novel and unexpected. They cannot be compared to the tired feeling following upon excessive muscular activity, as the muscle-sensations have been. On the contrary, a bracing feeling like that going with good physical tone serves better to convey some idea of the nature of these sensations. They are bracing but bland. They are not diffuse, like the sensations arising from pressure on the anaesthetised area, but are fairly compact and unified.

A peculiar error of localization was observed. Only a very few of the stimulations were referred even to the approximate point of stimulation. They were usually referred to some point on the tip of the finger. In one instance the stimulation was localized at a definite point on the palmar surface of the finger tip, when in reality it was applied near the back. This one error was repeated several times, when the same point was stimulated without my being aware of the place of contact. The sensations leading to these faulty localizations were of a more complex nature than (and of a different quality from) what was considered to be the elementary sense-quality. These errors of reference still persist in the second digit, and it is now possible to hold the stump rigid and yet to have a definite feeling of flexion at the distal joint.

Similar observations of the nature of these deeper-lying sensations were carried out two months later, with the *abductor pollicis brevis* of the right hand uncovered, and essentially the same qualitative characteristics were noted as before, accompanied by even more marked errors of localization. In both regions the limen for temperature was very high, and at only a very few scattered points were temperatures that were not physiologically harmful perceptible.

These observations of difference in quality of the subcutaneous sensations when directly stimulated from their quality when investigated in the usual manner would seem to indicate that in the latter case we are still dealing with a complex from which only the surface sensations have been eliminated.

University of Iowa DONALD A. LAIRD

THE INSTITUTE OF PSYCHOLOGY AT PARIS

At the University of Paris there has been inaugurated this year the Institute of Psychology under the Faculty of Sciences. The following courses are scheduled: Delacroix, Psychologie générale, "La Psychologie française au XIX siècle;" Dumas, Psychologie pathologique et expérimentale, "Introduction générale, theories et méthodes" and "Les suppléances sensorielles;" Janet, at the Collège de France, Psychologie expérimentale et comparée, "L'évolution de la personnalité;" Piéron, Psychophysiologie générale, "Études des sensations;" and Rabaud, "Introduction à la psychologie animale."

The Section of Pedagogy of the Institute offers a series of conferences at the Sorbonne by Rabaud, Meyerson, Piéron, Lalo, and Wallon; a laboratory course on neuro-psychiatric children at the Bicêtre; and a course on retarded children at the Asnières by Roubinovitch.

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AN EXPERIMENTAL INVESTIGATION OF THE POSITIVE AFTER-IMAGE IN AUDITION[1]

By Homer Guy Bishop

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Our problem is to find out if there is a positive auditory after-image, analogous to the positive after-image in vision, and in that case to describe it in attributive terms. If we detect the presence of auditory experience after the stimulus ceases, we must seek to identify this experience as after-image, or memory after-image, or memory image, or whatever it may be.

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[1] From the Psychological Laboratory of Cornell University.

Historical

The first experiments upon the after-effect of auditory stimulation appear to have been made by Mayer.[2] The sources of tone were tuning-forks supplemented by resonators. The tones were conducted to the one ear by means of a rubber tube; the other ear was plugged with wax. Between the nipple of the resonator and the free end of the conduction-tube stood a siren disk. When a space between the holes in the disk was before the nipple, the sound waves were blocked; when a hole was before the nipple, the sound passed into the tube at full intensity. As the disk was rotated, short periods of stimulation alternated with equal periods without stimulation. The critical value sought was the length of the interval between tones which would just be bridged, without loss of sensible intensity, by the after-effect, so that the *O* should hear a smooth tone.

Urbantschitsch[3] worked by a similar method. The tones were interrupted by a pendulum, which carried the one end of a conduction-tube to and fro past the limbs of a Y-tube. The tube to *O*'s ear, which completed the conduction system, was connected to the stem of the Y-tube. Unlike Mayer, Urbantschitsch sought to determine the critical interval at which the tones would just fall apart. The aim of the experiments was to measure the full duration of the after-effect, whereas Mayer measured only that part of it which showed no decrease of sensible intensity.

Schaefer[4] criticizes the method of interruptions on the ground that it cannot take separate account of the *Abklingen* and *Anklingen* of the tones employed. Marbe[5] also considers the method inadequate. " An investigation of the facts of *Abklingen* in auditory sensation can not be made . . . by means of successive, periodic stimuli, but only by the more difficult means of isolated auditory stimuli."

Urbantschitsch[6] further studied what he calls " primary " and " secondary " positive auditory after-images. The " primary " after-image is so closely joined to the sensation that there is no perceptible break between the two; the " secondary " after-image follows the sensation only after an interval, and may recur several times, at intervals of varying length. Urbantschitsch's method is so imperfectly reported that it is impossible to undertake a repetition of his experiments. We are told only that three tuning-forks were sounded at four intensities (characterized as " very strong," " strong," " moderately strong," and " weak ") for periods of 5 and 15 sec.

EXPERIMENTAL

We employed two sources of tone: Stern variators, with tube-transmission, and vibrating telephone receivers. It may be said at once that the principal difficulty with either source was to secure a clean cut-off of the stimulus-tone. Our successive arrangements, therefore, represent repeated efforts at improvement in this respct. The experiments were performed in a suite of three rooms. It was necessary to spread the apparatus over a wide space on account of the power of tones to penetrate even heavy stone walls. Accordingly the *O*'s sat in one room, *E* operated the apparatus in another, and a large unoccupied room lay between. Even so we found that the sources of the higher pitches at greatest intensity must be enclosed in soundproof boxes.

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[2] A. M. Mayer, " Researches in Acoustics," *Amer. Jour. of Science and Arts,* 147, 1894, 1 ff.

[3] V. Urbantschitsch, " Ueber das An- und Abklingen acustischer Empfindungen," *Archiv. für d. ges. Physiol.,* 25, 1881, 323 ff.

[4] K. L. Schaefer, Nagel's *Handb. der Physiol.,* 3, 1905, 507.

[5] K. Marbe, " Akustische Prüfung der Thatsachen des Talbotschen Gesetzes," *Archiv für d. ges. Physiol.,* 100, 1903, 557.

[6] V. Urbantschitsch, *op. cit.;* and " Zur Lehre von der Schallempfindung," *Archiv für d. ges. Physiol.,* 24, 1881, 585 ff.

Series I

In our first series of experiments the source of tone was the variator. Four pitches were used, 1024, 512, 256, and 128 vs. The variators were blown at three intensities, and for three durations of stimulation. The three intensities were obtained by setting the air-pressure at 15.0, 7.6, and 4.0 mm. of water for the pitch of 1024 vs; at 5.0, 2.6, and 1.6 mm. for 512; at 19.0, 11.6, and 8.0 mm. for 256; and at 8.0, 5.5, and 4.0 mm. for 128. The nozzle of the variator was so adjusted to the mouth of the cylinder that a maximum of intensity should require the lowest possible pressure. We found that the pressures could not be equated, but varied from instrument to instrument, as indicated by the pressure-values just listed. The durations were 5, 15, and 30 sèc. read directly from a Pye clock. Since the seconds-hand of this instrument moves over a dial of 11 cm. diameter, the spaces between seconds are broad enough to admit of accurate reading.

A special control enabled us to adjust the air-pressure quickly and accurately. A wooden lever 85 cm. long, screwed to the handle of the rotary valve, swung before a ruled pressure-scale. From one side of the lever and 60 cm. from the valve, two metal arms (2 cm. long and 0.5 cm. broad) extended parallel to each other and at right angles to the edge of the lever. They were 4 cm. apart, and a fine wire was drawn taut from the outer end of the one to the outer end of the other. This wire, parallel to the edge of the lever and to the lines of the scale, lay on the surface of the scale in such wise that the error of parallax was avoided.

The sound-waves were picked up by a funnel of cardboard and reflected into a brass conduction-tube of 1.9 cm. inside diam. and 0.15 cm. thickness. The small end of the funnel fitted snugly over the end of the brass tube; the diameter of the large end was 15.0 cm.; and the length of an element in its surface was 45.0 cm. This slant is that of an old model of Victrola horn, and is approximately the same as the slope of the funnel in Marbe's *Sprachmelodie-Apparat*. When the variator was set in position, its mouth was in line with the axis of the funnel, just not touching the edge.

The brass conduction-tube was 8.76 m. long. It passed through two walls, across the middle room of the suite, and ended in *O*'s room in an iron pipe 2.3 m. long. The iron pipe, which lay horizontally against the wall and at right angles to the brass tube, served as distributor of the tones to the four booths in which the *O*'s sat. Four nipples were tapped into it, and from these four rubber tubes, of 0.6 cm. inside diam., led to four pairs of stethoscopic ear-pieces. The brass tube entered the iron pipe at its middle, and the nipples were at 36 cm. and 104 cm. right and left. Under these conditions, the intensity at all listening tubes was sensibly the same. The booths in which the *O*'s sat were separated by heavy curtains.

We have described above the valve which controlled the blowing pressure. It was necessary to introduce another valve between the pressure-valve and variator, which should cut off the air suddenly and bring the tone to a sharp ending. The special valve constructed for this purpose somewhat resembled the piston-valve of a cornet. In one position of the piston, the air went straight through the one opening to the variator, while the other opening was closed at the tube in which the piston moved. In the other position, the passage was reversed; the hole which had been open was now closed, and conversely. In this second position, no more air could enter the part of the tube between valve and variator, and the air which was present under full blowing pressure when the valve first closed had two outlets, the one by way of the nozzle of the variator, the other by way of the passage through the valve. Since the latter passage offered the lower resistance, most of the pressure was spent in this direction, and the terminal "whoop" which otherwise was very troublesome to the *O*'s was reduced to the vanishing point. (Unless the air escaping from the tube, in the process of reduction of the pressure within it to atmospheric pressure, can be diverted from its course through the nozzle, the pitch of the tone in dying falls quite perceptibly as the pressure diminishes.) The diam. of the holes through the piston, 0.6 cm., was the same as the inside diam. of the rubber tube carrying the air to the valve; so that the piston need be moved no more than 0.7 cm. to open the one passage and close the other. In the first series of experiments we threw the valve by hand.

As warning signals we used four small 4-volt incandescent lamps placed upon the wall before the *O*'s. The circuits were so arranged that the light could be made to glow at two intensities. The lamps were turned on at the lower intensity at the beginning of the experimental hour, and remained at this intensity throughout, except when they were flashed as signals; at the lower intensity they showed merely a dull red glow. Our object was, by keeping the filament constantly warmed, to increase the sensitivity of the lamp. We needed to have it flash brightly in an instant, an impossible result if the filament were cold at the outset. *E* closed a key to flash the usual "Ready," "Now" signals. When the air was turned on at the valve, the same movement closed a mercury-contact key attached to the piston of the piston-valve, and the lights burned brightly. When the air was cut off, the movement of the piston broke the circuit, and the lights dropped to the lower intensity. This method secured temporal coincidence of the cutting-off of the tone and the dimming of the lamp.

Every *O* had at his hand a key making or breaking the circuit in the recording apparatus. This apparatus consisted of eosin writers adapted to use with ticker paper. They were made of tin; and though different in structure their principle of operation was that of the draughtsman's ruling pen. They wrote from the horizontal position, withstood hard knocks, and did not spill the fluid when tapped rapidly against the paper. When *O*'s key was closed, the tip of the writer was pulled against the paper by an electromagnet, and a line was written representing the length of any after-effect which might be experienced. When *O* was ready for the next tone he tapped the key as a signal to *E*. The writers were so slender that we were able to write one time-line and four record-lines upon the usual ticker paper 1.8 cm. in width.

The dashes in the time-line were made by a Kronecker interrupter vibrating in tenths of sec. To fix the exact moment at which the tone began, we resorted to another mercury-contact key upon the piston of the piston-valve. When the air was cut off, the circuit was closed, and the time-writer wrote a continuous line; when the piston moved to turn on the air, the circuit was broken, and under power from another circuit the interrupter-point began to write tenths of a sec. The paper was drawn by a motor which ran throughout the experimental hour; but the feed of the paper was controlled by a clutch, and it was drawn past the writer only when needed. With this arrangement there was no lag of the paper; it was drawn immediately at full speed.

Observers. The *O*'s were Mr. R. T. Holland, graduate student and assistant in the department; Miss M. F. Martin, Dr. C. W. Perky, Miss A. H. Sullivan, and Mr. S. Takaki, graduate students majoring in Psychology. Observer P was somewhat more highly practised than the others, but all were experienced *O*'s.

Instructions. "You will hear a tone which will begin when the signal light brightens, and will cease abruptly as the light dims. You are to note whether the tonal sensation continues or recurs after the dimming of the light. If it continues, you are to press the key immediately and release it at its final disappearance. If it recurs, you are to press the key at every appearance and hold it until the disappearance of the recurring sensation. When the experiment is over, you will write a descriptive account of the after-sensation or after-sensations on the blanks provided."

For every pitch there were 9 stimulations presented in the order: strong-short; middle intensity-middle duration; weak-long; strong-middle duration; strong-long; middle intensity-short; middle intensity-long; weak-short; weak-middle duration. This series of 9 stimulations was run off first with the highest pitch, then with the remaining three pitches in descending order. There were thus 36 experiments in a series. Every series was repeated five times, giving a grand total of 180 experiments for every *O* with this first arrangement of apparatus.

Results

Our results showed a considerable number of cases in which the tones "ended abruptly," were "chopped off," with no trace of terminal modification or after-effect. Besides these, however, there were cases described in which the ending of the tone was modified, in quality, in intensity, or in temporal course. The *O*'s spoke of "slow dying," "prolonged ending," "not abrupt," a "whoop." Never did the modified ending carry sensibly beyond the dimming of the light, or suggest to *O* the continuation of the sensation in a positive after-image.

Table I is a summary of all modified endings. Unfortunately, we cannot be sure of their interpretation. The slow cooling of the filament in the lamp may have led to error in judgment on the part of the *O*'s. The apparatus could not guarantee that what was heard was the natural ending of a suddenly stopped tone; so that these modified endings may in fact have followed the last vibration, and thus may represent an after-effect. Besides this serious defect in the apparatus, our first series of experiments indicated that several minor changes were necessary. Noises in *E*'s room reached the *O*'s through funnel and conduction tube; the purr of the motor which drew the ticker paper had in it a note that was easily mistaken for an after-image; the hand-operated valve was not always thrown with equal speed from the one position to the other; and any echo of the tones in *E*'s room might reach the *O*'s.

TABLE I

	Intensity			Duration			Pitch				Total
0	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	22	18	7	17	14	16	18	14	14	1	47
M	30	27	29	29	32	25	28	20	20	18	86
P	36	29	22	27	30	30	24	23	27	13	87
S	37	32	38	39	38	30	35	34	16	20	107
T	30	25	20	26	26	23	22	19	23	11	75
Total	155	131	116	138	140	124	129	110	100	63	402

Experimental: Series II

We chose the lights as signals because they were silent; but they were not reliable; and we now substituted for them small electromagnets from a player-piano. These magnets are not noisy; and when operated by a weak current, just strong enough to produce a light, quick movement, they did not annoy the *O*'s. To the armature of the magnet we soldered a strip of tin, 4 cm. by 0.5 cm., to serve as signal flag. Every *O* had a magnet and flag in his booth before him, at the place where the light had been. When the current was off, the flag stood almost vertically; when it was turned on, the flag was pulled quickly down to the horizontal.

In order to operate the piston-valve quickly and at a constant speed at all times, we built two solenoids end to end on a single base. They were mounted at the end of the piston, and an extension of the piston passed through them both. One solenoid pulled the piston to turn the air on, the other pulled it back again to turn the air off. With a sufficient current the shift was almost instantaneous.

We shut out the noises from *E*'s room by placing the variators in a sound-proof box. We cut the funnel down to a smaller size (diam. 10 cm., length 25 cm.) for use inside the box. The whole conduction system was now isolated, though there was still the possibility of echo from the walls of the box. Indeed, we found that there was a faint tone which made its appearance regularly at a short interval after the air was cut off. We satisfied ourselves that this tone was in the apparatus, and was not subjective, by making records of the vibrations. A diaphragm of the Marbe *Sprachmelodie-Apparat* was held firmly against *O*'s end of the conduction tube, and the smoke-rings registered the tone.

To rid ourselves of this source of error, we so hinged one of the sections of brass conduction-tube that the one end of it could be drawn aside. The other end was attached by a heavy rubber tube to the fixed end of the next section of conduction-tube. An electromagnet in series with the solenoid which cut off the air to the variator pulled this swinging section of tubing to one side at the same time that the variator ceased to be blown. In this way the echo was diverted into the middle room of our suite, and the *O*'s could not hear it. We found it very difficult to swing the pipe noiselessly, but we finally hit upon the following arrangement. One end of a thin strip of wood, about 1 m. long, was fastened to the floor with a hinge; the other end carried the swinging end of the conduction-tube. The electromagnet was fastened to a firm support about 30 cm. from the floor. A piece of iron, screwed to the thin strip at this level, was attracted by the magnet, and the pipe was thus drawn aside. The iron was faced with felt; and by this means, supplemented by the elasticity of the strip of wood, the pipe was brought to rest silently in its new position. The supporting wooden strip stood so nearly vertical that it was easily pulled toward the magnet; but it was inclined so far away from the vertical that gravity pulled it back to the first position when the circuit to the magnet was broken. A piece of spring steel, faced with felt where it touched the wooden strip, furnished a silent stop for the pipe when it returned to the position for conduction. The swinging section was 1.82 m. long; with a radius so large, and an outside diam. of the tube of only 2.2 cm., the space between the swinging end and the fixed end of the next section could be about 0.25 mm. The space was adjusted at every experimental hour so that the ends should just not touch. The break in the pipe was 84 cm. from the point at which the brass tube joined the iron pipe in *O*'s room. We felt satisfied that, with a distance so short as this, no residual sound-waves remaining in the conduction system between the *O*'s and the break in the tube could have any effect for audition.

In the light of our first results, it is clear that the warning signal must be very accurately timed. The modification in the ending of the tone was so short that the flag must move in exact coincidence with the last sound-wave. We turned again to the Marbe apparatus. It was set up with two diaphragms and one gas-burner, all in the same gas system. One diaphragm made the tone-rings, the other the ring from the flag. The diaphragm at the flag was brought so near the flag that the first beginning of movement was recorded by the membrane. By this arrangement the effects from the two membranes were superimposed. The record should show a band of regular rings representing the tone, and then an aperiodic ring written by the flag, followed by a band with no rings. By carefully adjusting the contact placed where the swinging section of conduction-tube would close the circuit to the flags, we were able to secure this kind of record. The heavy ring from the flag showed as the last ring in the series, and it stood at the proper distance from the last ring but one to be in coincidence with the last tone-ring. To be sure, the record did not indicate whether the heavy ring was the result of a summation of two rings; but it did show that the flag was not more than one vibration out of step with the tone (256 vs.). Still, however, there was the possibility that the stroke of the flag blotted out the tone-rings; tone might be present, but unrecorded. We put this possibility to the test by allowing the flag to make its ring without cutting off the tone. We found that the tone waves were just as distinct in the record after the stroke of the flag as they were before. The disturbance in the flame set up by the flag did not blot out the effect of the tone, but gave a superposed effect.

With these improvements in the apparatus we were ready for the next set of experiments.

Observers. The *O*'s were H, M, S, T of the previous group.

Instructions. " You will hear a tone which will begin when the signal flag falls, and will continue for a varying period of time. About a second before the tone ends, the flag will fall as a warning signal that the tone is about to end. Just at the instant that the tone ceases abruptly, the flag will fall again. You are to note whether the tonal sensation continues or recurs after the flag falls. If it continues, you are to press the key immediately and release it at the final disappearance of the after-sensation. If it reappears, you are to press the key at every appearance and hold it until the disappearance of the recurring sensation. When the experiment is over, write a descriptive account of the after-sensation or after-sensations on the blanks provided."

The experiments in this series were conducted with the same tones as before, in the same order. Except for the changes in the apparatus, already described, and for the warning signal given during the course of the tone, the method is the same as in the first set of experiments.

Results

The distribution of the modified endings is shown in Table II. The decrease in number between Series I and Series II

TABLE II

O	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	5	1	0	2	3	1	2	0	4	0	6
S	35	28	26	36	32	21	32	30	18	9	89
T	6	2	2	7	3	0	4	2	0	4	10
M		(none)			(none)			(none)			
Total	46	31	28	45	38	22	38	32	22	13	105

seemed to indicate that further refinements might lead to their complete elimination. We, therefore made additional efforts to refine our technique.

Experimental: Series III

So long as the tones were stopped by cutting off the air, there was escape of the air under pressure between the nozzle of the variator and the piston-valve. An unknown part of this air must certainly escape by way of the nozzle; and its escape, if audible, might lead to *O*'s reports of modified endings.

Only after we had discovered how difficult it is to avoid the influence of the diminishing air pressure, did we turn to the rather obvious plan of allowing the variator to blow continuously, while we broke the path of conduction in order to obtain the periods of silence between stimulations. We accomplished this by swinging more sections of the conduction-tube. The section at the sound-proof box was pivoted at the middle to allow the ends to move up and down. The fulcrum was a wad of cotton, packed rather tightly around the pipe in the hole in the stone wall through which it passed. The next section was joined to this lever of conduction-tube and to the third section in the line by rubber tubing fitted over the ends of the pipe. When the end of the pipe in *E*'s room was thrown down, the end in the middle room went up, carrying with it the end of the second section. This movement produced a break in the line at the box, but preserved the continuity in the next room. At the other side of the room was the section adjusted to swing in the previous set of experiments. The swinging end was now supported by a wire and pulley, so that it could be drawn up and dropped down. The means of obtaining simultaneous and equal movement at both breaks in the conduction system was very simple. The wire just mentioned extended across the middle room; its ends dropped vertically down from the pulleys and were fastened to the conduction-tube, the one end to the end of the lever section, the other to the free end of the swinging section. When the end of the lever at the variators went down, the end in the next room went up by an equal amount, since the fulcrum was in the middle. This upward movement allowed the wire to roll over the pulleys, and the end across the room dropped by an amount equal to the movement of either end of the lever section. The displacement thus secured was about 5 cm. The pull upon the pipe was exerted by means of a solenoid fastened to the floor in *E*'s room. A cord tied to the end of the lever-section supported the core at a good pulling distance above the center of the solenoid. With a strong current, the displacement was sudden and free from noise. We found it necessary, however, to sew the core of the solenoid into a close fitting bag, in order to secure silent action of the core within the coil.

It is obvious that the shape of the ends of the conduction-tube at the break could not be circular. The diminishing amount of area for conduction as the ends became separated might be responsible for an observable decrease in intensity of the tone. The ends of the tube, except for a slit of 1.8 cm. by 0.3 cm., were accordingly plugged with sealing wax. The long dimension of the slits was horizontal, so that a quick movement of the end of the pipe downward brought them wide apart, too rapidly for any sensible decrease in intensity. We tried at first to use four of these slits, but found that they reduced the intensity; two at the sound-proof box were sufficient. The circuit driving the signal flag was again closed by the swinging pipe.

We found that the two high tones could be heard three rooms away, even though the variators were in a box with very heavy double walls, if they were sounded at more than our intermediate intensity. We accordingly decreased the blowing pressure to 2, 4, 7.6 cm. of water for the tone of 1024 vs.; and to 0.9, 1.6, and 2.6 cm. of water for that of 512 vs. The two breaks in the conduction-tube weakened the low variator tones beyond usable limits. In their stead we used tuning-forks (with resonators) of 256 and 128 vs. The resistance in the circuit was such that at 10 v. we obtained satisfactory driving power for the forks at 1, 2, and 3 amps. for 256 vs.; and at 2, 3, and 5 amps. for 128 vs.

Results

The results from this series of experiments are shown in Table III. We reserve comment upon them until later.

TABLE III

	Intensity			Duration			Pitch				Total
O	Strong	Medium	Weak	30	15	5	1024	512	256	128	
S	30	26	20	25	27	24	30	33	7	6	76
T	1	3	4	3	3	2	3	3	1	1	8
Total	31	29	24	28	30	26	33	36	8	7	84

Experimental: Series IV

The apparatus just described was satisfactory except for the fact that the tones were weak. As it stood we could be reasonably certain that the *O*'s heard the natural ending of a tone, the sensible process of transition from tone to silence. The noise incident upon swinging the pipes was, it is true, barely noticeable just after the ending of the tone was heard, and a very faint after-image might have been lost. But the main fault lay with the intensity of the stimuli themselves. We required a source which should have fairly intense tones, and which should be completely silent in the period following the cessation. After a good deal of cut-and-try experimenting, we had recourse to telephone receivers.

The instrument was the ordinary variety of receiver of 75 ohms resistance. It was actuated by a Petzold double induction coil, with a current of 2 amp. at 6 v. through the two primaries connected in series. The vibration frequency in the receivers was determined by the number of interruptions in the primary, and not by a separate source of sound. By this method we avoided the complications which a transmitter would have introduced. The interrupters were tuning-forks of 435, 350, 256, and 100 vs. We were unable to drive the forks satisfactorily by the same current which passed through the primaries of the coil; by means of an extra pair of contacts put upon the forks, we passed two separate currents through them, the one driving the fork, the other leading to the induction coil. A condenser connected across the point of interruption in the circuit to the coil kept down the sparks at the interrupter, and gave a tone free from the usual click and buzz of receivers.

The line across which the receivers were connected derived its power from the secondary coils connected in series. A special switch with tion. After a good deal of cut-and-try experimenting, we had re-breaking both sides of the line at once. When one side alone was broken, the receivers sounded faintly; and they could be made silent only by interrupting both sides of the line together. This same switch also carried contacts controlling the signal flags. When the path to the receivers was broken, the circuit to the flags was made, just far enough in advance to counteract the slight lag in the flags.

The three intensities of stimulation were obtained by setting the secondary coils both at 0; the one at 14, the other at 2; and both at 14. There was no further change in the experimental procedure.

Results

Table IV contains a summary of modified endings from Series IV of 360 observations for every *O*.

TABLE IV

0	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	120	120	0	80	80	80	60	60	60	60	240
S	119	120	113	120	118	114	90	89	87	86	352
T	6	2	1	2	3	4	1	0	4	4	9
Total	245	242	114	202	201	198	151	149	151	150	601

Significance of Results

So far as concerns a positive after-image, analogous to the positive after-image in vision, we have nothing to report. In a total of 3,780 observations, the results have been flatly negative. But our ‘modified’ endings are positive results; and as they have been ineradicable, we are obliged to discuss them.

The instructions laid emphasis upon the positive after-effects of our stimuli; and the different *O*’s took different attitudes toward the endings of the tones. In Series I and IV, H reported the modified endings frequently; in Series II, he was especially attentive to after-effects, and only the more pronounced of the modified endings were reported; in Series III, he made the general statement that he could hear a “ ylup,” if he made an effort to hear it, coincident with the drop of the flag. Throughout the series he took the instructions literally, and reported “ Nothing ” for the after-effect of every stimulus. M never reported modified endings after Series I, and like H never heard an after-effect. T reported modified endings very infrequently after Series I, but he never totally ignored them. S failed always to hear after-effects, but she persistently gave her attention to modified endings.

An examination of the totals in Tables I to IV shows that for every series the frequency of the report of modified endings is directly proportional to the degree of intensity of stimulus. Even in Series III, where the strongest stimulus was rather weak, the rule holds in some measure. The influence of duration is less regular; but there are always fewer modified endings reported at 5 sec. than at 15 and 30 sec. In the case of pitch, the tables may be misleading. In Series I and II, decrease in frequency of modified endings parallels a fall in pitch; but the high tones were intense and the low were weak; so that what might be taken for an effect of pitch is in fact an effect of intensity. We feel assured, from the results in Series III and IV, that this interpretation is correct. In both these series all pitches were of nearly equal intensity, and the distribution of modified endings is also nearly equal for all pitches. The contradiction in Table III is only apparent. It was very difficult to keep the tuning-forks of 256 and 128 vs. vibrating at low voltages, and for this reason we gave only one series with each. At higher voltages the forks had so great an amplitude of vibration that they struck the core of the electromagnet; for this reason it was impossible to grade the intensities from a higher maximum.

Throughout the earlier series, we regarded the modified ending as due to a fault in technique. We believed that refinement of apparatus would remove it. The agreement between number of modified endings and intensity of stimulus supported this opinion; but the influence of duration was against it. In order to explain the effect of duration, we searched the results to find what terms were used in describing the modified ending. Many references to it were not descriptions, but characterizations like "swoop," "whoop," "sigh," "groan," etc. In Series III, H and S agreed that the difference between the tone and its ending was, in part, one of 'vocality,' which changed as the tone went off. S further described the ending as having "less body than the tone;" it "disappeared out in space," "decreased in volume," "thinned out," "lost quality," etc. We have set down the attributive conditions of stimulus under which such descriptive terms were used in Table V. The results are all taken from Series III, since it is in this series only that the *O*'s maintained a constantly descriptive attitude.

The uniform distribution for duration shows that the modified ending is not dependent upon differences in that attribute of stimulus.

TABLE V

INTENSITY

O	Strong	Medium	Weak
H	120	120	0
S	37	41	43

DURATION

O	30	15	5
H	80	80	80
S	40	41	40

PITCH

O	435	350	256	100
H	60	60	60	60
S	33	42	26	20

In the same group of experiments, certain terms indicative of judgments of intensity, like "pronounced swoop" and "less ŭh," were used. Their distribution is shown in Table VI.

TABLE VI

INTENSITY

O	Strong	Medium	Weak
H	120	120	0
S	10	5	3

DURATION

O	30	15	5
H	80	80	80
S	6	6	6

PITCH

O	435	350	256	100
H	60	60	60	60
S	10	2	1	5

The extreme regularity of H's results is due to a 'set' for intensity. He reported only upon the relative intensity of the modified endings, which he found to be well above the limen. The figures show again that the modified endings are independent of duration. In Series I, where S frequently gave judgments of length of modified ending, the same result was found: the modified ending was short for all durations of stimulus.

It will be remembered that in every series every degree of intensity was given once with every duration of stimulus. In Series III, we counted the number of times that S used the same term to describe the modified ending for all three durations in a series or for all three intensities. The number of cases is shown in Table VII.

TABLE VII

INTENSITY

Strong	Medium	Weak	Total
12	8	1	21

DURATION

30	15	5	Total
1	2	2	5

PITCH

435	350	256	100
9	6	2	9

It is evident that duration does not markedly affect the modified ending, seeing that for 12 of the 15 times that the strong stimulus was given *O* is able to call the effect by the same name in spite of the 3 different durations of stimulus. The single case recorded in column 3 is very misleading in that 7 of the 12 weak stimuli were negative, *i.e.,* ended abruptly for sensation, leaving only 5 times that a single term could be used. The table shows that duration had a slight influence in determining the descriptive term; but it is only about one quarter as effective as intensity. The 7 negative cases were distributed, 5 to the short, and 2 to the intermediate duration.

In Tables I to IV, difference of duration appears to be effective in determining the number of reports of modified ending, and may, therefore, be a factor in determining their existence or non-existence. In Tables V to VII, duration does not modify the character of modified endings. Our experiments offered the suggestion that pressure in the ears[7] may have been of importance in forcing attention to the modified ending. T reported pressure or deafness following the stimulus under conditions which are shown in Table VIII.

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[7] On the perception of silence, see E. B. Titchener, "A Further Word on Black," *Jour. of Philos. Psychol. and Scient. Methods,* 13, 1916, 649 ff.

These results are all taken from Series IV, in which the stimuli were sufficiently strong to make pressure and deafness moderately conspicuous. We conclude from them that both intensity and duration co-operate to bring about a state called sometimes deafness, sometimes pressure. The only time that a weak stimulus produced pressure was when its duration was 30 sec. Here, then, is an effect of duration which may be responsible for its otherwise unexplained influence upon the frequency of report of modified endings. Through it, only those modified endings will be reported which force themselves to the focus of attention. Our instructions, however, tended to direct attention to the period following the ending of the tone. Hence we should expect a report of pronounced or conspicuous endings, but of no others.

TABLE VIII

Intensity

Strong	Medium	Weak	
19	18	1	Pressure
4	7	0	Deafness
23	25	1	Total

Duration

30	15	5	
18	12	8	Pressure
6	2	3	Deafness
24	14	11	Total

Pitch

435	350	256	100	
19	4	9	6	Pressure
4	3	4	0	Deafness
23	7	13	6	Total

In order to substantiate this explanation, it becomes necessary to show, by a special direction of attention, that modified endings are, in reality, always present. H had already said that he could hear them with every stimulus, if he tried. Prof. H. P. Weld (W), and Dr. L. B. Hoisington (Ho.), members of the department and highly experienced *O*'s, had previously been called in for supplementary observations following Series III. At first, they reported modified endings, but when they assumed a passive attitude toward them, and directed their attention to the silence following the end of the tone, the number of modified endings observed became fewer. Their reports indicated, nevertheless, a possibility that all endings of tone are modified.

To put the matter to the test, we made a few experiments, with Dr. K. M. Dallenbach, instructor in the department, H, Ho, M, T, and B (the writer) as *O*'s.

Experimental: Series V

The stem of the Y-tube at the ear-pieces was held in the left hand; and the end of the rubber tube, which previously had been joined to the Y-tube, was held in the right. Both free ends were brought together, just not touching, for stimulation, and were suddenly drawn apart to remove the stimulus tone. The movement was made in all possible directions, with equal effect upon the auditory experience; and the thumb held against the end of the rubber tube to cut off the tone had no different result from that noted with movement of the tube. These movements provided a soundless means of discontinuing stimulation. *O* was asked, first, to direct his attention to the change in the character of the tone just as it ended, and to report whether it could always be heard; secondly, to turn his attention to the period of silence following the tone, and to report, from stimulation to stimulation, whether the silence seemed more or less distinct from time to time; and thirdly, to report the times when the modified ending especially attracted attention. Stimulations were given for 5, 15, and 30 sec.

Results

Every *O* was able to hear a modified ending of every stimulation, if attention were directed upon it, even with weak tones carried almost to the limen. Every *O* found that the stimulus of 5 sec. was incapable of producing any but the faintest pressure which is characteristic of perceiving silence. Durations of 15 and 30 sec. definitely produced the pressure; but the *O*'s did not agree whether 30 or 15 sec. produced the greater pressure. Every *O* found also that the modified ending was not insistent after the 5 sec. stimulation, but that it stood out very conspicuously from stimulations of 15 and 30 sec. In B's experience the heaviness of the deeper silence from long stimulation worked, as if by contrast, to make the modified endings stand out more vividly without necessarily becoming more intense. It seemed to be the 'contrast' between silence and modified ending which grew more intense as the stimulus-time was lengthened. Since the pressure is cumulative with the product of intensity and duration of stimulus; and since the intensity of the modified ending seems to depend upon intensity of stimulus; we conclude that the enhanced 'contrast' effect comes by way of increased pressure.

This, then, seems to be the explanation of the apparently contradictory results. The modified ending varies in intensity directly with intensity of stimulus, and is always present. When the *O*'s report abrupt endings, or fail to report the ending at all, their direction of attention to the period of silence is complete; and no endings, except the most vivid, come to their notice. Long stimulations enhance the contrast-effect between tone and silence, and increase the number of times that *O* hears the modified ending; they thus give the impression that the longer the stimulus-time, the more frequent is the modified ending.

Nature of the Modified Ending

It is tempting to regard the modified ending as 'subjective' in nature, and to identify it with the *Abklingen*[8] or 'drop' of auditory sensation which Mayer and others have attempted to measure. This view is supported by the facts that the modified ending varies directly with intensity of stimulus, and that it shows always a thinning-out or reduction of the 'body' of the tonal complex. While, however, we have no wish to dispute the presence of an *Abklingen,* we are nevertheless of the opinion that, in our experiments, this subjective factor was outweighed by objective changes.

In the first place, we mistrust the results from the telephones. So long as the tone is stopped by taking away the driving power of the diaphragm, there is a source of error remaining: the diaphragm must come to rest from the position in which it happens to be when the current is cut off, and this return is definitely audible. Our arrangement with induction coil and condenser served to eliminate the noisy click natural to the instrument at the moment when it ceases to be under the influence of the electromagnet; but it is evident that the very fact of vibration still necessitates a return to a state of equilibrium.

For a similar reason, the results from the variators are not free from objection. When the variator ceases to blow, we have no means of knowing through what stages the physical stimulus passes in coming to rest. At one moment the air is in motion, and a moment later it comes to rest. Our modified endings may depend in part upon objective change in the sound waves. When the path of conduction is broken by pulling the conduction-tube aside, eddy-curents must be set up around the ends of the tubes; and these, though not audible at low intensities, may change the character of the stronger tones as they end.

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[8] For *Abklingen* in general see C. Stumpf, *Tonpsychologie,* 1883-90, references in index: K. L. Schaefer, Nagel's *Handb. der Physiol.,* iii., 1905, 504 ff.

There are, indeed, positive indications that the modified ending is in part objective. (1) The results from Series I-III differ widely with respect to the number of times the pitch changed in any direction. A summary is given in Table IX.

TABLE IX

	Series		
	I	II	III
Pitch rises	6	25	48
" the same	48	15	23
" falls	11	7	5
Total	65	47	76

In Series I, the vibration in the air spent itself normally. In Series II, when the pipe was drawn aside, the path of conduction was interrupted; and eddy-currents at the break may have changed the character of the ending of the tone. The ends of the tube were not plugged; and even though the pipe moved quickly, a certain brief time was required for the movement. In Series III, the ends were plugged, and the movement of the pipe was quicker than in Series II. The eddy-currents would be more intense, owing to the greater speed with which the pipe moved and to the narrowed opening in the pipe. Table IX shows that the increase in the number of judgments of higher pitch, and the decrease in the number of lower pitch, are correlated with the suddenness of the ending of the tone and the violence of the movement of the pipe. (2) We made a few experiments as a check upon the implication of Table IX by the method of pulling the rubber tube away from the stem of the Y-tube at the ear-pieces. When the end of the rubber tube vibrates rapidly before the end of the Y-tube, the movement produces a distinct thud or noisy puff. If the movement be made sufficiently violent, the noise covers up the tone. B, H, Ho, M, and T all reported that the modified ending was most intense, at any intensity of stimulus, when the rubber tube vibrated 4 to 6 times per sec. This result proves that long stimulation does not add to the intensity of the modified ending, but that rate of vibration does, probably from the increase in energy of the stimulus due to interference of air-waves at the ends of the tubes. (3) B and Ho made a number of observations to determine the pitch of the modified ending, and found that it is dependent upon the rate of movement of the end of the tube. A quick movement gives an " oop " higher in pitch than the tone; and a slower movement gives the same kind of sound, but its pitch is lower, and it is softer and has greater volume. Certain rates of movement give a pitch that is equal to that of the generator. Both forks and variators were used as stimuli. If these observations, made under loose experimental conditions with hand-control of the speed of movement, may be trusted, it is possible that we have an explanation of the results in Table IX; for the judgment of pitch of the modified ending is correlated with the objective suddenness of the movement which interrupted the tones. The observations also furnish additional evidence that the modified ending may be objective;[9] and that, if a means of interrupting tones could be found which did not affect the air in the path of conduction, it might in so far be eliminated. It may, however, be impossible to deprive a tone of its normal means of propagation, the air, without at the same time setting up disturbances in the air.

Such results as ours call for further experiments conducted under the best possible conditions. As our problem was to demonstrate the presence or absence of an auditory positive after-image, the modified endings are incidental to our purpose; and it is unnecessary that we should make a more complete investigation of the conditions upon which they depend. Since, however, their occurence, in so far as it represents an objective disturbance of stimulus at cessation, might give ground for the objection that a possible faint after-image has been covered up and overborne, and thus has escaped the notice of our *O*'s, we have made some further experiments with tones of greater intensity and longer duration.

Experimental: Complex Tones

In order to extend our results to other kinds of stimuli and to long stimulation-times, we conducted a group of experiments with more complex tones. In some experiments the original stimulation-times were used, and in others the time was extended to 5 and 15 min. The tones were *C′* and *C″* from organ pipes of metal; *C* and *C′* from organ pipes of wood; and tones number 2, 4, 6, 8, and 10 from an Appunn reedbox of the overtones of a fundamental of 64 vs.

Our *O*'s in this group were Miss E. C. Comstock (C), graduate student in psychology; Mr. F. L. Dimmick (D), assistant in the department; H, Ho, M, S, T; Mrs. A. K. Whitchurch (Wh), graduate student in psychology; and a class of 7 students in the advanced laboratory course.

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[9] It was noticeable from the very first that *O*'s who were observing in the same group did not by any means necessarily report in the same way upon the same stimuli. We have already shown that they varied in their attitude, attending sometimes to the tone's ending and sometimes to the period immediately following. The objective or partially objective character of the modified ending is, therefore, not put in question by the variety of report. Moreover, as we have also shown above, prescribed direction of attention led to uniformity of report.

All tones were blown at full intensity, at the different durations. The *O*'s sat where they could see the *E*'s hand move as the tone was cut off. This movement replaced the signal flag of previous experiments. In all, 448 observations were made. Of these 11 were with 15 min., and 7 with 5 min. stimulation.

Results

Not an after-image was reported; but our practised *O*'s heard modified endings, and felt pressure in the ear from long stimulation.

Our regular experiments had indicated that pressure in the ears made its appearance in place of an after-image, and we wanted striking evidence that this indication was true. If naïve *O*'s could be brought to sense the pressure, without practice, this result would be good evidence of its fundamental nature. Accordingly, our unpractised *O*'s were instructed to describe any after-effect of stimulation that might appear. They were then given number 2 on the reedbox as stimulus for 15 min. All of the group, except one who misunderstood the instruction, reported vivid, throbbing pressure which lasted from 5 to 10 min. after stimulation. Nothing was heard. One *O* remarked that there was no after-image, unless the throbbing were a negative after-image; and added that in this condition the ears "felt very sensitive to noise." If 15 min. of stimulation resulted in so perceptible a pressure, it may be supposed that the naïve *O*'s who were able to report it were also capable of perceiving any tonal after-effect, and that their failure to perceive tone is further evidence that no auditory after-image exists. The case of heightened sensitivity to noise may help to explain why, in the regular series of experiments, long stimuli led to more frequent report of modified endings.

Auditory Recurrent Images

For T, the tone frequently recurred after stimulation. S reported only a single recurrence, the other *O*'s none. The attributive correlates of T's recurrences are given in Table X.

The recurrences were generally 1 to 2 sec. in length, but sometimes were as long as 10 sec. More than a single recurrence was not reported after the sources of tone had been placed in sound-proof boxes. There were numerous 'recurrences,' as many as seven, when the purr of the motor could be heard through the conduction-tube. These recurrences are, of course, not trustworthy. As a rule, the recurrent tone had the same pitch as the stimulus; but it was sometimes one or two octaves above or below. Localization was generally in the head, though sometimes recurrences were unlocalized. B found that, after serving as *E* for one or two hours, the recurrent tone came as an addition to any faint continuous objective sound, but that in a quiet room no recurrent image could be heard. Our recurrent images obviously resemble the " secondary " after-sensations of Urbantschitsch.

TABLE X

	Intensity			Duration			Pitch			
Series	Str.	Med.	Wk.	30	15	5	1024	512	256	128
I	10	11	3	8	9	7	1	8	11	4
II	0	1	1	2	0	0	0	1	0	1
III	8	3	0	3	5	3	5	3	0	3
							435	350	256	100
IV	5	1	1	4	2	1	2	1	4	0
Total	23	16	5	17	16	11				

In Tables III and IV, T's results are often not in agreement with those of the other *O*'s; but it will now be seen that the disagreement is due to the number of recurrences at higher and middle intensities, or in long and middle durations. The unequal distribution for pitch in Table X may be due, in part, to the influence of the register of T's voice; the recurrences are most frequent within his singing range.

Conclusions

1. There is no positive after-image of tone, analogous to the positive after-image of vision.

2. The 'modified ending' of tones, which we have been unable to eliminate, is probably a compound effect, due in part to tonal *Abklingen,* in larger part to the objective conditions of our experimental arrangement.

3. Intensity of modified ending is dependent upon intensity of stimulus; its vividness or insistence upon pressure in the ear, which increases with intensity and duration of stimulation.

4. It is impossible from our experience to say which of our two sources of tone, the variators with air conduction or telephone receivers, is to be preferred. The variator tones were relatively weak; when they are cut off, slight noise accompanies the swinging of the conduction-tube; if the conduction-tube remains unbroken, echo is a source of error. They furnish, however, an almost purely tonal *Abklingen.* The telephone receivers give tones approximately equal in intensity at all pitches; in the period when the after-image is expected to appear, they are absolutely noiseless; there is no difficulty from echo; but the *Abklingen* is very impure, owing to an admixture of noise from the diaphragm.

THE INFLUENCE OF COLOR UPON MENTAL AND MOTOR EFFICIENCY*

By Sidney L. Pressey

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I. Problem

The problem of the study may be put as follows: Do different hues and brightnesses of general illumination have (*aside from* the obvious importance of these factors in conditioning the effective functioning of the visual apparatus) any specific influence upon mental or motor efficiency? The subject is obviously of great practical importance. If adequate lighting in factory or office tends to stimulate activity and increase the amount of work done, as well as to aid in visual efficiency, the matter is of no slight moment from a business point of view. If, as is often supposed, an office or school-room furnished in red has a distinctive effect, upon mood and upon sustained capacity for work, as compared with a room furnished in light green or buff, then the definite demonstration of that fact would be of decided practical value. The aim of the present study was to investigate experimentally, in the laboratory, this question as to the effect of brightness and of hue upon mental and physical work.

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* From the Psychological Laboratory of Harvard University. The paper is a summary of the writer's thesis for the doctorate, which was presented in April, 1917, but which, because of pressure of other work, it has not been possible until now (October, 1919) to condense and arrange for publication.

II. Previous Work Bearing Upon the Subject

As thus conceived, the study differed, in certain important particulars, from other investigations in this general field. In the first place, color combinations were not considered. In the second place, large surfaces, involving the subject's entire field of vision, were used, not small stimulus-areas (usually the subject worked in a room illuminated solely by a light of the desired brightness or hue). In the third place, the interest was in objective measurements of mental work and neuro-muscular tone, not in introspective reports. Finally, as has already been mentioned, the study was not concerned with the effects of hue or brightness upon visual acuity or ocular fatigue; any such effects were carefully avoided. Previous studies in the aesthetics of color have, however, most important bearings upon the writer's problem. A consideration of these previous studies may be conveniently taken up under three heads, according as the work was (*a*) introspective or observational, (*b*) experimental, or (*c*) analytical and critical.

(*a*) *Introspective and Observational Studies.* The earlier work treats rather of the artistic values of the different colors than of the narrower problem of the relation of color to affection. It is impossible in brief space to summarize the mass of this material. It may be said, shortly, that introspective consideration of the matter seemed to indicate that each color conditioned a specific emotional and affective response of great richness.

Goethe's "Farbenlehre" is the most important early treatment. There are "active" and "passive" colors. The hues from yellow through red are exciting, lively, arousing, the climax being reached in the red. From blue back to red is the minus or passive side; and at green is a balance of arousing and depressing influence which gives a feeling of peace and comfort. There is thus a complex and intimate relation between color and affection.[6] Such theories find their best known and most systematic formulation in Wundt.[40]

Anecdotal data with regard to color,[38] anthropological observations,[15] and studies in the history of art,[16] may also contribute to the problem. Savages, and also many animals, show a marked fondness for bright colors and for shiny, brilliant objects. As evidences of the same tendency in civilized man may be cited the fondness for diamonds and other gems, for fire-works, for lacquer, gilding, boot-blacking, and the adoption of shiny metal for money.[15] Among the hues red has an outstanding position; the power of red to attract attention and arouse action, in both men and animals, is made much of.[10] However, there may be striking differences in the affective character of a hue, from one locality to another or one period to another. Thus yellow was in classical times a favorite color, and is now least liked.[11]

(*b*) *Experimental Studies.* Midway between such observational studies and the laboratory research comes a mass of more or less roughly controlled investigations, mostly with children, of which the monographs of Miss Shinn,[34] Mrs. Moore,[26] Preyer,[29] and Major,[24] and the papers of Winch,[39] Wells,[37] Jastrow,[18] may be mentioned. The gist of the results may be put in a sentence. There is an early fondness for bright colors, decreasing with age; an especially rapid dropping back of yellow, ending in least liking; in general blue and red are liked best.

The laboratory work may be summarized quite as briefly. There is an almost absurd difference of opinion among many of the experimenters as to the affective value of the various hues. And experimental studies of special problems in the aesthetics of color show little more (relevant to the present problem) than the extreme complexity of many reactions to color, and the multitude of factors which may play a part.

Cohn[7] states that his is the first experimental study of the aesthetics of color. He placed his subjects in a dark chamber, and used for stimuli small gelatine plates; the illumination was by daylight. He found, briefly, that saturated colors were preferred, but that as between different hues of equal saturation the choice was a matter of individual idiosyncrasy. On the whole, yellow was least liked; but the data are not considered adequate for any generalization in the matter. Cohn had seven subjects, and worked by the method of paired comparisons. Major[25] used colored papers, and a different method. He had four subjects. Neither antipathy for yellow nor preference for saturation was discovered. Cohn[8] thereupon experimented further, and confirmed his previous results as to saturation. Miss Baker[2] found the warm end of the spectrum most pleasant. Fernberger[13] found relative dislike for yellow and yellow-green. Miss Washburn[36] found that thirty-five college girls liked tints best, shades next, and saturated colors least. Blue was preferred, of the hues.

Some few papers have appeared which try to deal experimentally with the apperceptive elements in color attitudes. Thus Bullough[4] worked on the question of the apparent heaviness of colors. He concluded that a color looks light or heavy according to its luminosity, stressing this element rather than any association of dark and low objects, or analogous explanations. Pierce[28] found that bright colors "weigh" more than dark ones in making up an ornamental balance; he explains this on the basis of the "action theory" and greater demand of the bright objects for eye-movements. Quantz[30] and Larguier des Bancels[20] agree that color affects the apparent size of objects. Thus red surfaces look larger than equal blue ones. But no satisfactory explanation is offered.

Investigation by the method of expression has led to similar contradictory results. Or rather, it may be said, that the best work of this type has given negative findings.

First to be mentioned of such studies should be Féré,[12] who, while experimenting with hysterics, found the most remarkable differential reactions to different tones, different tastes, different smells, and finally to different colors. If the patient was shown a red light there was an increase in pulse, a disturbance of breathing, a distinct increase in muscular strength as shown by the dynamometer. And, since he considered his neurotics to exihibit in marked degree what was true in kind of normal individuals, Féré concluded that, with normal individuals also, red had a dynamogenic effect.* A Leipzig study by Stefanescu-Goanga[35] comes to a somewhat similar conclusion. Striking results, giving confirmation of the Wundtian theory of the correlation of bodily changes and tri-dimensional feeling, were obtained. However, McDougall[23] found no specific effect of either hue or brightness upon reaction time.† Angell and Thompson[1] found no evidence of any specific association between organic changes and various visual stimuli. Shepherd's very careful work also failed to yield any evidence of such a connection.[33]

(*c*) *Analytical and Critical Studies.* The great importance of social influences, of literary and religious symbolism, and of language, in developing affective reactions to color is the one point constantly stressed by the more critical writers. That other than intrinsic values are important is apparent from the way color preferences change in art, and even with famous artists during their lifetimes. The effect of a color may be totally changed by slight change of circumstances. And such affective toning, though the result of previous associations, need by no means show any trace of those associations. The ideas and feelings have grown together, amalgamated their effects, until these are indistinguishable from innate hereditary attitudes.

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* Féré's work is extremely interesting. But surely these hysterics, who present as perhaps the fundamental symptom an extreme suggestibility, are exactly the persons who would respond most completely to the many subtle suggestions of language, art, and custom, which will shortly be mentioned (not to speak of possible unconscious suggestion from Féré himself). For the distinguishing of a true dynamogenesis, as distinct from the effect of such influences, no subjects could be less reliable.

† Prof. E. B. Holt has told the writer of a very similar study, made by Holt, with completely negative results. The average times, with the different hues, were almost perfectly identical.

So Müller-Freienfels[27] emphasizes the extent to which a color may be given an affective value simply by its name, as blood-red, or violet, or lilac, or orange, or lavender. The red-furnished room mentioned by Miss Calinich[6] is also of some interest. If the room was warm the reds seemed bright, lively, cheerful, warm and enlivening. But if the room was cold these same reds were dreary, depressing, with a peculiar, dead, chill effect.

The odd artistic career of yellow may also be instanced to show the extent to which even very special and artificial circumstances may have a profound effect upon the esteem with which a color is regarded. Yellow was a favorite color in classical times, and is now in the East. But there is, as has already been mentioned, some evidence to indicate that for the average European it is the color least liked. The change is (according to Havelock Ellis) due to the Church; the early church authorities looked upon the color with disfavor because of its association with pagan festivals and licentiousness, and succeeded in building up about the hue a group of symbolisms and associations of the most unpleasant character.[11]

Previous work bearing upon the writer's problem may, then, be briefly summarized as follows. (*a*) Introspective and observational studies show colors, as they appear in everyday experience, to have a marvelous richness and complexity of affective significance. There is the suggestion that color may be a conditioning factor of distinct importance in influencing the efficiency of mental work. (*b*) Laboratory experimentation for the most part fails to find any constant relationship between color and either introspective evaluation or organic reaction. (*c*) Analytical and critical studies emphasize the importance of artistic conventions, symbolism, language, and everyday association of certain colors to other sensations affectively toned, in giving an emotional connotation to the hues and brightnesses.

It would then seem reasonable to conclude that if color *does* have any fundamental physiological effect, such as would influence mental and motor efficiency, the connection must be of a very general and elementary nature; brightness may stimulate, or red irritate and distract, but more specific effects are hardly to be expected. It is some such relationship as this, between illumination and mental work, which is sought in the writer's experimentation.

III. Experimental Results

A. *Preliminary Experimental Definition of the Problem*

This experimentation began with a brief preliminary investigation, of an exploratory character, in which large sheets of colored paper were used. Nothing more need be said about the method than that the subject was seated comfortably at a table, with a large sheet of colored paper arranged in front of him so as to be almost the only object within his field of vision. While in this situation, thus dominated visually by the color, the subject was asked to introspect, and to do certain tests. Nothing more was done with the tests than to make trial of them. But the introspection yielded some points of interest. In the first place, the finish and texture of the paper appeared exceedingly important, especially so far as pleasantness and hold on the attention were concerned; the "energizing" or "stimulating" effect seemed more dependent on the hue. Where there is a shimmer, or where the light falls across a rough finish so as to emphasize the roughnesses, the result is peculiarly disagreeable. Any smudges on the paper were sure to be noticed, and were likely to play a part in the association; thus the dominant feeling of one subject for an orange was uneasiness centering in the kinaesthesis of a desire to reach out and smooth an unevenness in the paper. In the second place, the introspection suggested that the special affective values often assigned to such colors as lavender, purple, orange might be due to other causes than the hue itself. Such colors are unusual, and so attract and hold the attention; because they are unusual, what associations they have are definite and concrete, and give great vividness. With repetition, however, these factors usually drop out, and the color is ranked along with the primaries, as more or less bright, stimulating, depressing; is sometimes liked merely because it has in it more or less yellow or blue. Finally, individual differences, both in the evaluation of the different colors and in the general type of reaction to them, were marked. In fact all of the types mentioned by Bullough[5] and Bradford,[3] with variations, appeared at one time or another, from crude sensory effect to complex personalizing attitude. The reports made excellent evidence for the complexity of responses to color mentioned in the preceding section.

Thus one subject reported of a striking reddish purple that he "liked the color. But there was no special effect, aside from the pleasantness; otherwise it was neutral." And at the end of the color period it was "about as before. No great fatigue. The affective value was somewhat less, but fairly durable." Very different are the descriptions of another man, who says of a rough-finished yellow that it is "heavy, like an overcoat; queer feeling; not pleasant, not unpleasant. Impression that color is deep, and thick. Kinaesthetic sensation in finger tips as result of thickness. Heaviness seems referred largely to texture." A light yellow green is "very pleasant, a buoyant feeling." Another day this same subject finds the same color "depressing; the color of grass when it's dying."

More explicitly associative is a report on yellow. "A slight greenish effect; brought up idea of grapefruit tree at the Exposition and emotions going with that trip. Now wonders how got green; had seen stripes of green." An orange was liked; but then, with a thought of the war, came an idea of this as the color of bloody water, and a feeling of repugnance.

Different still in type of response is the characterization of a red as "too aggressive; it pushes itself upon you, won't let you alone, rude. I don't like it."

Enough has already been said to suggest the unsatisfactoriness of colored papers as stimulus areas. In the first place, no adequate control of brightness was possible; but such control was clearly essential if an adequate analysis of the problem was to be made. In the second place, the texture of the papers, and any wrinkles or spots, played too important a part in the subject's reaction to make adequate control possible. And in the third place it was not feasible to arrange the experimental setting, with colored papers, as was necessary for the problem. The problem was to determine the effect of color, as a dominant but unobtrusive and natural element in a situation, on mental and physiological processes. If paper were to be used little less than to have the walls covered with it would suffice. Under the circumstances, trial was naturally made of colored lights.

With regard to the preliminary work with colored lights, nothing more need be said than that it was of the same general character as the systematic experimentation to be described shortly. Work with the colored lights was begun in March (1915) and the remainder of that school year was spent simply in trial of various tests, light screens, and so on. The results to be described in the next section were, therefore, obtained with methods which had been carefully elaborated in this trial series.

The preliminary work served chiefly to develop methods and setting and to make trial of tests. The introspective material may be summarized as follows. (1) Affective reaction to colors is highly variable, both from individual to individual, and from week to week with the same individual. No hue, except perhaps red, may be said to show characteristics of any constancy. (2) The reaction is very easily modified, or often wholly changed, by (*a*) subjective factors such as general condition, mood, constellation of ideas, or chance association, and by (*b*) incidental objective factors such as unevennesses in the colored surface, and especially the texture of this surface and the way in which the light is reflected from it (softly, or with a shimmer, glint, or glare). (3)

The reaction may be of all degrees of complexity, from simple sensory effect to elaborate associative development or subtle personalizing attitude.

Introspective treatment of the subject will be returned to, briefly, later.

B. *General Experimental Setting and Method*

The experimental setting developed during the last part of the preliminary work was continued, essentially unchanged, throughout the remainder of the study. It may be described as follows. One end of a long narrow dark room was shut off by a heavy black curtain, making a smaller room about 9x6 ft. Backed up against the curtain was a table three by five. Behind this, that is, facing the curtain and with his back to the end wall of the room, sat the subject. The table was covered with heavy unglazed drawing paper. Over the center of the table, and 26 in. above its surface, was the light-box (8 by 10 by 12 in.), held by a frame-work from the ceiling. This contained four 100 c. p. Mazdas. In the base of the box were slots, where the colored plates could be slipped in. The light box was dead black outside, but lined with metal to reflect the light downward; on the side of the box next to the subject was a black pasteboard drop hanging just low enough so that none of the direct light came into his face. At one end of the table, and dropping down almost to its surface, was the cord, with its glove finger, for the tapping test. Well off to the other side was the small block of wood with the two keys, for the reaction test; this was hooded over with a piece of the white drawing paper, to make it as unobtrusive as possible. The glove finger and the reaction key block were the only pieces of apparatus visible to the subject.

As has been said, none of the direct light came into the subject's face. The light shone directly, however, on almost the whole surface of the table, but very little beyond this. The situation was thus not at all uncomfortable for the subject. He sat easily at the table; the light came from a position not uncommon in ordinary lighting arrangements. There was no shimmer, glint, or glare, from the unglazed paper, only a soft, diffused, indirect illumination. And aside from this there was only the hardly-to-be-seen blackness of walls and curtain. This was the situation when the test lights were burning; when the "normal" light (a 16 c. p. Mazda fastened to the light-box on the side away from the subject) was on, between tests, the situation was very little changed. The lighting was still indirect, came from the same position approximately, and was of roughly the same brightness as the colors used.

On the other side of the curtain, and against the wall, was the experimenter's table. On this were the various recording devices, the theatre dimmer for equating the brightnesses of the colored lights, the time keys, and so on. Here also was the double-throw switch, arranged to throw on the test lights at the same instant that the "normal" was thrown off (and the reverse), so that there should be no period of darkness, or irregular interval, between the two stimuli. The experimenter's table was lit by a four candle-power lamp in a hooded "goose-neck" holder; it was wholly invisible to the subject behind his heavy curtain.

From now on the study consisted of two separate pieces of experimentation, making use, however, of the same tests, the same methods and the same setting; the "hue series" was planned to discover any possible effects of hue, independent of brightness, and the "brightness series" to isolate any influence of brightness, upon mental work. The plates used in the hue series were complementary blue, pure green, red. The stimulus lights were equated for brightness (by episcotister) in terms of the darkest, blue; to do this a theatre dimmer was used to shift the illuminating power of the lamps according to the absorption of the different plates.* The illumination, on the top of the table under the lamps, was about seven candle-meters. The brightness series with which most work was done was also a three-unit series. The dim light was obtained by stepping down the current with the dimmer and putting in the white screen; it gave an illumination on the surface of the table of about one candle-meter. The medium light was given by a single 32 c. p. Mazda, placed in among the larger lamps in the box so as to give the same distribution of light, and thrown in by double-throw switch after the "normal" in the same way. The bright light finally settled upon was the full power of the four 100 c. p. Mazdas.

For one semester, however, a five-hue and a four-brightness series were run. The five-hue series consisted of the hues above mentioned *plus* yellow and white. For white a special plate, a double thickness of "pot-opal" glass, was made; at the above-mentioned brightness the light through this was found to be almost pure physiological white. The four-brightness series consisted of the same two extremes as were used in the three-brightness series, but with the white of the hue series, and a medium bright light made by the white plate with the full power of the four Mazdas behind it, in place of the 32 c. p. "medium."

Three possible criticisms of this choice of stimuli remain for brief discussion. In the first place, it may be said that an effort should have been made to obtain monochromatic colors for the hue series. To this it may be replied shortly that the problem of the study was essentially a problem in applied psychology, and that what was sought was the effect of colors such as might be used in actual schemes of illumination or decoration. The important requirement was that the lights should be seen by the subjects as red, green, and blue, of a not unusual composition or saturation, and that the saturations should be approximately equal. The lights were, as a matter of fact, almost invariably judged by the subject as simply blue, green, or red without admixture of other hues; and they were considered about equally saturated (the yellow was dropped partly because less saturated). Nothing more seemed essential. Monochromatic light, supposing it could have been obtained for use over such areas, would have been a highly artificial form of stimulus much less relevant to the practical problem.

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* The writer is indebted to Dr. L. T. Troland for the equating of the brightnesses, and for the rough analysis of the lights here given.

It may be objected, in the second place, that more hues (and more brightnesses) should have been used. The difficulty was mainly a matter of method, and of experimental practicability. Most of the subjects came to the experiment only once a week, for a 50 min. period; five or six colors meant only seven or eight minutes with each. The problem, however, was to determine, not the effect of the presentation of the color, but the influence of its continued presence as part of a work situation. Longer periods were thus clearly necessary. So it seemed best, for this and various other reasons, to limit the experimentation to intensive study of three hues, and three brightnesses. Practically all the previous work (not to mention the writer's own preliminary study) indicated the colors at the two ends of the spectrum as the hues most diverse, fundamentally, in their effects. It seemed reasonable to suppose, therefore, that an intensive study of red, of green, and of blue would indicate with fair adequacy the direction and amount of the influence, if any were to be found, and give the best evidence for negative conclusions in case the results were negative.

In the third place, criticism might be made of the "very bright" light of the brightness series, as too strong, possibly so bright as to trouble the eyes, and so introduce factors not relevant to the problem. The writer expected such difficulties. And he was much surprised to find, after extended trial with all the subjects, that in most instances this bright light was the most pleasant of the series! The subject received only indirect, diffused light; there was no glint, glare, or other unpleasant distribution. Under these circumstances this light was usually introspected upon as stimulating, cheering, very pleasant. And anything less bright failed to give the desired distinctness from the "medium" light.

As has already been intimated, most of the work was done with three hues, or three brightnesses, to the hour. This permitted 12 to 15 min. with each color, with intermissions between colors of about 4 min. The colors were given in a definite routine; if, for instance, subject "A" was given on the first day red first, green next and blue last, then for his next hour (the following week) he was given green, blue, red, and the third hour blue, red, green. After three hours, then, each color had appeared once in each position in the hour. This made up a unit, within which the various factors of the work curve (fatigue, practice, and so on) were evenly distributed among the colors. The next group of three would begin red, blue, green: in this way every possible order was tried. Between colors the " normal " light already mentioned illuminated the subject's room.

The number of subjects varied from one semester to another; usually there were six or eight, and two came twice a week, the rest once. Subjects who came twice a week were given one hour with the hue, and the other hour with the brightness series; the other subjects were divided up equally between the two series. Regular hours were assigned; the experimentation was done in the morning, in hour periods, between nine and one. The subjects were all trained observers, and familiar with laboratory methods and technique.

In the entire study (including the preliminary work) twenty-six persons at one time or another acted as subjects. One was a professor of psychology; seventeen were graduate students with experience in research. Eight were Harvard undergraduates doing advanced work in psychology; three of these were carrying on independent research. Three of the graduate students were women. It should, perhaps, be added that the study was made during the university years 1914-1915, 1915-1916, and 1916-1917 to the first of March.

Much difficulty was found in securing satisfactory tests, because of the fact that the purpose of the experiment prevented the employment of any test requiring use of the eyes; the purpose was not to study visual acuity with lights of different colors, or the effects of eye strain in dim light on mental work, but to discover whether *apart from such factors* different hues and different brightness had any influence on general neural tone and mental efficiency. The vast majority of tests previously used in such comparative studies (studies of the effects of drugs, for instance) were thus barred from the writer's experimentation. And much time was spent in simply developing tests which would meet these special requirements. The tests finally adopted will be described in detail later, along with the presentation of the results obtained. A large number of other tests (as dynamometer measurements, maximum speed in tapping, several forms of memory test) were tried, but found unsatisfactory and rejected.

The test routine varied, of couse, with the tests employed. The first of the work was done with the following tests, and in the following order. First, under the normal light, the subject tapped for 30 sec. (this was not the usual tapping test, but a tapping "at most comfortable rate." as will be described later). Then the normal light was thrown off and the test color on; the tapping, however, continued for another 45 sec. Introspection was now called for. After this, the multiplication was given, then the pressure test, then the memorizing. Forty-five sec. more of tapping followed this. Finally, introspection was asked as to any change in the influence of the color from that reported when the light first appeared. The normal light was now thrown in, and the subject rested for 4 min. while the experimenter changed the colored plates, adjusted the theatre dimmer, slipped new smoked papers on the kymograph drums and adjusted the recording apparatus. Then the same programme was repeated with the other two hues. Introspection was called for only during the first two or three weeks at the beginning and end of a half year's work; for the greater part of the time the work was wholly objective.

This general arrangement was adhered to throughout the experimentation. As sufficient data were accumulated with a test it was dropped, and a new test substituted; the routine, however, remained essentially the same. Work in the color series and in the brightness series was also throughout the same; there were the same conditions, the same tests, and the same routine; so that the two series were in all respects strictly comparable.

Every half year saw a change in programme of some sort. With a new test three hours of practice were allowed, as a rule, before results were used. Data from twelve hours following this were the aim. This was not always possible, however. By the methods used, the results with a given color, in a given hour, and on a given test, obtained their significance from comparison with the results, on that test, under *all* the other lights. If now, reaction times under green were lengthened by the sound of the bell at the end of the hour then all the results, with the other colors as well, were warped in their significance. So the rule was made that no results from any hour which was interrupted, in any way, on any test, should be included. A large amount of material was, of course, eliminated by this rule; but the eliminations seemed necessary, if the subtle effects sought were not to be buried completely under such chance variations. The endeavor was also made to obtain, in each series, results from at least six individuals, on each test. This was, again, not always possible (or worth while) but was done in the majority of cases.

A word remains to be said regarding the handling and interpretation of the data. In the investigation of any such subtle influences as were being sought chance variations in the results are certain to be large, and differences (if any) due to the factors studied, small. Satisfactory interpretation and evaluation of the data are thus extremely difficult. The situation is, essentially, too complex to make possible any summary of the consistency of the findings, which could be treated statistically. Instead, a judgment upon the reliability of the data has been attempted, based upon three related, but distinct, sets of facts: (1) the consistency of the results obtained from each subject, on each single test; (2) the degree of agreement among different subjects on each test; and (3) the consistency of the results from test to test with each subject (a correlative consideration here being, of course, the consistency of the group from test to test). Any final numerical statement of the reliability of the findings must, of necessity, be partial and inadequate. Instead, an attempt has been made to bring all the facts, considered from these three points of view, together in a judgment as to the probable meaning of the data.

The writer is not at all sure, now (1919), that a more determinate and decisive statistical treatment of the material would not have been possible. But, in defence of the procedure adopted, it may be pointed out that practically all the well-known studies having similar statistical problems have been quite as indecisive in their statistical statement. Most similar, in the statistical problem presented, are studies of the effects of drugs, of which the monographs of Hollingworth[17] and Dodge[9] may be cited. And it might be pointed out in this connection that, so far as difficulties of control and analysis are concerned, study of the effect of only three colors, in one short hour, is not a little analogous to an attempt to study the effect of three drugs in that brief space of time. The problem was not an easy one, and abounded in unusual difficulties of analysis and statistical treatment.

The methods used will be discussed in more detail in considering the results obtained with each test.

C. *Results*

1. *The Effect of Color upon Rate of Rhythmic Movement of the Finger.* This test was planned to give an indication of any change in neuro-muscular tone and tendency to movement. It was, essentially, an ergograph with minimal resistance. Clamped to the end of the table was a rod which reached obliquely in over the table top. The end was about 15 in. above the table, and the same distance in from the table end. To the rod was fastened a pulley. Through this ran a silk cord, to which was sewed a glove finger. The subject put his left fore-finger in the glove, and rested his arm easily on a support which kept his finger free above the surface of the table. The test consisted simply in moving the finger up and down with a tapping movement. The white silk cord ran over to the experimenters' table, where it was fastened to a marker which wrote on a smoked drum, and to the resistance, a single piece of ordinary rubber band. The subject was told to tap at an easy, comfortable rate, which could be kept up without fatigue during the minute tapping period; to find the rate which was most natural and comfortable for him; then to forget all about the test, letting the movement go on automatically. There was a specific caution against any effort at speed.

The test should, the writer felt, be quite highly sensitive to any energizing or stimulating effect the colors might have. If red *did* excite, or a dim light subdue, this ought to show easily in such a half-conscious rhythmic activity as this simple movement, up and down, of the finger. But there were other advantages. Practice effects were unimportant; there was no learning; there should be nothing of those wide variations likely to appear when there is any requirement of maximal effort.

The place of the test in the routine has already been described. The tapping was begun 30 sec. before the stimulus light was turned on, and was continued for 45 sec. after this. The other tests were then given. And finally, just before the stimulus light was turned off, there was tapping again for another 45 sec. Time was marked off in 15 sec. units. The results were handled in terms of these units. The number of taps in the first two quarter-minutes under the normal light were averaged to give the average normal for the period. The number of taps in each one of the six quarter-minutes under the stimulus light were then scored so much *plus* or *minus* from this normal for the period. And the difference between the variation from its normal produced by red, and the variation from *its* normal produced by the blue, gave the difference in effect of these two colors.

This final figure, it should be emphasized, was almost wholly free from the influence of constant errors. With some of the tests, systematic variation through the hour, due chiefly to difficulty in getting started with the work, and to fatigue in the last period, were considerable. However, with this arrangement of the tapping, the normal, for each period, just preceded the color period itself. If red came last in the hour on a given day, its normal came near the last too; the normal took account of the variations through the hour (as well as of the daily variations), and none of these irrelevant factors got itself into the results.

In handling the results the number of taps, in each 15 sec. under the color, was first scored as so much *plus* or *minus* from the average number of taps per 15 sec. under the normal light, as has been just described. The variations for the first three quarter-minutes under the color were then added, giving the total variation for the first 45 sec.; and the variations for the last three quarter-minutes also added. For instance, suppose the number of taps per 15 sec. ran as follows:

	Green		
normal	first 45 sec.		last 45 sec.
17 15	16 17 19	(other tests)	17 19 20

This would be scored

16	0 1 3 (4)		1 3 4 (8)

To obtain the final percentage statement, the average normal was multiplied by three, to give the average normal for 45 sec. and the succeeding variations per 45 sec. stated as per cents of this. The above record would thus become

		Green	
	normal	first 45 sec.	second 45 sec.
	48	4	8
and in per cents	48	8%	17%

This could, of course, have been done less elaborately by running a normal of 45 sec. instead of 30 sec. But since the work was begun with a shorter normal it was so continued, and the above method of handling used.

TABLE I

Tapping Rates: in Terms of % Variation from "Normal"

Four Brightness Series

Sub.	Av. Nor. 45"	I				II				III				IV			
		1st 45",		Last		1st 45",		Last		1st 45",		Last		1st 45",		Last	
		av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And......	46	5	6	5	11	— 2	4	0	10	— 3	6	—1	11	2	10	— 6	10
Dav......	55	—2	3	13	5	1	3	3	7	0	2	2	11	2	4	1	9
Fin.......	49	—8	6	—6	9	—12	11	—1	10	— 5	3	—2	4	—12	6	—11	10
Kal.......	102	7	6	12	14	5	7	10	13	— 2	7	3	20	—11	9	— 5	20
Average	...	.5	5	6	10	— 2	6	3	10	—2.5	4	.5	11	— 5	7	— 5	12

(Ten hours each subject.)

Three Brightness Series

Sub.	Av. Nor.	I				II				III			
		1st 45"		Last		1st 45"		Last		1st 45",		Last	
		av.	m.v.	av.	m.v.	av.	mv.	av.	m.v.	av.	m.v.	av.	m.v.
And................................	62	16	7	18	12	2	5	13	9	0	8	5	9
Cur................................	68	4	5	9	6	6	3	2	8	2	3	11	12
Dav................................	77	7	8	9	4	0	2	6	10	7	10	4	7
Woolb.............................	51	1	5	0	7	3	3	—4	8	0	3	— 7	10
Mor................................	124	6	7	12	8	11	10	16	11	7	5	18	13
Average............................	...	7	6	10	7	4	5	7	9	3	6	6	10

(For each subject in order the hours were 20, 10, 8, 9, 9.)

TABTE I—*Continued*

Five Hue Series

Subj.	Av. N.	Red				Yellow				Green				Blue				White			
		av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
Br.	93	2	4	8	3	2	3	4	2	—2	3	0	6	2	2	0	3	—2	2	5	5
Ch.	84	—3	6	6	9	0	6	5	2	—2	3	2	5	1	6	2	7	0	2	10	4
Da.	57	0	5	9	8	0	5	1	7	2	2	12	6	0	4	1	8	—1	3	0	7
Ohl.	73	3	4	6	4	3	7	0	6	2	4	3	4	0	5	3	4	3	5	1	7
La.	109	—2	4	7	7	2	3	10	5	0	2	10	6	1	4	4	4	—1	3	4	6
Average	...	0	5	7	6	1	5	4	4	0	3	5	5	1	4	2	5	0	3	4	6

(Ten hours each subject.)

Three Hue Series

Subj.	Av. N.	Red				Green				Blue			
		av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
Allp.	147	6	4	4	7	6	3	6	7	3	6	2	6
La.	107	2	4	6	6	1	3	4	6	1	2	9	9
Bul.	44	3	4	9	8	2	5	1	12	3	4	6	8
Br.	148	3	2	5	3	—2	2	—1	3	—1	3	3	3
Ch.	106	3	4	2	14	0	3	12	8	0	3	4	12
Wo.	49	—2	2	—6	11	1	3	1	9	0	4	1	7
Sp.	60	10	.	1	14	1.5	12	6	10	0	4	9	6
Average	...	4	3	3	9	1	4	4	8	1	4	5	7

(Twelve hours three subjects, nine hours four subjects.)

The m. v. after the group average is the average of the m. v.'s, not the m. v. of the averages. In handling the per cents, fractions of one per cent were read as the nearest whole number.

The results for the brightness series are given in the following table (Table I). They are stated, as has just been described, as per cent variation, from average under normal light for 45 sec., for 45 sec. under the color. The first series was run with four lights, bright, moderately bright, medium and dim. In this series were four subjects; the number of hours for each was ten. In the second series three brightnesses were used; the first and the last were the same as in the first series, but in place of the moderately bright and medium light a new medium, the 32 c. p. Mazda, was substituted. In this series were five subjects, two of whom had also been in the previous series. The number of hours with one subject was twenty, with one ten, with one eight, and with two nine.

The results may be summarized as follows. Of seven subjects five, including those from whom there are most results, show an increase in rate of tapping with bright light, and a decrease in dim light. One subject showed a slight, the other a more considerable, reversal of these results. In the case of this last subject, there is some evidence to suggest a special, somewhat morbid, peculiarity in this respect. For those subjects giving positive findings there was not only an increase under bright light, but a decrease under the dim light as compared with rate under the medium light. There was also no evidence of decrease of these positive findings through a semester; one subject, kept at the test for a year and a half with a view to obtaining evidence on this question, gave as positive results at the last as at the beginning.

It must be emphasized that the results from the two tapping periods, the first 45 sec. and the last 45 sec., have a very different significance as regards the possible effect of the lights. Any change in the first period, when the light had just been thrown on, might be due simply to a shift of attention, or some similar transitory element in the situation. The results from the last 45 sec. should be of much more importance, since the stimulus light has been on, by that time, some 12 min., and any effect then observed must be of some permanence.

As a matter of fact, the results are most distinctive in this last period. The group averages show greatest number of taps for the brightest light, and a regular and even decrease down to the least number for the dimmest light. When the groups are analyzed it appears, in the first place, that the two subjects who worked in both series agree substantially, in the indications they give under the two somewhat different conditions. In the first group of four (with four lights) all the subjects agree in showing most rapid tapping with the bright light, slowest with the dim. In the second group of five, three subjects give this same result. One, however, shows a slight, the other a fairly distinct increase with the dim light over the bright one. In this last case, introspection may throw some light on the situation. This subject (Mor.) was extremely fond of dim lighting. This was due, he said, to a general liking for subdued light effects. He was fond of sitting in his room in the dark, or with the lights low, was very fond of candle-light, could think best when the lighting was dim. With the other subject giving opposed results there is no such clear-cut preference, though he did say once or twice that he could think better in the dark.

The results with the hues may be very briefly summarized. No differential effect of different hues, at equal brightnesses, upon rate of tapping appears. Two series were run, one consisting of red, yellow, green, blue, white, the other of red, green, blue. Eight subjects took part, five in the first series, four of these and three more in the second. It may be said, shortly, that no consistent findings appear with regard to any one of the hues studied. (See Table I.)

2. *The Effect of Color upon* (*a*) *Pulse and Respiration,* (*b*) *Estimates of Pressure, and* (*c*) *Judgments of the Pleasantness of Touch Substances.* It may be said at once that the work with pulse and respiration gave results which were entirely negative. The work was very carefully done; records were taken both at the beginning and the end of the color period, and studied with reference to both rate and shape of curve. But nothing appeared which could be correlated, in any way, with either brightness or hue.

The experiment of Stefanescu-Goanga,[35] already mentioned, was also largely repeated, with the writer's setting, but with negative results.

These negative findings are not, of course, proof positive of lack of any effect of hue or brightness upon the physiological processes. There may be an effect, and of a cumulative importance, on respiration and pulse. But the changes under such conditions must be, within the time any convenient measurement might extend (30, 45, or even 60 sec.) so minute as to be probably below the threshold of even the best technique.

It may also be said, with equal brevity, that results with the "pressure" and "pleasantness" tests were similarly negative. No influence of either brightness or hue, upon performance with these two tests, could be discovered.

The apparatus for the "pressure" test was simply an ordinary postage scale. For 1 min., during the 5 min. intermission between colors, the subject practiced pressing down exactly 16 oz., while looking at the dial. The scale was then taken away, but presented again, when the time during the color period came for the test; the subject was now given five trials at pressing down 16 oz. as previously practiced, but this time with the dial turned away from him. The record taken was the amount actually pressed at each attempt (the amounts pressed in the five trials being summed to make the "pressure" score for each color).

Just what function the test might be supposed to sample was not clear. Perhaps a bright light, or a red, if stimulating, would bring about a more energetic push on the scale, and an overestimate. And a dim light might bring about less activity. But the fact was that no such relationships appeared. The test was interesting, but of no value, at least for the writer's experiment.

The experimentation with the touch substances was almost as simple and direct. A large variety of touch substances (different kinds of cloth, wood, metal, and so on) were fastened to small blocks of wood (one and one-fourth by two in. and one-half in. thick). Twenty of these blocks were put in a shallow box just big enough to hold them, five in a row with four rows. This box was covered with a hood of the heavy, unglazed drawing paper. When the time came for this test, the box was put on the table in front of the subject who, with his right hand, beginning at the upper left-hand corner, worked across the top row to the right. Each substance was felt in turn, and its pleasantness expressed on a scale of seven, where one was most unpleasant, four indifferent, and seven most pleasant. After the top row was finished, the subject dropped his hand to the next row, working from right to left; and so on till the twenty substances were all judged. The hood hid the substances from view, so that the subject had only a touch acquaintance with the materials.

After the test was finished the box was taken from the table, and the other tests given. During the intermission between color periods the experimenter, by a simple arrangement, completely and systematically changed the order of the substances. And from week to week the substances used were changed. The purpose in each case was to break up any memory of previous judgments on a particular substance, to make each estimate a product simply of the two present factors, the touch substance as it felt now, and the present affective attitude caused by the color. The results were handled by simply adding the values assigned to the twenty substances under each color, giving a total which might, perhaps, be called the affective level under the given light. The test seemed to the writer to have possibilities. But the findings were altogether negative. He still feels, though, that something of this sort might yield interesting data in a study of some other problem, to which it was better adapted.

3. *The Effect of Color upon Rate of Multiplying.* No study of this type would be complete without some sort of arithmetical test. Such tests seemed almost impossible of control sufficient for the purposes of the present problem. Little help could be obtained from previous work, since practically all tests previously used were ruled out, from the start, by the requirement that there should be no visual presentation. Nevertheless, the writer felt that an adequate study of his problem required at least an attempt at measurement of possible effects of color upon some such relatively complex form of mental work.

The test, as finally made up, consisted of multiplications of one-place by two-place numbers. Eleven such multiplications were given with each color; a test sheet, for a given day, thus consisted of three rows each having eleven problems. Three such sheets were prepared, enough for three weeks (or a single series, where the colors came each once first in the hour). The three were then repeated in order. But the order of the colors was varied, so that any difference in difficulty of the different groups of eleven was distributed. The record was in time for each multiplication, as taken with a stop-watch, and in errors. In handling the material, that multiplication of the eleven which had the longest time was struck out; quite frequently a time would run much beyond the average as a result of some slight distraction; an elimination of the longest time of each eleven allowed, to some extent, for these variations. The times for the remaining ten were summed.

TABLE II

TIME IN MULTIPLYING (BRIGHTNESS SERIES) IN TERMS OF % OF AVERAGE TIME FOR HOURS

Four Brightness Series

	I. (bright)		II.		III.		IV. (dim)	
Subject	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And	89	13.0	101	13.0	106	13.0	103	8.0
Dav	96	5.6	106	9.5	101	7.8	97	9.8
Fin	97	3.5	99	7.5	104	9.0	99	8.0
Kal	99	9.0	94	11.0	97	5.0	109	6.0
Average	95	7.7	100	10.2	102	8.7	102	7.9

(Ten hours each subject.)

Three Brightness Series

	I.		II.		III.	
And	100	6.2	99	4.7	101	5.6
Cur	95	7.6	101	10.0	104	7.6
Dav	99	4.9	99	4.8	102	5.3
Woolb	97	7.0	102	3.9	100	6.3
Mor	100	9.5	100	9.3	100	10.1
Average	98	7.0	100	6.5	101	6.9

(For each subject in order the number of hours is 10, 10, 8, 9, 9.)

TIME IN MULTIPLYING (HUE SERIES). IN TERMS OF % OF AVERAGE TIME FOR HOUR

	Red		Green		Blue	
Subject	av.	m.v.	av.	m.v.	av.	m.v.
Allp	99	4.7	101	6.2	99	8.2
La	101	4.4	102	4.5	97	2.7
Bul	100	8.3	99	7.9	100	8.8
Br	103	6.4	98	5.6	99	3.6
Cha	100	6.5	98	5.8	101	10.2
Woolb	98	7.5	104	7.8	97	12.0
Tul	103	4.4	102	6.1	95	6.0
Average	101	6.0	101	6.2	98	7.3

(Twelve hours three subjects, nine hours four subjects. m.v. after the group average is, for each table, the average of the m.v.'s, not the m.v. of the averages.)

The sets of eleven were made up to be as equivalent as possible. Any three numbers, as 6, 7, 9, can obviously be combined to form six problems of the type used: 6 times 79, 6 times 97, 7 times 69, 7 times 96, and so on. Five and a half such groups of six would, therefore, make up the materials for a day's test sheet: eleven problems with each of three colors. The problems were distributed among the three colors to give as great variety as possible, and as great uniformity: if 7 was used twice with a color as multiplier those two multiplications did not come together, and there were two sevens as multipliers with each of the other colors. In giving the test, the experimenter simply read each problem to the subject, slowly and distinctly, as " six times eighty-seven." The subject did the work mentally, and then replied with the answer, " five twenty-two." And the experimenter took time from the last syllable he said in giving the problem to the last syllable of the subject's reply.

In combining the results for a half year or more, two important factors had to be taken into account: (1) variations within the hour, and (2) variations from one experimental hour to another through the year. In dealing with practice through the year, some percentage statement of ranking of the colors, within the hour, was naturally suggested. The method finally adopted employed, as norm for the hour, the average of work under all three colors. The time under each color was then rated as a per cent of this.

For instance, suppose on a given day subject A's time for the ten multiplications, under each one of the three colors, ran as follows:

red	green	blue
55.0	57.2	59.2

these three would be averaged, giving 57.1., and per cents taken from this making a score of

red	green	blue
96%	100%	104%

Averages of these per cents through the year gave results free from practice effects from hour to hour. They were free from the many other variations from week to week, due to changes in condition, previous fatigue, and so on. Finally, the results from individual to individual were directly comparable; the average actual time for some individuals was over twice that for others.

The results are summarized in Table II. Mor., who gave results contrary to the tendency of the group in the tapping, does the same here. Otherwise there is a fairly consistent slowing with dim light, and acceleration of work with the bright.

The hue series gave no results so consistent. There is a suggestion of most rapid work with blue and slowest with green. But inspection of the individual averages reveals no consistent tendency such as appears with the brightnesses; minimum rate occurs three times in green, twice in red, once under blue, red and blue are once " tied for first place."

4. *The Effect of Color upon (a) Rate of Free Association, and (b) Immediate Memory for Nonsense Syllables.* The arithmetical work called for associational processes of a rigidly controlled and limited sort. As a contrast to this, it seemed interesting to try what the effect of color might be upon wholly uncontrolled, free association. Some test of memory also seemed necessary for a rounded test programme. Memory for nonsense syllables and rate in free association, under the various hues and brightnesses, were therefore investigated. And, it may be said at once, the results were entirely negative.

The materials and method for the free association test were substantially the same as used by Professor Langfeld in his study of the effects of prolonged fasting.[19] His lists of words, twenty to a list, were used, twenty words with each color. Timing was with a stopwatch. The experimenter simply read each word, slowly and distinctly, to the subject, who was instructed to reply at once with the first word that came into his mind. Time was from the last syllable of the stimulus word to the last syllable of the response. In handling the results, the longest and the shortest times were struck out, as most likely to contain extreme chance variations, the longest as perhaps due to a complex, the shortest as due, perhaps, to a mere perseveration from some previous word or associate. The total for the remaining eighteen words was then found.

The memory test was quite as simple. Immediate memory was clearly the only type which could be controlled adequately for the purposes of the experiment. Various kinds of material were tried; nonsense syllables were found to be the best suited to the test programme. Ten syllables were used with each color; a sample list runs as follows:

zok dib niv bam zek miz niz dop lor tem

In making the lists no long vowels were used (since these were found to "cling" in the memory more), and various other more usual precautions were taken, to make the lists of even and equivalent difficulty.

In giving the syllables they were read at a rate of two per sec., with a grouping in pairs by inflection of the voice. After the first reading the subject was asked to "give back" as many as he could. The list was then read to him again, and he was again asked to repeat all he could remember. This was done twice more, making four readings in all with each list. The syllables were scored as correct or half correct, in the recall; the final record was the sum of the recalls after each of the four readings. For instance, under red, the number of syllables given back after the first reading might be four, after the second five and a half, after the third seven, after the last eight. The record under the red would then be 24.5. The test was a great bore to the subjects, and yielded results of no value.

5. *The Effect of Color upon Rate of Continuous Choice Reaction.* A measurement frequently obtained, in work of this general nature, is speed in choice reaction. The writer felt strongly, however, that the classical reaction-time experi-

ment was neither suited for inclusion in his test programme nor calculated to get at the effects which were being sought.

It was at first planned to use the standard method, two stimuli and two keys, and the time in sigma for each separate reaction. But the technique was considerable; much time would be required to accumulate a small amount of data; and the measurements would be so minute that the appearance in them of so subtle an effect as the colors might be expected to have would hardly be likely. In fact, such investigation, with negative results, had already been reported.[23]

Besides, the writer had a feeling that the effect of a bright light, for instance, might consist not so much in an actual stimulation as in a "sustaining" of any activity which might be going on. Such an influence should show best on some form of rather monotonous work which could go on for an appreciable length of time in routine fashion; something comparable to work in a factory, where there are a few well understood stimuli and half-automatic responses, occurring over and over. Rivers[31] and Hollingworth[17] both used typewriting. Typewriting could obviously not be used in the present study; but something of this general nature, a continuous series of reactions to simple stimuli, was the type of task desired.

It finally occurred to the writer that instead of single reactions, and a time measurement of each, a continuous series, with timing only of the whole series, could be arranged; such a test would give exactly the sustained routine activity desired, and would also accommodate itself much more easily to the total test programme. The test, as finally worked out, may be briefly described. For signals two telegraph sounders were used; one, giving a sharp, loud click, to the right of the subject's table; the other, sounding duller and less loud, to his left. On the table, mounted on a small block of wood about 5 by 9 in. in size, were two keys, close enough together to be operated by the first and second fingers of the subject's right hand. The subject was simply to press the right-hand key when the right-hand signal sounded, and to respond with the other key to the left-hand signal. And the apparatus was so arranged that correct response on the part of the subject to a given signal automatically brought about the presentation of the next signal. But if the subject responded incorrectly, pressed the wrong key, no new signal appeared, and the subject had to rectify his mistake before he could proceed.

The test thus consisted of a continuous series of choice reactions, the subject setting his own rate and the apparatus exactly keeping pace with him, presenting new signals as fast as the old were reacted to. The test was run 4 min. with each color, 2 min. at the beginning of a color period and 2 min. at the end (taking the place of the tapping test, after work with the tapping test was completed, in the routine).

Highest possible speed on the part of the subject was not desired; he was told rather to work along steadily, much as he would in running a typewriter, and to emphasize accuracy rather than speed. Time was marked in 30 sec. intervals; both time and correct reactions were recorded by electric markers on a smoked drum. Only two choices were used, in order that learning might be easy and practice effects not great, and mistakes not over numerous.

The important, and original, feature of the test was, of course, the apparatus by which reactions and stimuli were thus interrelated. This interrelation was brought about by means of a special adjustment between contacts with each signal and with each key, and contacts on a revolving wheel. The wheel consisted of two brass cog wheels soldered together; bearing on the right hand wheel were the contact connecting with the right-hand signal and the contact connecting with the key to be pressed when the right-hand signal sounded, and bearing on the left-hand wheel were the contact to the left-hand signal and the contact to the key to be pressed when the left-hand signal sounded. The key contacts were side by side (that is, bearing on cogs which were side by side on the wheel); and underneath, making on the next cogs below, were the two signal contacts, also side by side, that is, making on cogs which were side by side. Certain cogs on each wheel were filed out, however, and these were so chosen that one, and only

TABLE III

Number of Correct Choice Reactions in Two Minutes. In Terms of % of Average Number, Per Two Minutes, for the Hour

Brightness Series

	I. (bright)				II.				III. (dim)			
Sub.	First 2′		Second 2′		First 2′		Second 2′		First 2′		Second 2′	
	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And......	100	2.0	102	6.9	100	8.5	104	8.9	95	7.4	98	7.6
Cur.......	98	5.5	102	5.0	107	5.2	105	2.8	91	8.3	98	7.1
Dav......	103	4.1	99	3.3	100	4.8	99	4.5	101	4.6	96	6.6
Pep.......	101	2.0	101	4.0	101	1.0	100	4.0	99	2.0	98	4.0
Mor......	99	4.2	99	5.9	98	5.2	102	3.9	99	6.2	102	3.9
Woo......	97	3.4	101	2.5	100	5.9	100	4.0	98	2.9	103	4.3
Av.......	99	3.5	101	4.6	101	5.1	102	4.7	97	5.2	99	5.6

(Twelve hours each subject.)

Hue Series

	Red				Green				Blue			
Sub.	First 2′		Second 2′		First 2′		Second 2′		First 2′		Second 2′	
	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
Allp......	100	2.9	102	1.7	100	4.6	103	2.0	96	4.7	99	2.1
Br........	101	2.5	98	1.6	99	2.5	100	2.1	101	3.1	99	3.1
Bul.......	100	4.5	101	2.5	101	3.1	103	3.3	104	5.3	91	6.1
Chac......	102	3.1	99	2.1	98	3.2	100	3.3	99	2.9	101	1.6
Tul.......	102	3.0	100	3.0	102	2.0	99	3.0	102	2.0	96	3.0
Woo......	95	4.2	103	2.0	95	6.0	106	3.9	100	3.9	101	4.4
Av.......	100	3.4	100	2.1	99	3.6	102	2.9	100	3.6	98	3.4

(Twelve hours each subject.)
The m. v. in the average row is the average m. v., not the m.v. of the averages.
In handling per cents values up to .5% were dropped.

one, cog was presented at any given point. The result was that one, and only one, signal contact, and one and only one key contact, were making with the entire wheel at any given time.

Suppose, then, the subject responds (correctly) to a previous signal by pressing the proper key, thus closing the circuit, and suppose the right-hand signal contact is making with a cog on the wheel. The right-hand signal will be sounded. At the same time the current will go through a magnet, jerking forward a rachet to engage a succeeding cog on the wheel, so that when the subject breaks the circuit, by releasing the key, a spring action against the magnet will jerk the wheel around one cog's distance. The result will be that the cog which was making with the right-hand signal contact will now be making with the right-hand key contact. And only by pressing the right-hand key can the subject again close the circuit, and proceed further with the reaction series.

The essential feature of the device thus consists of this arrangement by which a given contact on the wheel makes successively with a signal and a key contact, so bringing it about that the reaction called for is the only one which can be completed.* The apparatus, which was mounted on a base about 4 by 12 in. (it could have been built half this size) and stood about 3 in. high, was in a sound-proof box under the experimenter's table. The cogs made in irregular order, so that the subject was given signals sometimes alternately right and left, again two or three in succession left or right, and in various combinations. After some five months of practice one subject began to learn the combinations; the connections were then simply reversed, and so all the combinations changed. Aside from this, there were no evidences of marked practice effects.

The results were first handled simply as total number of correct reactions in the first and the last 2 min.; this was taken as a measure of what might be called " reaction efficiency " under the given conditions. However, since individual differences, and variations from day to day, were marked these figures were converted into per cents of the average for the day, as were the times in the multiplication test.

It should be mentioned that 1 min. practice was given before the hour began; there was nevertheless evidence of practice effects through the hour, especially with certain subjects. And since no convenient way was found for eliminating such constant errors from the results they may be supposed to make the mean variation somewhat larger than it should be.

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* The apparatus can be made to handle three, four or more choices, simply by using three, four or more cog wheels with their pairs of contacts. It can be used with any type of stimulus which can be electrically operated, and with any type of reaction. The arrangement would, therefore, seem of some general usefulness. The mechanism used by the writer was experimented with up to speeds about twice those of the average subject, and found perfectly accurate at such rates.

As has been pointed out to the writer by Prof. W. F. Dearborn, the test is somewhat similar to Seashore's " Psychergograph;"[32] the apparatus above described is, however, much more practicable. A mechanism very similar to the writer's has recently been described by McComas.[22]

In the brightness series the group average shows "reaction efficiency" to be greatest under the medium light, least under the dim. As before, Mor. is an exception, and this time also Wool. Mean variations are also slightly greater under the dim light. Differences between first and last 2 min. periods are hardly consistent or definite enough to allow any significance to be attached to them; the two periods are probably best considered together. In the hue series the group average is least with blue; red and green show no important difference. It can hardly be said that in either series the results are of any significance, except in so far as certain subjects in the brightness series give findings in agreement with their previous records on the tapping and arithmetic tests. But this consistency would seem of some possible importance.

6. *The Effects of Color, as given in the Introspective Reports.* As has already been said, for the major part of the experimentation no introspection was called for. Introspection was obtained, however, for the first and last hours of each term, in order to study (1) group tendencies and the amount of individual variations in report, and (2) the effects of habituation to the colors. The tables are too complicated to present; the findings may, however, be briefly summarized.

In this work introspection in terms of the Wundtian tridimensional theory was called for.

This was done only after considerable hesitation. But more of a description than a mere indication as to pleasure value was wanted. And if each subject was left to form his own categories, tabular statement and cross-comparison would have been difficult. The subjects were therefore asked, for a time, to report on the colors as pleasant or unpleasant, exciting or depressing, tensing or relaxing. Two degrees of each quality were recognized; a color could be pleasant or it could be very pleasant, or it could be indifferent, neither pleasant nor unpleasant. The work was done with the four-color brightness and the five-color hue series, and was continued four weeks with the first and five with the second (making up on each case one "unit" where each color occurs once in each position through the hour).

The group totals show, for the brightness series, the two bright lights as most pleasant, the dim least. The bright lights are most exacting, tensing; the dim is most depressing, relaxing; indifference centers with the medium light. All of this was to be expected, except perhaps the pleasantness of the very bright light, which was not found unpleasant once in spite of its brightness. In the hue series, green was most pleasant, white next, blue most unpleasant. Red is most exciting and tensing, blue and white are most often called depressing and relaxing. Judgments of indifference are rarest under red, showing that this hue is usually taken positively, one way or the other.

The variability of a given individual, from one hour to another, was considerable. For instance, And. reports the medium-bright light twice as very pleasant, and twice as wholly indifferent; Cha. finds blue three times exciting, once very exciting, once depressing; Bul. finds white tensing once, relaxing twice, indifferent twice; Lang. finds blue pleasant twice, unpleasant once, indifferent twice.

Individual averages also differ markedly. Bul. finds all the hues and all the brightnesses uniformly pleasant, with green and the brightest light each called very pleasant once. The dim light is for Fin. and Dav. uniformly unpleasant. Cha. finds red very pleasant, Ohl. very unpleasant. Red is for Bul. relaxing, for Dav. very tensing. The results of continued experience with the colors were quite what would be expected. There was a thinning of the feeling, a dropping-away of associative enrichment and toning, a matter-of-course attitude which paid very little attention to the colors one way or another.

Thus one individual, in the first weeks of his experience as subject, reported the bright light as "exhilarating, hard on the eyes at first, but less hard than the dim light," and later in the same period, "pleasant, more awake." The dim light was disagreeable, unpleasant, sleepy, tensing, bothering to the eyes. After three terms with the experiment he speaks of the bright light as "just an ordinary light, a bit pleasant when first turned on, doesn't mind it at all." And at the end of the period he says that there "is nothing special to report, slightly pleasant, that's all." Another subject reports in the first weeks that the bright light is "exciting, livening, like sunlight," the dim is "easy on the eyes, relaxing; doesn't feel like working." At the end of the year introspection on the bright light is simply that "the dirt spots on the paper show up more;" the dim light "tends to put to sleep, a little depressing."

Similarly, in the hue series, at the beginning of the year, a subject reports red "very pleasant, because of its richness," and again, "very pleasant, it is so rich." Blue is "pleasant, reminds one of the sky and clouds," again is "ghastly, unnatural, unpleasant, exciting" (this the following week!). At the end of the year blue is "neutral" and again "neutral in all ways." Red is "perhaps slightly pleasant" and again "slightly pleasant: it came as something of a surprise." Another subject who at first "didn't like the red a bit" and again found it "decidedly unpleasant, tensing, exciting," toward the end of the year called it merely "not quite so pleasant as the others," and "on the pleasant side." Blue was at first "very unpleasant, disagreeable, tiresome;" by June it had become "quite pleasant, rather restful," and again "quite pleasant, rather restful."

It may be said shortly that there is, if the introspective reports may be trusted, a definite decrease in affective reaction to both hue and brightness, with habituation.

The results obtained in the systematic experimentation may, then, be briefly summarized.

(1) Results with a tapping test, a multiplication test, and a continuous reaction test, suggest a decrease in function under dim, and an increase under bright light. With the other tests no effect of brightness was observable.

(2) The objective measurements showed no effect of hue, independent of brightness, upon the functions tested.

(3) Introspective estimates indicated marked variability, and marked individual differences, in the feeling-attitude toward brightness, and especially toward hue. A marked decrease in affective reaction to both hue and brightness, with habituation, is the only feature common to all the introspective reports.

IV. Summary and Conclusions

So much, then, for the writer's experimentation. It remains to evaluate these findings in the light of previous work, and to summarize what seem to be the most probable conclusions with regard to the problem.

First to be considered (1) are possible differences among the hues, in their effects upon mental and physical activity. As has already been intimated, the writer feels that there is no adequate evidence in previous work to show any such differences. He has found no such differences in the present experimentation. The common notion that certain hues have a marked influence upon emotional tone and mental work is the result, he believes, of such subtle and pervasive influences, arising from figures of speech, custom, artistic and social convention, and everyday association of certain hues to other sensations of a strong affective toning, as have already been mentioned in the historical review. The misconception may also be due in part to a failure to differentiate the influence of brightness from the influence of hue.

If there is an effect of illumination upon mental work, it would appear, then, (2) that brightness must be the important factor. Certain of the experimental results obtained by the writer have suggested a slowing of mental work under dim light and a stimulation under bright light. Such an effect is at least not incompatible with what little previous work has been done in this field; and the hypothesis of a stimulating or dynamogenic influence of brightness upon mental work and neuro-muscular tone seems by no means unreasonable. It need hardly be added that, if there *is* such an influence, it is a matter of great general importance, from a practical point of view.

It must, however, be emphasized at once that the writer's positive findings are few and far between, and that their reliability is by no means all that could be desired. The writer has argued that in such a study the interrelation of all the results should be taken into consideration, in judging of the significance of the findings. And he has based his conclusions on (1) the consistency of the results obtained from each subject on each single test, (2) the appreciable agreement among different subjects on the same test, and (3) the consistency of the results from test to test with each subject. But even with such a liberal basis of interpretation, it can hardly be said that the results do more than "suggest" the inferences which have been drawn.

It is also obvious that fifteen minutes of experimentation in a laboratory dark-room tell us little about what might be the effect of the same brightness continued throughout the day, in shop or schoolroom. Fundamentally, the problem is not a laboratory problem, and needs study of a larger sort, in schoolroom or shop. The present study can be thought of as contributing little more than a definition of the problem, and a suggestion of methods. But the writer is convinced that there is here a real problem in applied psychology, well worth more extended study.*

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* But not, let it be repeated, in the laboratory! The writer finds himself, after three years of intensive research with mental tests, going over this manuscript with oddly mixed feelings. Not so long ago he returned from a four days' visit to a city of about 11,000 population: during those four days, he personally tested every child of the 1,500 children in that school system. And in contrast, three years of weary, discouraged puttering in a dark room, with a total of twenty-six subjects! He would hardly think of basing any very serious conclusion, now, on less than that many hundred. But by all this he does not mean to imply a loss of interest in the older type of experimental problem. Rather he believes that practically all of the laboratory problems can be handled, in the large, by the "test" method, with such massed cases. He is convinced that recent developments in the field of group-tests are much more important for experimental psychology than is usually realized. The group-test is really, he believes, a new method, of splendid promise, applicable to the entire range of psychological investigation. And it is to the "group-test" that he looks for a final conclusive treatment of the majority of the older experimental problems: that is, a majority of those problems which may be worth such treatment!

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THE NATURE OF THE AFFECTIVE JUDGMENT IN THE METHOD OF PAIRED COMPARISONS[1]

By M. Yokoyama

In a previous article the writer has reported data which show the manner in which judgments of pleasantness are dependent upon the simultaneous operation of the color and the form of geometrical figures cut from colored paper.[2] These results were obtained by the method of paired comparisons, the most usual tool in the experimental study of affection. In this investigation a great many introspective reports were obtained from observers at various stages of practice but were not fully presented for the reason that they threw but little light upon the immediate problem. These protocols, however, bear quite intimately upon the nature of the material which the method of paired comparisons yields, and it is proposed in the present paper to study them with especial regard to the nature of the affective judgment.

Titchener in 1908, discussing the nature of affection in the light of the experimental work available at that time, concluded that affection could be neither sensation nor attribute of sensation.[3] Since then, however, the face of introspective psychology has been changed by the realization of the need for a constant differentiation between mental process on the one hand and meaning on the other.[4] Young noted how affective judgments may be meanings of preference, not dependent directly upon affective process;[5] but he was not concerned with raising the fundamental issue, and sought primarily to rule out from his results a class of cases that was of a different order from those with which he was mainly concerned. The present writer seeks to raise the major question whether the method of paired comparisons can ever be relied upon to yield material other than affective meanings. He has no hope of settling finally the issue of the existential nature of affection, but believes that a demonstration of the inapplicability of this orthodox method to an experimental study of affective processes is worth the making. Certainly, so it seems to him, it is no longer possible for the experimental psychologist to assume that the mere utilization of a method assures results that exhibit laws of affective process.[6]

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[1] From the Psychological Laboratory of Clark University. The present paper is abbreviated for publication by the omission of many introspective reports. A bound manuscript containing detailed protocols will be found under the same title in the Clark University Library and may be borrowed under the usual courtesies of library exchange.

[2] M. Yokoyama, Affective tendency as conditioned by color and form, *Amer. J. Psychol.*, 1921, 32, 81-107.

[3] E. B. Titchener, *Psychology of Feeling and Attention,* 1908, 1-168, esp. 33-78, 167 f.

[4] Orth's distinction in 1903 between feelings and *Bewusstseinslagen* was, of course, prior to the full recognition of the relation of meaning to conscious attitude: J. Orth, *Gefühl und Bewusstseinslage,* 1903.

[5] P. T. Young, An experimental study of mixed feelings, *Amer. J. Psychol.,* 1918, 29, 237-271, esp. 252-258: *cf.* also C. E. Kellogg, Alternation and interference of feelings, *Psychol. Monog.,* 1915, No. 79, esp. p. 89.

Observers

Our *O*'s were peculiarly well qualified to bring out distinctions between meaning and process, because they had been sensitized in the 'atmosphere' of the Clark Laboratory to the process-nature of introspective material and to a persistent distinction between *Beschreibung* and *Kundgabe*. On the other hand, the very fact that the problem of meaning is so much 'in the air' at Clark may perhaps be thought to have predisposed them unfairly toward the discovery of meanings. However, without some such sensitization, which amounts to little more than a training in the refinement of modern introspection, it would have been hopeless to seek results which bear upon our problem at all.

An *O*, in approaching this problem, is apt to establish by his protocols his own theoretical bias. Hence we note here these biases, as indicated in formal statements by the *O*'s themselves. Observer B, the director of the laboratory, stated that for a number of years he had been sceptical of affection as an existential mental process and had been becoming constantly more convinced that it could be adequately accounted for as meaning or act. Observer D, a psychologist of some years standing, stated that she began the experiment without definite theoretical biases, but with more or less sympathy toward the theory that affection may be accounted for as meaning. Observer F, assistant professor of psychology, stated that he had always held that affections were simple and ultimate psychological processes, capable, except in clearness, of attributive description and coordinate with sensations and images. Observer P, a graduate student of three years training, was less interested in the general theoretical consideration of affection, but stated that he believed in the possibility of rendering reports upon feeling as independent mental elements. The protocols of Observer M are not considered in this paper for the reason that her lack of introspective training at the time of the experiment leaves her reports equivocal with respect to the point at issue. All the *O*'s, it may be said, however, insisted that they approached the experiment with an open mind, and that their sole motive was to make accurate and detailed descriptions and analyses of the experiences observed under these particular experimental conditions.

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[6] As, it would seem, could Titchener, *Philos. Stud.*, 1902, 20, 382-406; S. P. Hayes, *Amer. J. Psychol.*, 1906, 17, 358-393; W. S. Foster and K. Roese, *ibid.*, 1916, 27, 157-170.

Quantitative Summary of Introspections

The absolute frequencies of the occurrence of various affective items in the protocols are presented in Table I, which thus constitutes a rough summary of the introspections and of the individual differences of report among *O*'s. In general, it may be said of the table, the first items refer to the two stimuli compared: "2 P" (=two pleasantnesses) means that pleasantness was reported in connection with each stimulus, "P and U" (=pleasantness and unpleasantness) means that one stimulus was pleasant and the other unpleasant; "1 P" (=one pleasantness) means that one stimulus was pleasant and that there was no affection connected with the other stimulus; and so on. The item "affective or aesthetic characterizations" covers reports of the stimuli as "nice," "good," *etc.*

TABLE I

ABSOLUTE FREQUENCIES OF INTROSPECTIVE REPORTS AS CHARACTERIZED BY THE AFFECTIVE CONTENT INDICATED. P = PLEASANTNESS; U = UNPLEASANTNESS

Observer	B	D	F	P
2 P	12	4	2	1
P and U, or P and not-P	3	0	1	9
2 U	2	1	2	1
1 P	9	0	3	16
1 U	1	3	16	6
Affective or aesthetic characterization	6	30	7	5
No P or U	1	0	17	3
P or U not connected with stimulus	3	1	4	3
Total number of introspections, excluding duplications	31	36	34	39

Summary of Introspections

Observer B. The *Aufgabe* was seldom focal for B in the foreperiod. He was only vaguely aware of the instructions in terms of rigid bodily position and the fixation of attention upon the screen.

The process of comparing varied for him from time to time. As a rule, however, the initial tendency to determine a preference manifested itself with the first perception of one stimulus of the pair and took the form of an incipient absolute judgment of that stimulus; a judgment which never came immediately, but was always preceded by organic and kinaesthetic processes of a certain pattern, accompanying the visual perception and forming in turn the basis of the judgment. B's attention would then shift to the other stimulus and the same procedure would be repeated. During this period he would form a tentative, preferential judgment. The reaction and the announcement of the final judgment usually came with a second perception of one of the alternatives. In cases of difficult judgment the processes were more complex and involved. B would fixate now one and then the other stimulus several times in quick succession, with consciousness dominated by a general emotional stir-up that would seemingly delay the usual organic "building-up-process-for-pleasantness" (see below). In judgment of this kind, the decision was generally accompanied by awareness of the *Aufgabe.*

B's affective judgment was mediate throughout. He never observed pleasantness and unpleasantness as existential mental processes. He reported them as conscious attitudes or "meanings," carried by complexes of organic and kinaesthetic sensations or images. In order to declare a preference, therefore, it was always necessary for him to have this sensory mediator, and he was thus always in search of some such cue. (In the writer's previous paper he has characterized this attitude as a sensorial attitude.[7])

In the early stages of the experiment, B was uncertain of the proper cue for affection. At first he sought to base his judgments of pleasantness upon an *Eindringlichkeit* which consisted of the bare perception of the stimulus with high degree of clearness, intensity and distinctness (*Schärfe*) *plus* the kinaesthetic response of fixation and head-movement toward the stimulus.

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[7] *Op. cit.,* p. 88.

"Eye-movement went back to right stimulus and the eye stuck there, as pattern of eye-kinaesthesis, which was quite intense and carried the meaning: 'Striking color.' Then at once automatically throat-kinaesthesis, which meant incipient judgment for the right" (Nov. 18, 1919). "It was *eindringlich, i.e.,* a kinaesthetic context of receptivity. This kinaesthetic context was mostly eye-kinaesethesis, but had a little bodily adjustment in it too. . . . I do not know if anything carried the *P* or was the *P*. It may be that part of the eye-kinaesthesis in the *Eindringlichkeit*-context was or meant *P,* but I am not sure" (Nov. 26, 1919).

In more advanced stages, the criterion of pleasantness changed from *Eindringlichkeit* to what he termed a "building-up-process-for-pleasantness," which consisted of a suffused organic "glow," in quality something like the organic ache of stretching a fatigued muscle:

"Fixation of yellow with yellow both distinct and clear, and an organic attitude (organic sensations in trunk welling up, like a very mild 'affection' in the popular meaning of that term) that I might feel for a pretty tone or a little child. In content simply a pressure-pain glow (pleasant pain, of course) in the stomachic region or just above. Not unlike appetite in sensory nature; totally different in meaning. . . . When the glow got to a given pitch, it meant that the judgment was formed" (Dec. 11, 1919). "I am sure that the thing that builds up *P* is something that is sensory in nature and belongs to the tactual-kinaesthetic-organic groups of qualities. It is very weak and never very clear. I can scarcely localize it, yet I am sure it is in 'me' and not in my legs and arms. I should say it was above the abdomen and partly in the trunk, but that it also involves the facial muscles. Verbally I should label it a *glow,* perhaps a warm glow; but I do not think the sensation warmth actually figures. The phrase is intended to convey the meaning rather than the content. As sensory content I take the thing to belong to the class of pleasant organic pressure-aches, in which the pleasant ache of stretching fatigued muscles belongs. But it is nowise so distinct as the stretching complex; it is just a tiny, little, obscure, glowing tingle, as much painy as a bit of food going into the stomach under one of Carlson's appetites, and by no means so definitely localized" (Dec. 20, 1919).

Although he found exact localization impossible (*vide supra*), at times he reported this organic complex in chest and face, and at other times in the region of stomach and trunk. The 'building-up' process was slow at the very first; later it became very rapid, and still later its contents began to drop out of consciousness. In the final stage, it degenerated to a mere kinaesthesis of adjustment toward the stimulus wherein it was differentiated from '*Eindringlichkeit*' only by presence of a limited amount of "glow" or a trace of the kinaesthesis of a smile. It was at this stage of the experiment that B remarked that mental content might not be essential to the making of a preferential judgment.

"The attitude of preference developed for the right. It is a bare skeleton of the old 'building-up process' now. I look at the stimulus and turn (in imagery, I suppose) toward it, and sustain this attitude. The whole thing came in this time not immediately, but it built up very rapidly. When it had got built up, it was absolutely nothing more than a kinaesthesis of adjustment toward the stimulus, and it meant: 'I prefer that'" (Jan. 20, 1920). "There is a bit of process to the *P*, but less than there used to be; *i.e.*, in perceiving the circle there was a hint of a glow; conscious attitude, perhaps a trace of the kinaesthesis of a smile. Most of the *P*, however, was sheer meaning; *i.e.*, the circle was a pleasant circle, I knew it for a pleasant circle. My guess is that I am reacting to the meaning of *P*, that the content that carries the *P* is not essential to it, but generally exists" (Jan. 27, 1920).

Unpleasant experiences were infrequent with B and he furnished no complete description of a carrier of unpleasantness. One of his reports states:

"I can't describe *U*. I think it is some sort of organic pattern in my body. I actually believe that it is as much like the dull pressure-aches of nausea as *P* is like the bright contacts of tickle; but it is a far cry from *P* and *U* to these simple sensory things" (Dec. 20, 1919).

Observer D. Throughout the foreperiod, D maintained a relaxed bodily position. She had a vague consciousness of the *Aufgabe* to judge and to introspect carried in terms of kinaesthesis in throat and neck and relatively intense strains of the eyes from fixation upon the exposure apparatus.

Her consciousness during the main period consisted chiefly of visual and kinaesthetic-organic experiences. The initial act of preferring for her usually took the form of an attitude of incipient acceptance or rejection of one or the other stimulus, accompanied on many occasions by verbal (imaginal or actually innervated) characterizations of the stimuli. In all other respects her procedure in comparing was similar to B's.

D's attitude of acceptance consisted mainly of kinaesthesis in the head (an incipient nod) and an organic "glow" (chiefly circulatory and respiratory), or of general bodily relaxations and such vocimotor processes as: "That's a nice color;" "rather nice;" "it's a nice shape;" *etc.*

"When the red was fixated, there was a pleasant glow, accompanied by imagery in vocimotor terms: 'That's a lovely red.' I am retrospectively aware that there was a brief arrest of respiration, followed by a deep inspiration which carried the meaning of 'general satisfaction'" (Nov. 13, 1919). "My eyes followed the outline of the figure counterclockwise from lower right-hand corner. At the same time there was, besides the eye-movement-kinaesthesis, a slightly changed bodily attitude, especially involving kinaesthesis from holding head in particular way. It meant: 'That's a nice compact figure'" (Nov. 18, 1919). "There followed a particular kinaesthetic and organic attitude that meant my affective reaction to this particular stimulus: 'Rather nice' is what this bodily attitude meant. . . . The consciousness that conveyed the preference was a very vague affair, partly respiratory (as slight inhibition of breathing), partly kinaesthesis of the head (an incipient nod), partly the very turning of the eyes to the accepted stimulus" (Dec. 9, 1919). "There was some eye-kinaesthesis as my eyes followed the outline of the figure, and there was an 'evaluating consciousness' which consisted chiefly of mild but 'pleasantly toned' organic sensations (chiefly respiratory and circulatory) and a bit of throat-kinaesthesis that carried the meaning: 'It's a nice shape'" (March 9, 1920).

The attitude of rejection was not fully described. It seems to have been essentially a complex of kinaesthesis in the region of the diaphragm (constrained breathing, a feeling of tightness), throat-kinaesthesis (similar in quality to nausea), and the accompanying verbomotor processes meaning: "That's not so nice;" "dislike;" *etc.*

"Next came a kinaesthetic set which carried the meaning: 'That's not so nice a figure; it takes too much eye-movement to see it clearly'" (Nov. 18, 1919). "Then followed an attitude of incipient rejection of this stimulus in terms of kinaesthesis in diaphragm region, *i.e.*, constrained breathing; and a bit of throat-kinaesthesis that was related to nausea" (Jan. 8, 1920).

Like B, D found no affective elements as distinguished from sensations. *P* and *U* to her were simply conceptual terms, embracing, among other things, affective characterizations such as "nice," "lovely," "not nice," *etc.*, and designating at the same time an attitude of acceptance or of rejection, which was in content nothing more than a group of kinaesthetic and organic processes. These organic and kinaesthetic sensations appear to be for her a *sine qua non* of affectively toned experiences, for the reason that *P* and *U* are regarded as meanings carried by these organic and kinaesthetic sensations.

Observer F. The foreperiod in the early part of the experiment may be characterized for this *O* as a period of active attention. His eyes were always fixed upon that portion of the screen where the left-hand stimulus would appear. There was a focal visual perception of a limited part of the screen, and intense kinaesthesis in brows, eyes, jaw, neck, chest and abdomen; all of which constituted F's attitudinal response to the *Aufgabe.* Later on this attitude gradually changed into one of passive attention, in which the kinaesthetic and organic processes were very much less marked than before and the visual perception of the screen was alone focal.

In the initial stages of practice, the main period was especially rich in affective and kinaesthetic experience, although the clearest part of consciousness was always the visual perception of the stimuli. In these stages F, like B, made a tentative, absolute judgment of *P* and *U* separately upon each of the alternatives before determining his preference. These judgments were almost invariably accompanied either by some kinaesthetic-organic adjustment (a nod, strains in the eyes, kinaesthesis in nose and upper lip, or the like) or by certain imaginal or actually innervated verbal expressions, such as: "not so good;" "blue, fine;" *etc.*

> "When the screen was raised I was aware of a rapid focal visual perception of the right-hand form. Great *P* raising rapidly to an intense maximum. Verbomotor processes: 'Oh, that one' accompanied by motor image of drawing my hands apart; left hand upward and right hand downward" (Nov. 13, 1919). "Very focal and clear perception of it (stimulus) with rapidly appearing *P*, which rapidly increased in intensity. Verbomotor 'Blue, fine.' Rapid relaxation of bodily strains, deep inhalation and closing of the eyes" (Nov. 13, 1919). "Non-focal kinaesthetic sensations (actually innervated) in nose and upper lip (facial expression of disgust). This meant to me a very unpleasant stimulus" (Nov. 18, 1919).

The termination of preferential judgment usually came while F's eyes were firmly fixing the stimulus and was followed by general bodily relaxations. The difficult judgments were, as for the other *O*'s, characterized by several rapid shifts of fixation and by the presence of unpleasant feeling and strains.

In the later stages the process of comparing became highly mechanized and the final judgment came immediately upon perceiving both stimuli, without even slight *P* or *U* entering consciousness. F's judgment now was reduced to a motor attitude, which consisted of the visual fixation of the stimulus preferred and of his mechanical reaction upon the key.

> "This visual perception was terminated by closure of the eyes which was either accompanied by or immediately followed by a very mechanical reaction, both of which were without other antecedent so far as I am aware. . . . I had no affective processes in this experiment whatever" (Jan. 29, 1920). "This visual perception was terminated by a series of processes which took place in the following order. (1) Absolutely mechanical and automatic reaction on the key without other conscious antecedent. (2) Closure of the eyes. (3) Rapid exhalation. (4) Relaxation of the widespread bodily and muscular strains of the foreperiod. . . . I am sure there were no affective processes in the entire experience" (Feb. 12, 1920). "Almost at once upon the beginning of the visual perception I found that I was reacting on the key in a perfectly mechanical fashion and without other antecedent" (March 4, 1920).

F reported *P* and *U* as if they were existential mental stuff. They were always accompanied by organic or kinaesthetic processes of some sort, but F never saw any connection between these processes and the *P* and *U*. He habitually noted the intensity and the temporal course of a given affection: "Very focal and clear perception of it [stimulus] with rapidly appearing *P* which rapidly increased in intensity;" "relaxation of general bodily strains and rapid decrease in the intensity of *U*." On the other hand there is nothing in F's reports that insists unequivocally upon the independent status of affection; most of his statements could have been made of conscious attitudes. His judgments, moreover, appear to have been conditioned directly upon the stimuli, especially in the final stages of the experiment, and not to have been consciously mediated by the reported *P* and *U*. (*Cf.* quotations above.)

Observer P. P's mechanism of comparing was similar to the other *O*'s. The foreperiod was characterized for him by the 'attitudinal set' or *'Einstellung'* for carrying out the instructions. This set consisted of muscular strains about neck and shoulders, and organic sensations about lower oesophageal and upper stomachic regions. A general tenseness of body remained constant, but non-focal, until the appearance of the stimuli.

With exposure P fixed his attention alternately upon the two stimuli, evaluating each of them in its turn. Rapid, telescoped, verbal characterizations at times would accompany the perceptions: "That red's good, but orange not bad;" "triangle," *etc.* At the beginning of the evaluating process the non-focal tenseness of the foreperiod would increase momentarily in intensity, and then gradually break down. There would then be a general 'welling up' of organic sensations or an organic 'glow' in the region of stomach and oesophagus, accompanied by *P* or *U* as the case might be, and issuing in the formation of the preferential judgment. When judgment was difficult, the period of evaluation would be much prolonged and several shifts of fixation would take place. In these cases consciousness was dominated by unpleasantly toned organic processes, and the decision occurred only after reappearance of the awareness of the *Aufgabe.*

"This [perception of the form at right] was followed by rapid welling up of acute, intense, organic sensations, localized in oesophagus and stomach, rather intense *U*. Increase in the bodily strains, especially eye-kinaesthesis and in skull" (Nov. 15, 1919). "Focal visual perception of the stimulus at the right and very non-focal visual perception of the stimulus at the left and very non-focal, indistinct, vocimotor-auditory imagery of 'red,' accompanied by slight *P*. . . . Then stimulus at right dropped out to a very non-focal level. The stimulus at left then again became focal. This was accompanied by the slight organic sensations localized in the region of stomach. Then reaction to the 'red-consciousness'" (Dec. 23, 1919). "Fairly focal visual perception of the stimulus, accompanied immediately after by the image (visual) of the left; then *U*. This was accompanied by organic sensations around stomach and oesophagus, a slight increase

TABLE II

Occurrence of Various Data of Consciousness (Meanings, Processes) at Various Stages of the Experimental Procedure. 4 Observers. P = Pleasantness; U = Unpleasantness

Obs.	Datum	Initial Stage	Second Stage	Third Stage	Final Stage
B	P and U as:	Psychological meaning	Psychological meaning	Degenerated psychological meaning	Practically biological meaning
	Process-basis of P and U.	Eindringlichkeit = P. Organic and kinaesthetic complexes = P.	Building-up-process (organic) = P. Organic and kinaesthetic complexes = P.	A trace, merely, of building-up-process (organic) = P.	P = stimulus itself. Rarely incipient tendency for building-up-process.
	Genetic decay of process-basis.	Rich and well-marked.	Rich and well-marked.	Partial decay of organic process. Rarely bodily attitude only.	Almost complete decay of organic processes. Usually bodily attitude only.
D	P and U as:	Psychological meaning		Practically biological meaning	
	Process-basis of P and U.	Attitude of acceptance (organic and kinaesthetic) = P. Attitude of rejection (organic and kinaesthetic) = U.		Vestigial remains of attitudes of acceptance and rejection.	
	Genetic decay of process-basis.	Rich and well-marked.		Partial but incomplete decay; occasional organic and kinaesthetic processes.	
F	P and U as:	Existential mental processes, either unique elements or conscious attitudes		Preference = biological meaning No longer P and U as mental process	
	Concomitant relevant sensory processes.	Organic and kinaesthetic processes.		Bodily attitude of fixation of stimulus, but no prominent organic or kinaesthetic processes.	
	Genetic decay of relevant processes.	Rich and well-marked.		Almost complete decay of affective and organic processes.	
P	P and U as:	Existential mental processes, either unique elements or conscious attitudes			
	Concomitant relevant sensory processes.	Organic and kinaesthetic processes.			
	Genetic decay of relevant processes.	Rich and well-marked; practically no decay.			

in muscular tension (general, non-localizable) and by kinaesthesis about eyebrows" (Jan. 21, 1920). "Rapid welling up of intense organics about the stomach and oesophagus, accompanied by *P*" (Jan. 31, 1920). [Difficult case]: "Non-focal rapid vocimotor imagery: 'Huh, neither' followed by very rapid shift of fixation from right to left and back again (4 or 5 times), accompanied by organic sensations. Then slight awareness of *Aufgabe* and vocimotor imagery: 'Oh, left is a little better'" (Nov. 18, 1919).

In the advanced stages P's judgment was mechanized and was made at times without presence of reportable *P* or *U*.

Like F, P reported *P* and *U* as if they were mental processes distinct from sensations. He noted their intensities and durations in making comparisons. The organic and kinaesthetic sensations, which were concomitant with affections, he almost always localized in the region of the oesophagus and of the stomach. He gave no clear-cut descriptions of these sensory processes. It seems that the organic glow, usually concurrent with *P*, was of a milder, more diffused nature and thus differentiated from the acute, intense, straining quality that appeared closely associated with *U*.

General Summary

Table II constitutes a summary of the protocols fractionated into successive genetic stages for those *O*'s for whom marked serial changes of consciousness appeared. The table gives for B and D the nature of the meanings of *P* and *U*, the process-bases of these meanings, and a characterization of the extent of the decay of the sensory content. In accordance with Titchener's context-theory of meaning,[8] the table uses the term "psychological meaning" for a meaning that is reportable both as such and in terms of its sensory carrier, and "biological meaning" for a meaning reportable as such that occurs with little or no observable sensory basis. F and P, who imply the independent elementary status of feeling, are summarized in the table by a statement of concomitant organic processes and a characterization of the decay of the total conscious affective experience.

The question of the mental status of the affective process comes up as an issue between observers. B and D were emphatic in their denial of *P* and *U* as directly observable, nonsensory, unique, mental processes, but reported them consistently as unique meanings, carried by complexes of organic and kinaesthetic processes. In any case observable processes were to be expected, since meanings in the early stages of their life history can be expressed in terms of existential sensory process. The issue hinges therefore upon the determination of the sphere in which *P* and *U* are unique and possess independent status, the location of this sphere in the realm of observable mental process. Unfortunately the question is one that we cannot finally determine here; we must content ourselves merely with an additional contribution. B called *P* an "attitude-for-pleasantness" and D an "attitude of acceptance." The organic and kinaesthetic bases of these attitudes were (especially for B) very vivid in the early consciousnesses; they underwent, however, the usual degeneration to which the sensory carriers of meaning are subject, and as the experiment progressed they became finally illusive tags of sensation or disappeared altogether. In other words, "psychological meaning" became "biological." In contrast to B and D, F and P reported *P* and *U* as if they were existential mental processes, although they found no occasion especially to assert their existentiality. It is not to be expected that an *O* will especially assert the obvious, and it is thus a reasonable interpretation of their reports to infer that the process-status of feeling was obvious to them and that a present feeling required therefore nothing further than its mention. Nevertheless, when the reports of F and P are taken in the light of the scepticism of B and D, we may perhaps wonder whether the readiness with which *P* and *U* are spoken of as such is a readiness that comes from their independent existential status or the readiness with which conscious attitudes are reportable as mental units. The latter interpretation is supported by the course of F's consciousness (Table II), in which a degeneration of the affective experience was covariant with the degeneration of the concomitant organic and kinaesthetic sensory content.

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[8] For general statements of the context-theory of meaning, see E. B. Titchener, *Thought-Processes*, 1909, 175 ff.; *Text-book of Psychology*, 1910, 367 ff.; H. P. Weld, Meaning and processes as distinguished by the reaction method, *Titchener Studies in Psychology*, 1917, 181 ff.

Taken all in all, our findings may be summarized with respect to the particular point at issue as follows:

1. *P* and *U* may be meanings for any observer; for B and D always; for F consistently at one stage; for P occasionally.

2. Evidence for *P* and *U* as nonsensory existential processes is weaker than evidence for them as sensory mental processes. B and D explicitly describe them as sensory; F and P do not explicitly describe them but merely name them. There is no statement in any introspection that could not have been made of a conscious attitude.

3. Organic sensory content is the *sine qua non* of *P* and *U*, except in advanced stages of degeneration; for B and D as carriers of the pleasant and unpleasant meanings; for F as concomitant or carrier; for P as concomitant.

4. Conscious *P* and *U* drop away from the affective perception in the course of its decay, thus following the law of perceptual contextual sensations.

5. The loss of *P* and *U* is concomitant with the diminution or disappearance of the sensory organic contents of consciousness.

6. The two preceding items of fact indicate that *P* and *U*, whether as sensory or nonsensory, are to be thought of as integral to the total organic complex.

It follows therefore:

(*a*) That *P* and *U* (*of the method of paired comparisons*) are most universally and definitely statable as meanings;

(*b*) that on the side of process *P* and *U* are predominantly sensory; and

(*c*) that there exists a portion of the process-aspect of *P* and *U* which is equivocal in its essential nature (*i.e.*, it is not certain whether it is sensory or nonsensory), but that this equivocal portion of the process-complex follows a law of sensory decay.

The case is by no means conclusive against the status of affection as independent mental process. For one thing it applies only to the method of paired comparisons. It does show, however, that much greater concern needs to be given to the part played by meaning and the illusive carriers of meaning in the consideration of feeling as elicited under this method. Upon the broader extension of these conclusions to the problem of feeling in general the writer is unprepared even to make a surmise. He feels, however, that his results indicate that the method of paired comparisons can no longer be taken as a typical laboratory setting for affections of process-nature, and that the experimental establishment of the process-affection is therefore more than ever in need of an experimental method.

A STUDY IN LOGICAL MEMORY[1]

By Sarah D. Mackay Austin

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INTRODUCTION

(1) *The Problem.* The chief aim of this investigation has been to see if certain of the laws established for the recall of nonsense material will hold for sense or logical material as well. The special phase of this problem which has been studied is to determine whether divided repetitions prove more effective than accumulated repetitions in learning material with meaning. The more important subordinate problems which have developed from this larger problem are: the relation of logical learning to rote learning, or memory for ideas as opposed to words; and, in particular, the effect for recall of distributed as compared with accumulated repetitions; and the influence of time (the curve of forgetting) upon the amount and character of the material retained. Although the chief interest in the problem has lain in the quantitative aspect of the process of remembering, careful introspections taken

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[1] From the Psychological Laboratory of the University of Michigan.

throughout the experiments show something of the qualitative nature of the problem as well.

(2) *Literature.* Relatively little of the vast amount of literature which has been published on memory and the learning processes has a direct bearing on this problem. Only that work which deals more or less directly with the learning of logical material will be briefly reviewed.

(1) *Learning of (a) Nonsense Material and (b) Logical Material.*

(*a*) The pioneer experiments of Ebbinghaus (3) and Jost (7) in the field of memory were done almost entirely upon material without meaning. Following them have been investigators who have used both material without and material with meaning, as Radossawljewitsch, who used both in his study of forgetting. A number of experiments have been made upon logical material, but usually for verbal recall, not for the memory of ideas as opposed to words. Balaban (1) studied the difference between logical and mechanical memory, using syllables, words, and compound words.

(*b*) Henderson(6) has carried out probably the most complete research with logical material. He had 212 subjects, ranging from school children of the fifth grade to graduate students in college. A typewritten copy of a short prose selection was handed to each subject to be learned in 3 min. At the expiration of that time the subject was required to write as exact a reproduction as possible. The aim of the experiment was to determine the amount and character of the material retained. The subject was then tested two days and four weeks later.

The results of 259 complete papers were scored on the basis of the story, topics, sub-topics, details, and words. All words of the original text that were reproduced in their former contexts were scored full value. Commonplace words, articles, prepositions, and conjunctions were not scored when reproduced out of their context. On the other hand, an unusual word was regarded as remembered even though it appeared in the wrong context. Henderson found that the older students got ideas more than words, and that there was a slight tendency for more advanced students to remember better. He found also that a single reading, concentrated, was nearly as effective for immediate reproduction as 3 min. of study. Details disappeared owing to generalization and there was a tendency to substitute. The details least consonant with the thought were usually forgotten.

Whipple (18, pp. 394-403) refers to the same process of generalization. Referring to Binet and Henri, he says that they found a tendency to express ideas in their own terms, rather than those employed in the original passage. They estimate that memory for connected sentences is approximatly 25 times as good as memory for discrete terms. The general conclusion, we may say, of the investigators using logical material is that ideas are retained, not the original words and that there is a strong tendency to generalize when learning subject-matter with meaning.

(2) *Distributed as opposed to Cumulative Repetitions.*

(*a*) For nonsense material, Ebbinghaus showed the advantage of distributed repetitions in learning. Using the " Saving Method," he found a saving of more than 30 repetitions in relearning by distributing his repetitions over 3 days. Jost,(7) likewise, found a saving in relearning with repetitions scattered. He used the "Saving" and "Treffer" methods, and found that 30 repetitions at once tested on the following day were very ineffective as compared with the 30 repetitions distributed within limits. He found an advantage in scattered repetitions in both the methods which he used; a saving in relearning, and also a greater number of successes. He concluded that the "Treffer" method was the better of the two.

Perkins'(15) experiments upon nonsense syllables showed also the greater advantage of repeitions distributed within limits. Her main problems were, first, "to discover if there was a limit to the degree of distribution that might be advantageous" and, secondly, to determine, in cases where more than one repetition was made in a single day, "if learning was easier when more than one day was allowed to intervene between the groups of readings, and to discover, if possible, the most favorable period." The results showed that the highest percentage of success was obtained by the use of one reading every third day. She found that, in general, the smaller the number of repetitions used at a sitting, the higher was the percentage of success.

(*b*) For sense material. The most recent experiments in which scattered repetitions are compared with cumulative, and which are very closely related to this problem, are those of Edwards(4). He compared the relative value of 6.5 min. as opposed to 4 plus 2.5 min. when studying a page of history. It was so arranged that the total amount of time was the same for all. Half of each class was a review group, half a non-review group. His subjects were high school and grammar grade pupils. The non-review groups studied 6.5 min. and wrote 12 min. and took an examination from 10 to 12 days later. The review groups studied 4 min., wrote for 12 min., and about 5 or 6 days later reviewed for 2.5 min. It was found that the review groups were better without exception in reproduction. The review groups could do sometimes 50 per cent more than the non-review groups per pupil.

(3) *The Curve of Forgetting of* (*a*) *Nonsense Material,* (*b*) *Logical Material.*

(*a*) In any memory problem, one phase is its relation to the "Curve of Forgetting." Although material with meaning is retained better than nonsense syllables, the same general tendencies are shown in the rate of forgetting. Working with nonsense syllables, Ebbinghaus(3) found that 42% of forgetting took place after 20 min.; 56% after 60 min.; 64% after 9 hrs.; 66% after 24 hrs.; 75% after 6 days; and 79% after 31 days. That is, forgetting took place rapidly at first, and then more slowly.

Radossawljewitsch(17) also found approximately the same results for nonsense material, *i.e.,* a decided loss at first, then a more gradual loss of the syllables that had been learned. His experiments differed from those of Ebbinghaus only in a smaller amount forgotten at first. He found a relatively slow period of forgetting between 6 and 21 days.

(*b*) Although the major portion of his work was done with nonsense syllables, note may be made of one experiment by Ebbinghaus on material with meaning. Even after 22 years, he found a retention of stanzas of poetry learned so that they could be said once, perfectly, and not seen in the interim. Radossawljewitsch(17) also used material with meaning; a portion of Schiller's *Siege of Troy*. Two stanzas were assigned to his subjects who read them through repeatedly until they could reproduce them twice without error. He found that one-third of the material with meaning was gone in 2 days, one-half in 7 days, while material without meaning was lost much more quickly. With both nonsense and sense material, with these two investigators, the same general "Curve of Forgetting" holds, namely that at first forgetting takes place rapidly and then more gradually.

In connection with the curve of forgetting, Bean(2), in his monograph, presents results from his experiments on forgetting of relatively simple motor acts, sensations with relatively little meaning, and sensori-motor experiences. He says that his own results do not warrant altering the general character of the Ebbinghaus curve of forgetting, nor introducing any other general types of curve. But the rate of forgetting is variable, depending on (1) the degree to which the material had been learned in the first place, (2) the distribution of the repetitions, (3) the kind of material learned, (4) the method by which it is measured, and (5) individual difference in retentiveness.

II. The Method of Experiment

(1) *Subjects.* The following persons served as subjects, Prof. W. B. Pillsbury, Miss Nellie Perkins, Mr. J. Winter, and Mr. H. W. Crane, who performed memory tests during one college year for this experiment. Five other persons served as subjects for a few tests, so few that they had to be discarded. The class in Introductory Psychology furnished material for several tests. The writer served as a subject for a year and a half, and then after an interval of three and a half years continued the experiments for about six months. In the following experiments, these subjects will be designated as follows: Prof. W. B. Pillsbury, Subject I; Mr. Crane, Subject II; Mr. Winter, Subject III; Miss Perkins, Subject IV; Miss MacKay, Subject V.

(2) *Materials.* The so-called logical material was of two kinds for each subject. In each case, the subject chose or was assigned material with which he was relatively unfamiliar but in which he was interested to some extent. Subject I used Organic Chemistry and Russell's *Principles of Mathematics.* Subject II used Embryology (Piersol) and History (a detailed account of the Battle of Gettysburg). Subject III used the same Embryology, and Descriptive Geology. Subject IV used the same Embryology, and a History of South America. Subject V used Economics and a History of Scotland. Embryology was a new subject to those who used it in this experiment, and it presented difficulties in terminology as well as in meaning. The assignments in this were very short.

During the first part of the experiment, Subject V used a text book in *Money and Banking.* Experiments begun after an interval of three and a half years were made with the same text-book and the publications of the National City Bank in addition. The history used in these later experiments was the literature sent by Sir Gilbert Parker to the members of the American Bar Association, Wilson's *Division and Reunion,* Walling's *History of Socialism,* and a *History of French Literature.* In every case, the subjects used books they had never seen before.

Previous experiments in the use of sense material have been limited for the most part to sentences, short paragraphs, or two or three stanzas of poetry. In this experiment, the portions to be learned were much longer, and were always of about the same length. In mapping out the material for the other subjects, as well as for myself, I attempted to have the assignments as nearly equal in length as possible. I laid more emphasis, however, upon the logical place to end the assignment than upon having a uniform number of ideas. The assignments varied, with the exception of those in Embryology, from two pages to four or five. It may be noted, in this connection, that the length of the assignment had little effect upon comparative results.

An assignment covering several pages gives a test of logical memory in its practical aspect, since in school, in college, and in practical life, one is never limited to a mere stanza or paragraph. Such subjects as history and economics, for example, were chosen because they were the sort of subject-matter one generally reads, in addition to being logical material.

When I began working on this problem, after a three and one half year interim, it was necessary for me to use shorter assignments than those which have been described, assignments requiring perhaps 5 to 7 min. to read. This method enabled me to have many tests under way at the same time on different assignments of both history and economics.

Note may be made of one great difficulty which arose and made the number of experiments fewer than they otherwise would have been. The subject-matter for the tests was the consecutive material in a chosen text. Thus two pages, let us say, were chosen for the first assignment, the following two pages for the next assignment, the following two pages for the third, and so forth. If history, for example, had been read five times in one day and was to be tested in two weeks, no further experiments could well be performed in that subject until that test had been made. If a second experiment had been begun on the following pages before the first had been finished, such confusion in both learning and recall would have resulted that both final results would have been invalidated. It would have been impossible to distinguish one assignment from the other when the time for testing came.

The other subject-matter, embryology, could be introduced in the interim, and experiments made in that, but even then there was a loss of time in the experiments extending over longer intervals. As is evident, this difficulty did not present itself in the short-time tests. Repetitions could be made and the subject-matter tested on the following day; and the questions could be answered two days later and the next test begun. I obviated this difficulty, of the loss of time, in my own later experiments by learning various assignments in several books in both economics and history and having several tests overlap.

(3) *Method of Learning.* The total number of repetitions used throughout the experiment was five, which number was chosen arbitrarily. One repetition consisted in reading the assignment through once very carefully. The subject read his assignments at the same time of day, in so far as was possible. He chose his own method, which was to be uniform throughout the experiment; he could read aloud or not as he wished, slowly or rapidly. After reading the assignment, the subject attempted to drop it completely out of mind. This was very difficult to do at first; since the subject knew it should be remembered, it would be recalled again and again in the effort to have all the subject-matter of the assignment available when the test was to be made. Had this method been continued, it would have introduced a great source of error; some portions might have been reviewed more than others, portions incorrectly recalled might have been gone over until they seemed a part of the context, and there might have been a constant effort to keep the material fresh in mind, particularly if the test were two weeks away. A little practice, however, enabled the subjects to dismiss the assignments completely after reading.

The subject read his assignment as directed; for example, five times in one day, once each day for five days, or three times in one day and two times the next, and wrote all he could recall without any assistance of any kind at the expiration of the stated time. Two days later (this time was also chosen arbitrarily) he answered questions which had been carefully made out, covering as nearly as possible all the ideas in the given assignment. The repetitions were scattered over varying intervals of time, from five repetitions in one day, to one repetition every fifth day. The extreme time-limits for testing were from two hours to three, four, and six weeks, in one case, each. The majority of the tests, however, lay within the limits (including) two hours and one month. Anything interesting in the way of introspection at the time of reading or of testing was noted by the subject.

(4) *Scoring of Material.* The purpose of this investigation, as has been stated, has been to see how many ideas of any given prose assignment could be retained after certain periods of time, after five careful readings at varying intervals. The original assignment was scored very carefully. I read over the text and counted the number of ideas in this way. One half of a sheet of paper was used to score the ideas in the text, and the other half was used for the test. Usually the first and last words, with abbreviations between, of what I called an idea were written down. To give a concrete example from one of the assignments: "Only that statesman, writer, or sociologist, has the hearing of the public to-day who can bind his proposed reforms into some large far-seeing plan." This was abbreviated as follows:

Socialism

Pages 2 and 3

Text	Test
1. Only states, b. f. plan.	2.
2.	2.
3.	3.

In this way, when it came to scoring a test on the other half of the sheet, the original idea was perfectly obvious. The original assignment, in addition, was always at hand for reference in scoring the tests; for often the idea of the subject was correct, although phrased entirely differently. Just as the original text was scored, so the report of the subject was carefully gone over, the total number of ideas in each found, and the percentage of the ideas recalled in each case was calculated. No scoring was done for words or different parts of speech, the purpose being solely to find the percentage of the whole number of ideas in the original text which could be reproduced.

The scoring was done very closely, making the percentages low, particularly in tests extending over several weeks or more. This was necessary because a high degree of accuracy was obtained in an immediate test, say two or twenty-four hours, and many details were given which would have been forgotten in a longer interval.

The first question which naturally arises in regard to the method of scoring is: how were wrong or partial ideas scored? The answer is that, as a rule, either the subject-matter had been entirely forgotten or was recalled correctly. This was found to be true in general throughout the experiment, although there were a few exceptions. Wrong ideas, dates, or figures would be found occasionally, but comparatively seldom. These were simply disregarded if the idea was correctly recalled. If several partial ideas were found in one test, credit was given for a whole idea, but little scoring of partial ideas was necessary.

All figures in the following tables show the percentage of the material retained, that is, the number of correct ideas the subject retained. The first figure shows the original test, the second, the test made with the aid of the question. Thus, in subsequent discussion, 0—28, means that nothing was remembered by free recall, and 28% of the total number of ideas in the given assignment, with the aid of the questions.

III. Tabulation of Data and Introspections of Subjects

In the following pages the results of each subject will be summarized, and the introspections of interest noted, with particular reference to their bearing upon the problem of imagery or imageless thought. This experiment has shown that, while comparisons of the results of the same individual can be made, none can be made of the results of different individuals.

Table I, Subject I. (1) *Organic Chemistry.* This material required 15 or 20 min. each day to read. It proved interesting, and no pertinent introspections accompanied it. The greater value of divided repetitions is shown as compared with cumulative.

(2) *Mathematics.* These sections were long and difficult. The introspections show: "Not all understood and did not reread anywhere; general impression vague," on the first day. Second day: some mind-wandering. Third day: still rather blind; last part still hazy. Fourth day: "Felt disinclination for the work, one page not appreciated at all in the reading." Again, the introspections taken at the time of reading another assignment: "Read once, room quiet and conditions favorable, pretty well followed. Conditions favorable on following day, and seemed to understand and often to anticipate."

By reference to the table it will be seen that the assignment read two times the first day and three times the second and tested in two weeks gave 0-28 per cent. The notes taken at the time of reading show that it was not understood at the time. The introspections recorded at the time of writing the test show the dependence of recall upon some form of imagery. "At first there is nothing more in the way of recall than a picture of a large page with rather dirty type, with Sec. No. 41 rather low on the left-hand side of the page. As

I hold myself to it, I think that the subject-matter must be of 'implication,' but with no memory of what phase of it may be considered. All is accompanied by unpleasant strain sensations. Nothing more comes. I give it up for a time. Nothing more would come yesterday. Occasionally a stray word would present itself, with the feeling that something else was behind it, but nothing definite would develop, and I could not be sure that the idea came from the right passage."

Summary, Subject I. (1) *Organic Chemistry.* (*a*) Distributed repetitions proved to be more effective than those which followed one another closely, but there seemed to be little difference in value in the spacing of the repetition. That is, material read each day for five days was remembered as well as that read three times the first day and twice the second day. Also material read twice in one day and three times on the following day was recalled as well as that read on alternate days. The results are too few in number to give any definite conclusions, except that when the subject-matter is to be tested in two weeks, repetitions which are scattered are more advantageous. (*b*) The questions brought back more than free recall. (*c*) There was a decrease with time in the amount retained.

(2) *Mathematics.* There were too few results to give any conclusions.

TABLE I

Subject I

Organic Chemistry			Mathematics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
19.9	19.9	5 times in one day	....	....
25.8	48.6	Daily for 5 days	6.7	14.1
42	52	Alternate days for 5 days	29	40
29.1	55	3 times 1 day, twice 2nd day	24.3	45
40	65	2 times 1 day, three times 2nd day	0	28
		Tested in 10 Days		
43.9	46.1	Daily for 5 days	....	....
		Tested in 7 Days		
		Daily for 5 days	14.8	24.4
		Tested Following Day		
65	74	5 times in 1 day	....	....
72.7	83.3	Daily for 5 days	32.6	36.7

Table II, Subject II. (1) *Embryology.* These assignments were short, owing to difficulty in terminology. The approximate time for reading was from 5 to 6 min. Material read once each day for five days and tested in two weeks gave the highest percentage of any of the two weeks' tests. One introspection may be noted, showing the subject's dependence on visualization. "I can not remember much about this material. I could not visualize; it has no meaning for me because I can not picture it, or connect it with anything in my experience."

(2) *History.* The history read once every fifth day gives a very low percentage, showing that repetitions so far apart are probably past the limit of advantageous dimension. One introspection may be quoted: "This history I did not feel nearly as sure of as I did the first. I have a general sort of hazy notion; can get but very few facts, and they seem vague and unconnected. During the learning process, I was conscious of the confusion of the details of the material with that of the first. Had I been able to go back over it, I think I should have been able to keep it straight."

The portion read five times in one day and tested on the following gave a test that was an almost exact reproduction of the text, including figures and number of troops. This was very unusual. It may be noted that even after two days, with the additional help of the questions, there was a fall of 5%.

Summary, Subject II.[2] (1) *Embryology.* The results were too few in number for any positive conclusions.

(2) *History.* (*a*) Repetitions distributed within limits (up to one repetition a day for five days) proved most effective for recall when the subject-matter was to be tested in two

TABLE II

Subject II

History			Embryology	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
31.1	40.5	Daily for 5 days	43	75
0	43.4	....	....	....
....	....	3 times 1 day, twice 2nd day	0	10
....	....	2 times 1 day	....	....
....	....	2 times 1 day, 3 times 2nd day	25	40
4.12	10.9	Once every 5th day	22.2	....
31.9	40	Alternate days for 5 days	15.7	27.7
		Tested in 7 Days		
47.6	54.3	Daily for 5 days	....	....
		Tested Following Day		
98.6	92.3	5 times in 1 day	....	....
70.8	72	Daily for 5 days	65.1	65.1

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[2] These subjects, I and II, worked irregularly and the series is not complete. The results, however, show the tendency, confirmed by the more complete series which follow, for the scattered repetitions to have a greater value than the accumulated when the matter is recalled after the longer periods of time, while the two are of approximately the same value for recall after a single day.

weeks. (*b*) For immediate recall the accumulated repetitions proved more valuable than distributed. This, however, was in a single instance only. It shows the tendency, confirmed by the following series of other subjects, that for immediate use the value of the scattered and of the massed repetitions is about equal. (*c*) The questions brought back more than free recall. (*d*) There was a gradual decrease with time of the amount retained.

Table III, Subject III. (1) *Embryology.* The assignment read five times in one day and tested in two weeks gave the following introspection. "I can not recollect distinctly what the material was about. I studied it in succession I think, but I don't remember what it is on. I may be able to answer some of the questions when I see them." It is interesting to note, in relation to this, that the portion read once every fifth day and tested in two weeks gave almost exactly the same percentage and about the same introspection. "Have tried to think just what this section is about, but I can't place it. I have a mass of facts before my mind but I can not pick out the pertinent ones." Repetitions which come too close together or too far apart are of little value. There is utter blankness when trying to recall the subject-matter. Only when some form of imagery is possible does any of the content return.

Another introspection may be noted. "I am completely mystified as to the exact content of the paper I am supposed to reproduce to-day. Not that I do not know some of the facts, but I can not distinguish which they are. I think I shall be able to answer some of the questions without difficulty." When the questions were answered, the following comment was made. "This is a terrific paper, but I can not account for it exactly. Maybe it is due to overwork, or I was not in proper shape when I studied it."

Summary, Subject III. (1) *Embryology.* (*a*) Distributed repetitions are more effective than accumulated, without exception, in the two weeks tests. One reading a day was the most effective distribution. In the material read on alternate days, the initial test with free recall is absent, but the question recall shows the highest percentage of material retained in the two weeks series. (*b*) Repetitions too close together or too far apart are of little value. (*c*) For immediate recall, accumulated repetitions were as valuable as distributed. (*d*) The questions brought back more than free recall. (*e*) There was a decrease with time in the amount retained.

Without exception, with Subject III, distributed repetitions proved more effective for recall after intervals of seven days to two weeks.

(2) *Geology.* The tests were too few in number for a comparative summary. The two points which were shown are: (*a*) Questions brought back more than free recall; (*b*) There was a decrease with time in the amount of the material retained.

TABLE III

SUBJECT III

Geology Free Recall	Geology Question Recall	Repetition	Embryology Free Recall	Embryology Question Recall
		Tested in 2 Weeks		
33.3	22.2	Daily for 5 days	26.6	46.6
....	....	5 times in 1 day	0	16.6
13.7	35.5	Alternate days for 5 days	....	57.1
....	....	4 times 1 day, once 2nd day	0	24.2
....	....	Once every 5th day	....	16.3
....	....	3 times one day, twice 2nd day	0	25
		Tested in 10 Days		
13.8	33.3	Daily for 5 days	....	....
35.9	....	....	....	....
....	....	2 times 1 day, 3 times 2nd day	61.9	....
		Tested in 7 Days		
....	....	Daily for 5 days	38.4	42.1
....	....	Once 1 day, 4 times 2nd day	0	33.3
		Tested Following Day		
61.7	67.6	5 times in 1 day	47.7	63.1
....	....	Daily for 5 days	40	40
40	40	3 times 1 day, twice 2nd day	....	....
		Tested in 2 Hours		
47.2	41.7	Daily for 5 days	85.7	....
48.7	43.6	....	61.9	71.4
....	....	....	53.5	50

Table IV, Subject IV. (1) *Embryology.* As has been stated, embryology was entirely new subject-matter; the names and terminology were difficult and the sections, therefore, short. This was the same material used by Subject III. An introspection at the time of testing material read three times in one day and twice the second day is as follows. "I had a perfect picture of all of this at the time of reading. It has completely faded. This was one assignment I felt sure of, but the test shows almost a complete loss of material." After six weeks the questions were answered and 40% was retained, showing that the subject was justified in her feeling of surety.

(2) *History.* I shall quote this subject's introspections in detail, since they give clearly her method of learning and her dependence, for recall, upon a definite image. "In learning the material, I tried to visualize the whole, that is, to put it into an outline form and build up relations between the parts so that one would naturally call up what followed. I also pictured out very accurately every detail. In the recall I found that these two things helped me. It was only after I could not get a mental picture of the details that I could not remember. In recall, I found I was more or less dependent upon my outline. Just as soon as the main facts came to me, the rest fell in order. It seemed to be a continuous process without effort. In many

cases I could nearly get the facts. I could feel them on the fringe and yet they were inhibited. I could see just how they stood on the page, but just what they were I could not get. In several places there was confusion; several things came back, but I did not know their relation to each other or to the whole. I felt that if I could get the name I could fill in the detail. In a few cases whole areas were blank. I could not get a visual picture of the page or fact. The whole mass had fallen away. I knew something was gone, but just what or where I could not tell."

Summary, Subject IV. (1) *Embryology.* (*a*) Distributed repetitions proved more effective than accumulated repetitions, without exception, particularly in the two weeks tests. One repetition each day and one repetition on alternate days were most advantageous. There was little difference in the amount retained when the repetitions were divided in various ways, as three times in one day and twice the second day, or four times in one day and once the second day. Likewise, in the ten and seven day tests, daily repetitions were more valuable than those which were made all in one day or two or three a day. (*b*) For immediate recall, cumulative repetitions were as valuable as distributed. (*c*) Repetitions too far apart, as one repetition every fifth day, as well as those too close together, as five in one day, were of little value. (*d*) The questions brought back more than free recall. (*e*) There was a decrease with time in the amount retained.

(2) *History.* (*a*) Distributed and accumulated repetitions proved to be about equally valuable, for both long and short intervals. This is an exception to the rule which has seemed to hold up to this point. The embryology experiments of Subject IV showed, without exception, the advantage of distributed repetitions for recall after long intervals. One possible explanation for the equal effectiveness of accumulated repetitions in this subject-matter might be that the subject had an unusual ability in remembering historical material. She seemed to be able to reproduce it no matter how it had been read. (*b*) For immediate testing, scattered and massed repetitions were of equal value. (*c*) The questions brought back more than free recall. (*d*) There was a decrease with time in the amount retained.

One review test may be noted, in passing, by Subjects III and IV. They were given on February 26th material they had read the latter part of October and the first of November, and on which they had been tested at that time. The conditions of the review experiment were that they were to reread the original text and test in two hours. The four assignments which were retested in this way showed that, after several months, the subject-matter first learned with repetitions distributed within the limits of one repetition every fifth day showed a higher percentage of material retained.

TABLE IV

SUBJECT IV

History			Embryology	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
23	42.3	5 times in 1 day	0	27.7
32.5	46.3	Daily for 5 days	7.1	64
45.3	49.4	Daily for 5 days	60	66
23.2	61.3	Alternate days for 5 days	43.4	52.3
33.3	69.8	3 times 1 day, twice 2nd	25	40
16.2	50	Twice 1 day, 3 times 2nd	0	5
....	....	Once every 5th day	0	30
....	....	4 times 1 day, once 2nd day	24.2	24.2
		Tested in 10 Days		
52.6	59.4	Daily for 5 days	36.8	88.4
34.3	62.5	Twice 1 day, 3 times 2nd	19	14.5
		Tested in 7 Days		
59	49	Daily for 5 days	52.5	84.2
54	25.5	Daily for 5 days	52.3	66.6
54	25.5	Daily for 5 days	52.3	66.6
....	....	5 times in 1 day	28.5	45.6
		Tested Following Day		
56.9	62.8	5 times in 1 day	61.1	88.8
82.6	64.2	Daily for 5 days	20	16
52.8	64.3	3 times 1 day, twice 2nd	....	....
		Tested in 2 Hours		
63.4	65.3	Daily for 5 days	78.5	78.5

Table V, Subject V. My own experiments fall into two well-defined groups. The first group consists of tests made in fourteen, ten, seven, four, and two days, and two hours, with the five repetitions divided differently, as three times one day, two times the next, or four times one day and once the following day, etc.

The results of the other subjects, as well as my own, seemed to show the advantage of divided repetitions over longer periods of time. Believing this point to be worthy of further investigation, I undertook the second portion of this experiment. A series of 60 tests was planned. This series was made up of 10 tests of five times in one day, and 10 tests every day for five days, tested the following day, in two weeks, and one month. I made up the questions for myself many weeks, often several months, before I was ready to use the material. I made the sections to be read shorter and, as has been said, I used various books and pamphlets on economics, thereby making it possible to have many tests under way at the same time. In this way the second series of the 60 tests was carried on more systematically and with less loss of time than the first series.

First Series of the Writer's Experiments

(1) *Economics.* In my introspections I find that, almost without exception, I laid stress upon the necessity of visualization for recall. At first this was more or less unconscious. I did not realize my dependence upon it. One of the comments upon an economics assignment was: "I can visualize where what I don't know is. It is over the page, all of that page, and the top of the next, but I can not get it."

Comment may be made in passing upon one test of material read every day for five days and tested in ten, which gave only 12% in the initial recall. When I made this test, I was ill with tonsilitis. Any state of imperfect physical or mental health or fatigue greatly influences the amount recalled of material which has been learned, and explains departures from the general rule. In this connection there is this introspection; "I simply can't get it at all. This assignment was read five times in succession late at night after a busy day. I know where my last assignment stopped. I am curious to see the questions, for I feel I know some of the material if I could only start, but I have no visualization of any portion of it. I remember exactly where on the page the last assignment stopped, and something of its content, but what follows is an absolute blank." It is interesting to note that the above-mentioned last assignment had been read on five successive days and tested in two weeks. Thus, after several weeks, it was retained better than the later assignment read five times in one day and tested in four.

(2) *History.* The introspections written at the time of reading or of testing the history assignments show the same dependence on visualization for recall. One comment was: "I have lost all visualization here; that is, the continuity is gone, and these isolated facts have returned without any logical connection."

There was one three-weeks test in this group, which gave 3.8% with free recall and later 44% with the questions. The note accompanying the first test was: "I think if I could only start, I could remember it." Evidently this was true, as the questions two days later showed.

After four weeks, material read three times in one day and two times the following day gave very low percentages. Compared with history read in the same way and tested in two weeks, giving 14.6%—25.3% as compared with 9.3%—12.8% in this test, there was but relatively little difference. Little was lost between two weeks and one month.

Several other introspections may be noted which are general in their nature, and summarize in a way the introspections of this portion of the experiment. "After the assignment was read, it was dropped completely out of mind. So completely that often there would be no idea of what the subject-matter was about when the time came to test the material read, for instance, two weeks before. Then, by long continued thinking, the place where the assignment began came into consciousness, that is, on what part of the page. Next, the ideas in the text came, very often many of the same words if the test was an immediate one, but by no means verbatim if the interval had been a long one since the last reading. When the continuity was lost (and this continuity seemed strangely dependent upon the visualization of the subject-matter), that is, when the logical connection was gone, everything seemed to slip away. Isolated facts might come up, but without their connection. There is nothing quite comparable to the feeling of utter blankness, and of strain as well, in attempting to recall an assignment after a long period of time for which the repetitions had been cumulative. There is present a realizathat some of the subject-matter is retained, but what, it is impossible to tell, for it was not forcibly enough imprinted to be revived."

Summary of the Writer's First Series of Experiments

The full series of experiments in both economics and history shows the same general tendency throughout. The following summary will, therefore, be based upon the results of the work done in both subjects. For both economics and history, we may say that:

(1) Distributed repetitions proved more valuable than accumulated repetitions for recall, particularly after seven, ten, and fourteen days. One repetition each day and one on alternate days were the most effective. (2) Accumulated repetitions were as effective as scattered for immediate recall. There was no great difference in the amount recalled when the repetitions were divided differently. (3) The questions brought back more than free recall. (4) There was a decrease with time in the amount retained.

TABLE V

SUBJECT V, FIRST SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
36.5	36.5	Alternate days for 5 repetitions.....	22.2	33.3
14.6	25.3	3 times 1 day, twice 2nd...........	11.7	33.3
0	28.8	Twice 1 day, 3 times 2nd..........	17.4	30
		Tested in 10 Days		
44.8	49	Daily for 5 days..................	12.3	38.4
42.5	....	Daily for 5 days..................	....	....
18	38	3 times 1 day, twice 2nd...........	20	28.5
....	....	3 times 1 day, twice 2nd...........	0	32.4
....	....	Once 1st day, 4 times 2nd..........	28.5	46.4
25	33	5 times in 1 day..................	24.2	36.3

TABLE V—Continued

SUBJECT V, FIRST SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 7 Days		
46.8	59.3	Daily for 5 days	54	25.5
....	....	Daily for 5 days	36.1	44.9
5.7	30.7	Once 1st day, 4 times 2nd	32.2	41.9
34.6	38.4	3 times 1 day, twice 2nd	30.4	30.4
35.6	43.8	5 times in 1 day	20	33
		Tested in 4 Days		
46	52	5 times in 1 day	0	21.6
43.3	52.8	Daily for 5 days	47.3	47.3
51.8	62.5	3 times 1 day, twice 2nd	25.6	42.8
48.2	54.2	Once 1st day, 4 times 2nd	39.5	55.8
58.8	64.7	Once 1st day, 4 times 2nd	....	....
21	40	Twice 1 day, 3 times 2nd	38.4	53.8
		Tested Following Day		
52.8	65.5	3 times 1 day, twice 2nd	67.5	52.5
....	....	3 times 1 day, twice 2nd	40	46.6
47.1	54.5	Once 1st day, four times 2nd	52.5	56
52.1	52.1	Once 1st day, four times 2nd	....	....
....	....	Daily for 3 days, 1 day omitted and 2 succeeding days	41.9	54.4
....	....	Every alternate day for 5 repetitions, 2 days between last 2 readings	46.1	46.1
54	56	4 times 1 day, once 2nd		
58.1	66.6	Alternate days for 5 repetitions	47.5	55
....	....	Alternate days for 5 repetitions	52.9	52.9
58.8	68.8	Twice 1st, twice 2nd, once 3rd	....	....
		Tested in 2 Hours		
76.7	76.7	5 times in 1 day	67.7	67.7
70.3	79.2	Daily for 5 days	56.9	54.9
64.5	56.1	3 times 1 day, twice 2nd	65.7	60
		Tested in 3 Weeks		
3.8	44.2	Twice 1 day, 3 times 2nd	....	....
		Tested in 4 Weeks		
9.3	12.8	3 times 1 day, twice 2nd	....	....

Second Series of the Writer's Experiments

The particular noteworthy point which had been developed thus far was the greater value of distributed repetitions in learning, when the subject-matter was to be tested after a long interval. The following experiments were performed in an attempt to develop this hypothesis into a theory. A series of ten experiments each, of five repetitions in one day and five repetitions, one each day, tested on the following day, in two weeks, and one month was performed, making a total of 60 tests (or 120 in all, including the answers to the questions).

The results show a marked degree of consistency. All the figures which I shall use to compare one series with another will be averages of each series of ten experiments. For example, 11.49%—22.41% means that the average amount recalled for the ten tests, five in history and five in economics, for free recall was 11.49%, and with the aid of the questions, 22.41%.

(1) The first comparison of averages in this series will be that of material read five times in one day, and of material read each day for five days, both tested in one month.

Read	Tested	Average (10 tests)	Mean Variation
5 times in 1 day	1 month	11.49—22.41	4.8 — 6.48
Daily for 5 days	1 month	30.59—41.66	9.41—10.84

That is, almost three times as much was recalled after the end of a month, when the repetitions were divided. Almost twice as much was recalled with the aid of the questions when the repetitions were divided.

(2) The second comparison is as follows. The time of testing in this case was two weeks after the last reading.

Read	Tested	Average (10 tests)	Mean Variation
5 times in one day	2 weeks	13.13—30.2	10.21—10.57
Daily for 5 days	2 weeks	37.26—49.09	11.24—11.95

The relation is much the same as in the tests of one month; almost three times as much was retained when the repetitions were scattered. There is not so much difference in the amount retained with the questions as in the preceding case, although appreciably more was recalled with the questions when the repetitions had been scattered.

(3) The third comparison of this series is of the averages of subject matter tested the day after reading.

Read	Tested	Average (10 tests)	Mean Variation
5 times in 1 day	Next day	66 —69.5	10.62— 8.87
Daily for 5 days	Next day	64.4—70.6	10.57—10.19

For immediate recall, cumulative repetitions proved as effective as distributed. All of the subjects in this experiment have shown the same result, indicating the tendency for accumulated repetitions to be as effective as those which are scattered. There are always many factors which might influence a single test, and make variable the amount retained. This series is significant in that it represents averages, and therefore confirms the point that repetitions which follow one another closely are as valuable as those which are distributed, if material is to be utilized at once.

The introspections of this series have the same tenor as those of the former series and of the other subjects of the experiment. I shall quote only a few. Of the history assignments tested in one month we find: "I can see where the assignment is on the page even to the paragraphs; I know that if I can ever start, some facts will come back to me. I have no idea as to the words the author used; everything is general. I know just where a long succession of facts in the text is; if I only had the right cue to set me off, I am sure I could reproduce some of them. But it is all exasperatingly faint." I quote this because it is typical of many of the introspections written at the time of testing assignments read a month before. Again, "I have entirely lost all the wording of this; the sentences were rather long as I recall them, and only a few of the main ideas come, clothed in my own words."

Another comment was: "I have a general idea of the whole, but the several Russian names throughout the portion confused me. While learning it, I remember thinking, 'Now if I can't recall those names, I won't recall any of it, and I am afraid I can't recall them.'" It seems to have worked out so.

A general observation of the material read five times in one day and tested in one month is: "Invariably there is a decided vagueness, and if anything is recalled it is only in the most general terms. I have a visual picture of the whole assignment, and although I can see where it begins and ends, I have but little idea of the content."

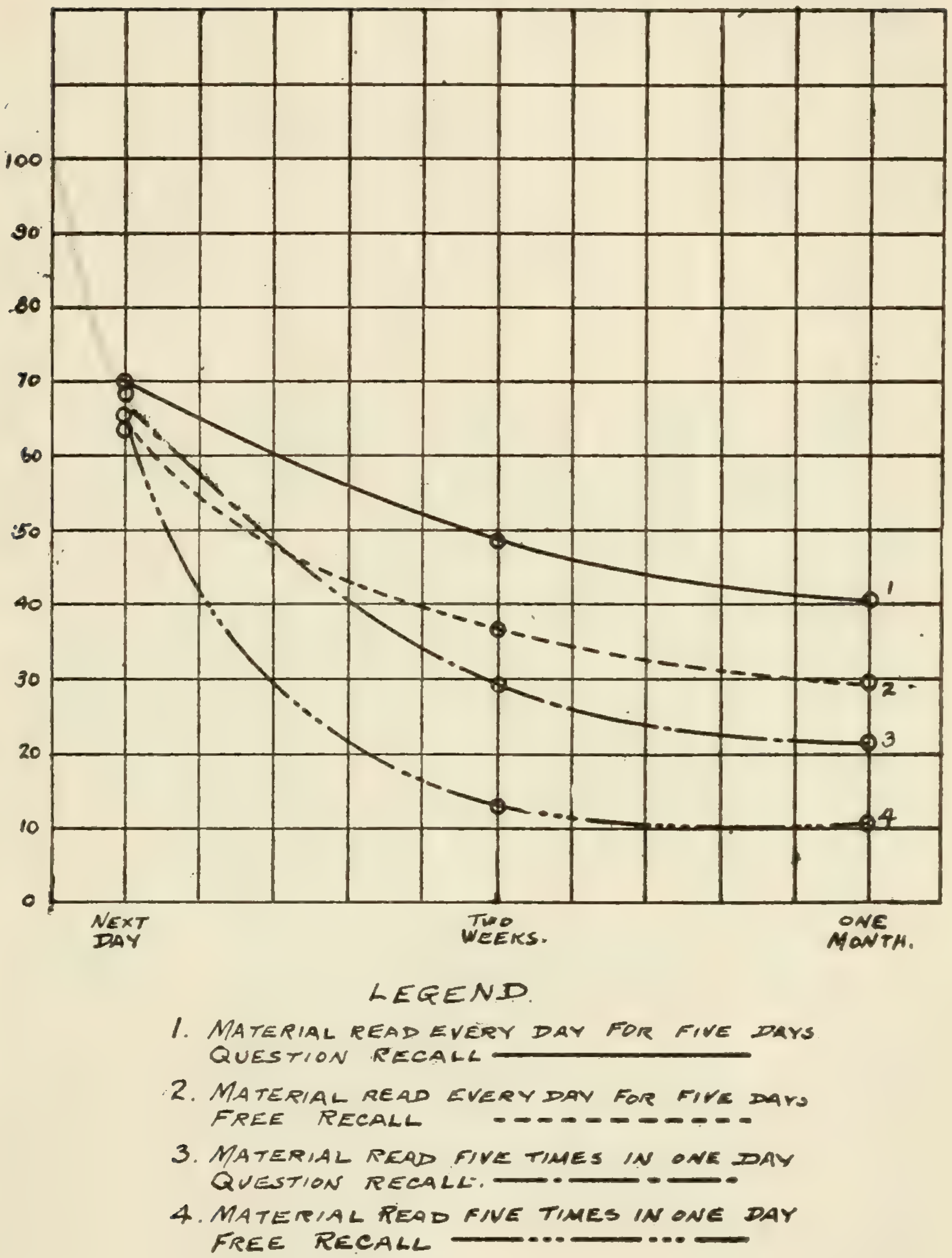
Similarly, of a test five times in one day and tested in two weeks, we find: "I can see this whole paragraph but I know absolutely nothing in it." Again, "I know that this assignment began on the right-hand side of the page about the middle, but I simply cannot recall anything." Very careful and full introspections were taken, but it is sufficient to quote these which I have chosen as typical of them all.

I do not know how I knew what the general idea was when I could not recall it. It is significant that I had to have, first of all, the visual picture of the appearance of the page for any recall; then came the feeling that I knew something of the subject-matter in the text. Yet I could not bridge the gap between my concrete image of the printed page, which I had once known, and the general idea of its content.

The Curve of Forgetting

The writer's second series of experiments contains data sufficient to show the influence of time upon the amount of material retained.

CURVE I.


100
90
80
70
60
50
40
30
20
10
0
NEXT DAY
TWO WEEKS.
ONE MONTH.
1
2
3
4
LEGEND.
1. MATERIAL READ EVERY DAY FOR FIVE DAYS QUESTION RECALL ————
2. MATERIAL READ EVERY DAY FOR FIVE DAYS FREE RECALL - - - - - - - - - - -
3. MATERIAL READ FIVE TIMES IN ONE DAY QUESTION RECALL. ——·——·——·
4. MATERIAL READ FIVE TIMES IN ONE DAY FREE RECALL ——···——···——

Subject Matter Read Five Times in One Day

Tested	Free Recall		Question Recall	
Following day.........	66 %	retained	69. 5%	retained
In 2 weeks............	13. 3%	"	30.12%	"
In 1 month............	11.49%	"	22.41%	"

[illegible]

Subject Matter Read ~~Five Times in One Day~~

Tested	Free Recall		Question Recall	
Following day.........	64.41%	retained	70.59%	retained
In 2 weeks............	37.26%	"	49.69%	"
In 1 month............	30 %	"	41.66%	"

The accompanying curve shows this result graphically. It conforms with the work done on the Curve of Forgetting. Forgetting occurred rapidly at first, and then relatively little was forgotten in the period from two weeks to one month. Forgetting occurred much more rapidly when the material had been learned with the repetitions massed, for the first two weeks, but after that the loss was slight. There was a much greater drop in the curve when the repetitions were accumulated. No matter in what way the material was learned, the loss in the first two weeks was very rapid. It is for retention after longer intervals that the value of divided repetitions is particularly noticeable. We may say, then, that for these experiments, Ebbinghaus' Curve of Forgetting applies to sense as well as to nonsense material.

Class Experiments.—The writer made some experiments on a class in Introductory Psychology. The results of four written lessons show the same general tendency we have found throughout; namely, that, for immediate recall, readings following one another closely are as effective as readings which are scattered. The method used was this. The instructor assigned a certain number of pages in the text-book in Elementary Psychology. He asked one half of the class, in preparation for their twenty minute written lesson, to read the assignment three times in succession, the other half to read it on three alternate days. One hundred eighty-five papers written by students who conformed to the instructions were obtained, despite the failure of some of the students to follow directions accurately.

The ideas in the text were scored as has been described, and the average number of ideas that could be reproduced for each group was found.

TABLE V

SUBJECT V, SECOND SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 1 Month		
10.5	26.3	5 times in 1 day..................	18.7	31.2
0	15	5 times in 1 day..................	0	16.6
10.5	15.7	5 times in 1 day..................	11.7	23.5
13.6	27.2	5 times in 1 day..................	9	27.2
21	31.5	5 times in 1 day..................	9.9	9.9
Average for free recall for both subjects 11.49, M. V. 4.8				
Average for question recall for both subjects 22.41, M. V. 6.48				
40.9	45.4	Daily for 5 days..................	41.1	58.8
18.7	31.2	Daily for 5 days..................	26.6	33.3
26.3	31.5	Daily for 5 days..................	20	33
22.2	38.8	Daily for 5 days..................	22.7	31.8
31.2	37.5	Daily for 5 days..................	56.2	75
Average for free recall for both subjects, 30.59, M. V. 9.41.				
Average for question recall for both subjects, 41.66, M. V. 10.84.				
		Tested in 2 Weeks		
9	32.7	5 times in 1 day..................	0	20.5
0	20	5 times in 1 day..................	0	7.1
31.5	42.1	5 times in 1 day..................	24.4	44.4
22.2	33.3	5 times in 1 day..................	26.6	40
0	20	5 times in 1 day..................	17.6	41.1
Average for free recall for both subjects, 13.13, M. V. 10.29.				
Average for question recall for both subjects, 30.22, M. V. 10.57.				
20	32.5	Daily for 5 days..................	40	50
47.3	63.1	Daily for 5 days..................	64.7	64.7
40	66.6	Daily for 5 days..................	50	61.1
25	33.3	Daily for 5 days..................	17.6	23.5
34.7	52.1	Daily for 5 days..................	33.3	50
Average for free recall for both subjects, 37.26, M. V. 11.24.				
Average for question recall for both subjects, 49.69, M. V. 11.95.				
		Tested Following Day		
55.7	55.7	5 times in 1 day..................	54.6	62.5
58.4	66	5 times in 1 day..................	51.3	51.3
90	85	5 times in 1 day..................	76.4	76.4
73.9	73.9	5 times in 1 day..................	60	73.3
72.2	83.3	5 times in 1 day..................	67.7	67.7
Average for free recall for both subjects, 66, M. V. 10.62.				
Average for question recall for both subjects, 69.5, M. V. 8.87.				
53.3	66.6	Daily for 5 days..................	49.3	49.3
49	63.4	Daily for 5 days..................	77.7	83.3
70.5	76.4	Daily for 5 days..................	61.9	66.6
62.5	62.5	Daily for 5 days..................	94.4	100
57.1	64.2	Daily for 5 days..................	68.4	73.6
Average for free recall for both subjects, 64.4, M. V. 10.57.				
Average for question recall for both subjects, 70.6, M. V. 10.19.				

"A" is material read three times in succession.
"B" is material read on three alternate days.

The table showing the results from these tests is as follows:

Quiz	No. of Papers	How Read	Average % Retained
I	17	A	34
	15	B	46
II	25	A	45.7
	24	B	45.6
III	34	A	49
	19	B	52.4
IV	32	A	40.2
	19	B	39.3

These results conform with the general tendency which has been shown in the experiments of the subjects tested at length, namely, that readings which are distributed and readings which are accumulated are equally effective for immediate recall.

Brief General Summary of the Results of All the Subjects of the Experiment

(1) Distributed repetitions were more valuable than those which were accumulated when the subject-matter was to be tested in two weeks or one month.

The experiments of Subject I in Organic Chemistry and Subject II in History, though few in number, followed this rule, and, without exception, the Embryology tests of Subject III. (The Geology experiments of Subject III were too few in number to be significant.) The full series of Embryology experiments of Subject IV followed the same law. An exception is to be found in the History experiments of Subject IV, in which accumulated and distributed repetitions were of equal value. The single experiments in the first series of Subject V showed the advantage of scattered repetitions, yet not so strikingly as the second series. In the latter, without any exception, distributed repetitions proved to be very much more valuable, for periods of two weeks and one month. Since the final percentages of this series are based upon averages, not single experiments, we may attach much more importance to them.

(2) For immediate recall (recall after two or twenty-four hours) accumulated repetitions were as valuable as distributed.

This was found to be true throughout the experiments for all subjects and for the tests made upon the class in Elementary Psychology.

(3) Repetitions too far apart were as ineffective as those which came too closely together. Although there were relatively few of the tests with repetitions distributed to one every fifth day, the results which were obtained showed that repetitions so far apart were almost valueless.

(4) The questions always brought back more than free recall.

(5) There was a decrease with time in the amount retained.

(6) For all subjects, there was a dependence for recall upon visualization of the text.

IV. Discussion of Results and Summary

(1) *Discussion of Results*

(*a*) *Sources of Error.* The sources of error which form an important factor in such an experiment as this should first be noted. Most important of all, perhaps, is the "personal equation," the individual variation in each subject from day to day, in general health, fatigue, mood, interest and lack of interest. This is an element which cannot be controlled experimentally. A passing mood, or a state of fatigue at the time of learning or of testing, will entirely alter the usual result.

Another source of error which cannot be controlled is the lack of perfect uniformity in the difficulty of the material to be learned. The same books and pamphlets were used throughout the experiment, but as in all subject-matter some pages were easier than others. They had to be taken in succession, otherwise the logical connection would have been lost. Thus if a few pages happened to be easier to remember and the repetitions to be distributed advantageously, an unusually high per cent was retained. This lack of absolute uniformity in the subject-matter was a constant factor, and can only be eliminated by averaging a number of results.

It may be said that knowing the time the test was to be made might introduce a source of error. Whether it was to be made in two hours or in two weeks might, unconsciously, influence the amount of effort expended in learning. Under ideal conditions of experiment, the subject would not know when he was to be tested. Practically, however, it was necessary to map out the work in advance, and the subject was given explicit directions when to read and when to test, for a short series of experiments at a time. In my own later experiments, when I had several tests overlapping, I paid but very little attention, if any, to the time of testing, while I was reading the assignment.

Another criticism, and perhaps a just one, is that I scored the text, test, and made out the questions on my own material. This may appear to have given me an unfair advantage in having seen the subject-matter before beginning to learn it. I do not think, however, that this was the case. The questions on the subject-matter were made out for other subjects as well as for myself, often months before they were to be used. As a rule, I did not even recognize the material when I read it.

Again, I scored all material myself; it was not rechecked by anyone else. I made two checks to see if this criticism was valid, and to see if my method of scoring was consistent. First, I scored again some tests of my first series which had been made a year or more before, and secured practically the same percentages. Secondly, when I began my experiments again, after an interval of over three years, during which time the problem was not touched, not even thought of, I compared the results with those obtained before the work was interrupted, and found a general uniformity. These two checks seem to indicate that the criticism has no vital significance.

(*b*) *Statement of Results and Their Practical Application*

One of the points to be considered first in a discussion of the results is this. All the subjects without exception, some to a far greater degree than others, were dependent upon visualization of the subject-matter for reproduction. There was in no case recall of the logical sense of the subject-matter without some imagery. This is in harmony with Finkenbinder's (5) investigations. He found with 18 subjects using problems of various kinds and of varying degrees of difficulty, as mathematical problems, mechanical puzzles and the like, that the visual image was the first definite figure to appear in 95% of the recalls. In this connection Offner(13) says (in relation to reproduction) that we have the factor of the appearance of the text, having a certain association of words in a spatial scheme.

Since visualization was essential for any reproduction, the results of my experiments agree with those of the investigators who hold that there must be some imaginal factor for recall, as opposed to those observers who hold that imagery is not essential. We can only conjecture as to the dependence upon imagery had some of the subjects for this experiment been of a non-visual type, as the acoustic-motor type, for example.

It is interesting to note, as has been stated before, that there were few errors in the tests which were scored. Gross errors were seldom made. As a rule, if the ideas in the text were retained, they remained in their correct form. In the immediate tests, the author's style and his actual wording were sometimes reproduced. As the time-intervals for testing increased, however, the important ideas remained and became more generalized, the original wording of the author was lost, and more and more details dropped out. This also coincides with the general results of memory investigations in logical material, particularly with those of Henderson. We also found a tendency to generalize.

One of the most important results of this investigation is to show that for immediate recall accumulated repetitions are as valuable as divided. This harmonizes with general observations. It is a common experience for us to learn something for immediate use; say, to be utilized in a few hours or on the following day. School children learn for immediate reproduction; the more quickly the content is reproduced the more nearly accurate it is. Offner(13) asks: "May the greater value of accumulated repetitions for recall be attributed to our habits of learning, or are our habits of learning due to this?"

Accumulated repetitions make material available for immediate use: in common parlance this is really "cramming." An example in a superlative degree is the actor's cramming or "winging" of a part. "A part taken in the morning can be done perfectly at night; every scene was read before going on, the shape of the part was mechanically fixed in memory, that is, the position of each speech on the paper. Words would be acquired but not always the sense, for there would be no time to think about the context . . . . One result of these hurried studies was, words would not remain, fading from the memory as rapidly as acquired."[3]

The same principles apply in a lesser degree to "cramming for examinations." Lessons which were not learned at the time they were assigned can be so intensively studied that, for 12 or 24 hours, the greater part of the work of the semester can be retained. It is a common observation that anything so learned vanishes almost immediately.

The facts in practice, then, are that for immediate use a great mass of material can be made available by intensive review or "cramming," a short time before it is to be utilized. Our experiments upon subject-matter such as is used in practical life, carried on as accurately as possible, showed exactly the same conditions to be true. We may conclude, therefore, that subject-matter which is studied very intensively a short time before it is to be used, both in practical life and in scientific investigation, is soon lost, and very little if any of it can be recalled after a lapse of time.

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[3] H. F. Osborn, Rapid Memorizing, *Psych. Rev.*, ix., 1902, 183 f.

The second important result of this research is the fact that accumulated repetitions, which are highly effective for immediate recall, are ineffective for recall after several weeks. Our experiments show the very great advantage of divided repetitions when learning is to be tested after longer time intervals, particularly two weeks and longer, after the last reading.

While our experiments cover too few periods of time to give a complete picture or to formulate an equation, our results for all of the subjects of the experiment show that forgetting took place rapidly at first, and then much more slowly. Meumann says that whatever we know twelve or more days after learning presumably stands longer at our disposal. On the other hand, what we can reproduce at a glance or at a single reading, we have for only a short time. Similarly in our experiments, there was a rapid loss at first, while if the subject-matter were retained two weeks there was little additional loss at the end of four weeks. Our few results indicate that the curve of forgetting has the same general form for logical as for nonsense material.

The fact that divided repetitions have an advantage over accumulated has been explained in two distinct ways. The first suggestions were based on the assumption that the distributed repetitions had some advantage for learning inherent in the distribution itself, that there was less fatigue or ennui or other disturbance, when few repetitions were made at one time. A second hypothesis is that the distributed repetions offer opportunities for some favorable change in the effects of learning, some change that takes place after the learning gives the distributed repetitions an advantage. Each of these general classes offers several subordinate suggestions. The first suggestion of the first class was made by Müller, to the effect that the fatigue from many repetitions in succession would explain the more favorable results from the distributed repetitions. Under Müller's direction Jost made a series of tests in which a number of repetitions of other series were made before each of the repetitions with distributed learnings that should make the total number equal to the number given to the accumulated repetitions. This insured that the subject should be more fatigued or bored during the divided than during the accumulated repetitions. Nevertheless the advantage of the divided repetitions persisted. Müller seems to have accepted Jost's results as convincing evidence that his suggestion did not furnish the real explanation.

Another explanation in terms of conditions incident to the nature of the individual has been suggested recently for animals. Animals, too, learn a maze much more quickly with divided than with accumulated repetitions. In testing the phenomenon Lashley (10) confirmed the results of earlier investigators objectively, but attributed the fact to the dislike of being taken away from the food, and to general handling. This was an immediate accompaniment of reaching the food-box in accumulated repetitions, but was not so prominent in the divided repetitions. He is particularly vigorous in his preference for this as opposed to any 'setting' process in the nervous system as an explanation of the advantage for the divided repetitions. Whatever may be the status of this explanation for animal learning, there is no corresponding advantage for distributed repetitions in human learning. As Jost's experiments show, fatigue or ennui is not a sufficient explanation. One factor that might be expected to favor accumulated repetitions may be suggested. This is the evidence of making progress in the learning. As several repetitions are made at one sitting one understands, and feels in consequence a confidence in the ability to repeat, that is pleasant as a sign of progress. To be sure one does not actually know that one is learning. The only direct evidence of that is furnished by the test itself, but confidence of that ability increases with understanding. Were any detrimental influence of accumulated repetitions to exist in man, one would expect it to show itself in Jost's experiment mentioned above, in which the divided repetitions were repeated after a number of repetitions had been made of other series to induce the same state of fatigue.

The more usual explanation of the second type is in terms of the perseveration tendency. This must stand or fall with the interpretation of that term. If we assume with Müller that there is a continuance of the activity of the nervous system for some time, as long as forty-eight hours, and that during that time associations constantly increase in strength, we should have all that is essential for our purposes. In this assumption it is necessary to distinguish what might be called the immediate readiness for response from the persisting tendency to return that is connected with association. This may be made clear if we think of dispositions left in the nervous system as consisting of two parts; one, more temporary, which gives the nerve tract affected a tendency to continue active for a relatively short time, and for this activity to become conscious whenever there is no other activity present; the other, more permanent, which serves in some way to connect that tract with another involved when the tract excited with it or immediately after it is aroused. This is the ordinary association. The former tendency dies away relatively quickly; the second persists in some degree for years and diminishes relatively slowly. Since the tracts are active in the same way during the perseveration period as during the original action, it may readily be assumed that the associative process or formation of connection between that and the tracts active during the process accompanying and immediately succeeding will be increasing in strength during the period of partial activity. If we grant this assumption, the greater effect of divided repetitions will be due to the fact that after each repetition the activity will continue for a time. With each repetition this activity will start at a maximum and will diminish to zero. When the same number of repetitions is made in immediate succession, the degree of activity of the tissues involved will not be much greater after the series than after each repetition, and its return to zero will be almost as quick as after each of the divided repetitions. What is gained for the divided repetitions as compared with the accumulated is the setting due to the perseveration for each of the groups of repetitions as compared with the single repetition.

The evidence for the existence of perseveration is, first, the general existence of a continuance of the cortical excitations as seen in the memory after-image. It is fair to assume that there is no sharp cessation of this persisting effect: the only question is how long it continues. Müller and Pilzecker, who first made use of the term perseveration, although the phenomenon had been observed by others before, cited the instances of songs that run in the head, the recurrence of vivid images of exciting positions in a game of chess, etc., which last for hours and occasionally days, as compared with the few seconds that the memory after-image has been demonstrated to continue. They also found that certain syllables shown in one experiment were likely to reappear in another experiment soon after, although there were no syllables shown, or other associations, that would suggest them. Foster, who repeated the experiments with the object of deciding whether these cases were sufficiently frequent to justify the assumption that perseveration was a factor to be reckoned with, came to the conclusion that cases of appearance of old syllables were due to unnoticed associations, or to the fact that they were sounds likely to be made because of their ease or similarity to emotional expressions. It should be noticed that all that he claims is that they might be due to association, not that he was able to trace the associates in each case; and the ease or resemblance to emotional expressions is a matter of opinion. Foster also does not question the persistence of the nervous activity, but only the likelihood that experiences will rise to consciousness soon after their original appearance when not excited through associations. He grants all that is needed for the explanation of our facts.

The indirect evidence is also striking. I mean by indirect evidence in this connection a series of well established facts which can be readily explained if we assume persveration, but which are hard to understand if we deny it. There are three such sets of facts: first, retroactive inhibition; secondly, the rapid forgetting for the first few hours or for two days, followed by a relatively slow decline of the strength of associations, and thirdly the fact we are dealing with, the advantage of divided repetitions. We can understand retroactive inhibition and the related retrograde amnesia of pathological states if we assume that the effects of experiences are at first of relatively little value for the formation of associations that shall lead to their reinstatement, and that, with the passage of time, through the perseveration they become more firmly established. The rapid course of forgetting of meaningless or nonsense material at first might also be explained if there are two parts to the memory process, one due to a quickly disappearing tendency to return, the other to a tendency that diminishes very slowly. The rapid diminution of the likelihood of recall at first would be due to the gradual dying out of the perseveration, while the later part of the curve would represent the more gradual weakening of associative bonds. Finally, we see that the perseverative tendency makes explicable the advantage of divided repetitions. Miss Perkins' results would also give an indication of the length of time the perseveration continues to be strong enough to be detected. Her results show that divisions up to one every third day were more effective than accumulated repetitions, and our present investigation confirms that for sense material. This agrees with the form of the curve of forgetting, which loses its sharp descent at the end of the second day, according to both investigations of the subject. We find, then, an explanation of the three important general facts and of the more detailed time relations, if we assume that retention is partly due to perseveration, partly due to association.

Closely related to this explanation is Jost's suggestion that the determining advantage of the divided repetitions is to be sought in the fact that older associations are more easily brought to full effect by new repetitions, and also that older associations are loosened less quickly than more recently formed ones even if they have the same strength. This law was established before Müller and Pilzecker formulated their doctrine of perseveration, and so takes no account of the latter. It is easy to see that perseveration constitutes an explanation of the greater persistence and more ready revivability of the older associates. If we assume the two parts or phases of the retention process, perseveration and association, the associations would increase in strength during (and because of) the perseveration process. The tests made soon after learning would give a high percentage of successes by the method of successes, because part of the recall would be due to the perseveration. They would not be so quickly learned to the full, because the association process would not have attained the strength that it would have after the perseveration period has passed, and this alone apparently is of value, or at least is of greatest value, in learning to the point where the series can be repeated without promptings.

It is interesting to note that both in these experiments and in Miss Perkins' the distributions could be greater than were advantageous. We found no effect, or very slight effect, from repetitions made one each fifth day. Miss Perkins found that repetitions one each fourth day were less effective than one each third day. This might emphasize the second part of Jost's first law, that old repetitions are more easily brought to full strength than are new of the same strength. Taken with no other implication this might mean that the testing was of little or no value unless the associative trace was refreshed by a new repetition made soon after the perseveration had ceased. This is compatible with the perseveration theory, but one must admit the fact that the effect of the immediate repetition *plus* the setting is lost unless a new repetition is made within a certain time. The time must be before all effect of the first repetition is lost, and to obtain the best effect it must come when we may assume the perseveration of the original repetition is just disappearing.

Ladd-Woodworth(9) suggest that some of the effect of the division may be due to the more rapid forgetting of errors, which serves to leave the correct responses more prominent than they are when the repetitions are accumulated. This would explain the more rapid learning of the mechanical operations of men and animals. It would be less important in rote or logical learning of verbal or symbolic material, in which few errors are made, and where the distributed form of learning is more effective even if each repetition is perfectly accurate. There it must be subordinate to some one of the other explanations.

On the whole it seems that there must be some change in the associations that takes place after each group of repetitions which is more favorable to the divided repetitions. In the light of Jost's law this also favors all older formed associations as compared with the new formed. The most likely source of this improvement in the period after each repetition is the perseveration tendency. The associations increase in strength during the perseveration, and if a new repetition is made before its strength has waned too much the effect of the distributions is beneficial.

We also need to explain the most striking general result of this investigation, that divided repetitions are relatively more favorable after a long period than after a short, after a week or a month than after twenty-four hours. Miss Perkins states that she had some evidence to this effect in her series of nonsense syllables.[1] If we compare her results with those obtained by Jost we find that the difference between the published results of the two is not very great. Jost tested after twenty-four hours, Miss Perkins after two weeks. If we compare his results for 3x8 (eight repetitions on each of three days) with her 2x8, and his 6x4 with her 2x4, and his 2x12 with her 2x8, we find that he has 18, 39, and 53% for one observer and 7, 31, and 55% for another as against an average for all observers for Miss Perkins of 9, 25, and 43%. If we average the results of Jost's two observers there is a slightly greater difference in favor of the more distributed repetitions after the longer period, but it is so slight as to be negligible as compared with the differences we have found for sense material. That the difference should exist does not seem open to explanation from the facts of related phenomena, although it harmonizes with the observations of daily life.

An explanation of the fact of the greater effect of distribution after a considerable period than after a short might be found in the mere average time that elapses between learning and test. For if one repeat once a day for five days and test on the day following the last repetition, the average time between learning and test would be four days as compared with one for the accumulated repetitions. On the other hand, if the test came after two weeks, the average time elapsed would be seventeen days for the distributed as compared with fourteen by the accumulated. The advantages of the distribution might be sufficient to overcome the smaller relative advantage of the long interval and not that of the short interval. There is nothing in the experiments to indicate why the difference should be more apparent with sense than with nonsense material. And the average time between repetition and test would be present for nonsense as well as for sense material. It is evident that other factors are necessary to complete the explanation. Whatever the reason, the increased effect of perseveration or the resulting ' setting ' becomes more apparent after the lapse of considerable time.

(2) *Summary*

(1) Divided repetitions, within limits, prove more effective than cumulative repetitions with logical material as well as with nonsense syllables. The greater value of distributed repetitions is particularly noticeable when material is tested two and four weeks after learning.

(2) For immediate recall, cumulative repetitions prove as effective as repetitions that are distributed.

(3) The forgetting of sense or logical material is rapid at first, then proceeds more slowly, as Ebbinghaus found for nonsense syllables.

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IN AID OF INTROSPECTION

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Introspection is neither an esoteric art which can be practised only by the initiated, nor an instinct placed by Nature in the breasts of all in order that the study of psychology might be possible. It is a scientific method. In spite of the inroads of Behaviorism, most of us still teach that introspecton is a very important psychological method. How many of us accept the implications of our position and *teach* that method? Text books on laboratory psychology give elaborate instructions designed to help the experimenter to the necessary technique, while it seems to be assumed that the much more difficult art of the observer will be "picked up." Now everyone can introspect, just as everyone can observe birds or flowers or any other natural phenomenon. But *scientific* observation implies a technique. This article is an effort to state the outlines of this technique in a form understandable to students. While I shall make no effort to conceal my own psychological articles of faith so far as these have been formulated, I shall attempt to state the method of introspection in a way which will be acceptable to those who do not have the same point of view in general psychology. At the same time, I shall offer a view of introspection which will not be acceptable to many. This view of introspection (as distinct from my general psychological position) cannot be separated from my account of the technique of introspection. One further caution: much that appears here will seem hackneyed or obvious to the professional psychologist. It has not seemed hackneyed to my students, nor am I aware of any account of the technique of introspection addressed to such readers.[1]

Introspection is a natural type of behavior. We all introspect frequently, just as we observe in other ways. For introspection is simply observing our own experiences. When we say "I feel warm," we have introspected. This may or may not be scientific introspection, that is, observation which has complied with certain definite canons which make for accuracy and completeness.

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[1] My obligations to G. E. Mueller and E. B. Titchener will be obvious to all who have followed the controversy aroused by Külpe's pupils. I owe much also to an unpublished laboratory manual by the late J. W. Baird.

The first difficulty is one of terminology. The words of daily life are woefully inadequate to scientific description. The first task of psychology is to refine terminology; the student's first task is to improve his power of distinguishing, classifying, describing, mental facts.

No real progress is possible in this task unless the student actually observes repeatedly the things described. The more you observe, the better you describe. The better you describe, the better you can observe and the more you will observe. This requirement is not peculiar to psychology. Compare the description of a flower made by the naturalist and the novice. Not merely is the first description clearer and more accurate in words; it is incomparably more complete in the details noted. To the beginner in any science, it seems discouragingly as if it were all a matter of "Words, words, words!" It is regrettable that we must spend so much time in the mere refinement of definition, but it is inevitable, not only for clear thought but also for complete observation.

Our next difficulty is that in psychology we do not observe things, but events or processes or happenings, the varying reactions of a mind or self to its environment. The thing you observe will not stand still while you watch. Indeed your watching is apt to make it change more rapidly; though one may add parenthetically that this last statement is by no means so universally true as would be implied from the majority of textbook discussions. The great trouble is not so much that introspection inhibits strong emotion, as that strong emotions tend to inhibit the introspective attitude. But the changing object of psychology is no peculiar difficulty. In many or most sciences, the object observed suffers changes rapid or slow. It is not inherently impossible to observe changing objects; it is merely more difficult. Laboratory devices may cause the object to change more slowly; increasing practice enables one to observe more and more of the swiftly moving panorama. One reason why students are introduced first to sensations rather than to affections is that they are relatively more stable, less swiftly moving. Finally we may take, as it were, moving pictures of the happening; each view is motionless, yet when taken in connection with others it closely simulates the real eventfulness of the experience. We may then proceed to examine a single view or snap-shot, to name the constituent features, to study their pattern or arrangement. We may study the progress of these features from view to view. We must beware, of course, of thinking of mind as essentially thus a sort of kaleidoscope. The kaleidoscope is merely the way in which our minds represent and describe what is essentially a changing process.

It is further a unitary and not-divisible process, but this is not to say it is an unanalyzable one. I quote an illustration from Titchener;[2] a half-trained student reports in an experiment a feeling of "perplexity." Now perplexity is clearly a complex experience. A group of processes is present, some of which we can experience in other contexts, disjoined from each other. True, I have a fair idea of what he has experienced. But only a *fair* idea. The description should be so full and complete that one can imaginatively or sympathetically reconstruct the experience. Obviously, to say that you have "a feeling of perplexity" leaves much to the acuteness of one's imagination. Our first rule, therefore, is: *As far as possible, describe the constituent features of the experience in terms that resist further analysis. Describe in terms of part-processes which cannot be thought of as being themselves made up of smaller or simpler part-processes, or of part-processes found in other contexts.*

Is our task ended when we have completely described the momentary state of our experience in elementary or unanalyzable terms? By no means. Even Titchener, with his insistence upon the necessity of description as complete as possible in elementary terms, laments the current neglect of the durative or temporal aspect of experience. We must study the currents and eddies in the "stream of consciousness." Philosophers are not agreed whether we can directly observe change or can observe only sequence. The result is the same for our immediate purposes of psychological description and explanation. What we want is not merely the complete analysis of the isolated moment, but the movement of our experiences in time.

For psychology is not a purely descriptive science. We are not interested in description for its own sake. Psychology must help us to understand our complex selves, must supply the theoretic knowledge necessary to enable us to control our own behavior and that of others. To this end a description of the experience of a single moment, of a momentary snapshot of our experience, couched in elementary terms, is far more useful than the average student is apt to realize. But it is not enough.

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[2] E. B. Titchener, Description vs. Statement of Meaning, *Am. J. Psychol.*, xxiii, 1912, 167.

In the passage from which we quoted our illustration about perplexity, Titchener says that "the word 'puzzle' or 'perplexity' gives him the key to the observer's predicament, enables him to handle it, . . . but the word tells him nothing whatsoever of the observer's individual experience, of the particular 'feels' that constituted the perplexity in the particular case." Agreed! And we have just been arguing that the "particular feels" should be reported whenever possible. And yet if Titchener is right in saying that such an appellation as "perplexity" gives us "the key to the observer's predicament," do we greatly need the detailed analytic description? We do not want this description for its own sake, but precisely because it does give us "the import of the situation . . . enabling one to handle it." My criticism is that Titchener is too generous to the sort of report he is criticising. Such descriptive appellation is too crude a "key to the predicament" Analytic description would in most cases be much better. Yet just in so far as descriptive appellation does give us the "import of the situation" and enable us to handle it, it is very welcome.[3] I therefore add a second rule: *In addition to analytic description, experiences which are rapidly changing should be characterized or communicated by descriptive appellations, laying stress upon the sequences and order of the part-processes.*

I think it is because this kind of observation has been—not omitted, for it abounds in every piece of introspection, but rather—scorned and rejected of psychologists, that we have the weakness of experimental psychology on the dynamic side, on the side that considers time as a real factor in mental life. An example will show my meaning. In a certain investigation of mine, one of the subjects was thoroughly impregnated with the doctrine that introspection must be exclusively in terms of elementary experiences. Page after page of very excellent introspective analysis of his consciousness did he give me. But the whole thing was somehow dead; it did not *move*. In his desire to get all the processes analyzed into their elements, he was precluded by a sort of negative abstraction from attention to and report on the growth and movement of his experience. Only occasionally a statement about sequences would break through as it were surreptitiously. Analytic description is long and tedious. Be it never forgotten that it is invaluable; but a descriptive appellation can often give one a whole experience or some important part of it in a nutshell. And because it is so much briefer, it enables us so to observe the sequences that in the end we may get just that clue to the observer's predicament which is the common aim of both analytic and appellative description.

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[3] It is possible that I have misinterpreted Titchener's meaning. The "predicament" of which he speaks may be wholly a laboratory predicament, the "key" merely a laboratory key. In any case I have to thank him for expressions so appropriate to the advantages of descriptive appellation.

The danger of appellation is that we are tempted to infer analytic description from it. This we cannot safely do. These are parallel methods for arriving at explanation. Only where an appellation has been agreed upon as representing a certain analysis already made, may one justifiably draw any conclusions about "the particular feels in the particular case."

A further danger of characterization or appellation is the likelihood that it will be confused with interpretation. In my opinion, this is an overrated danger, but it exists. It is clearly illegitimate in your report of an experience to add one jot or tittle more than was in that experience. If, then, in your report, you add your interpretation of your experience, you have strayed from the truth. An example will suffice to show the nature of the error. In reporting the experiences incident upon rearranging a number of letters to make a word, an observer reported that "I shied off from that mass of consonants at the beginning, which could not possibly make a word." At best this is ambiguous. Did *O* mean that he framed more or less definitely the idea that such a mass of consonants could not make a word and that attention then shifted voluntarily to the end of the group? Cross-questioning brought out the true state of affairs: there was an easy non-voluntary shift of attention, following a slightly unpleasant perception of the many consonants. The whole clause "which could not possibly make a word" was an explanation after the fact. However true as explanation, such an interpretation is profoundly misleading if presented as a part of the consciousness of the given moment. Rule 3 therefore runs: *Include interpretation sparingly and always label it carefully as such.*[4]

As long ago as the 17th Century, Descartes called attention to the errors which slip into introspection, itself highly reliable, because of a persistent tendency to shift unconsciously from attention to experiences into attention to external objects.[5] As Titchener says, the error is both insidious and persistent. Introspection being defined as observation of one's individual experiences, everyone admits the impropriety of attempting to estimate the stimulus, which is an external thing. If introspection were to attempt judgments about the stimulus, its inadequacy would justify all the strictures that have been heaped upon it. How pitifully this pseudo-introspection falls short of accuracy, for example, in estimating the distance apart of two points of the esthesiometer! But that is not its proper task. What has introspection to do with physical measurements? Its task is to report on consciousness. In strict accuracy one ought to say, not "one point" or "two points," but "one pressure" or "two." No harm is done if it be clearly understood that "one point" as a report shall mean that the observer feels one clearly defined pressure. To insist otherwise would be to split hairs. Yet be sure you observe your convention. To slip over gradually into reporting "two points" whenever you *judge* that there were two stimuli leads to confusion. For such a report is not an unambiguous statement about your consciousness. You may have been aware of one pressure too large (so you judge) to have proceeded from one point. Yet according to your agreement with the experimenter, "two points" was to mean two distinct pressures; and this was not the case. On the face of the matter, the distinction seems trifling, but strict truth is never trifling. As a matter of fact, the failure to observe the distinction in this very experiment led to some very significant errors, and careful discrimination to their explanation. Rule 4: *Avoiding the "stimulus error," make no attempt to estimate the stimulus; confine your report to your consciousness, to your experiences. Nothing else is introspection; it is merely physical observation under difficulties.*

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[4] The above account of interpretation differs materially from Titchener's. I confess I do not understand what Titchener and his pupils mean by the term, but they clearly include what I have illustrated above and more.

Rule 5: *Ordinarily describe experiences in their temporal order. But sacrifice this if necessary to catch some fleeting and elusive experience.* The advantages of this rule from the mere standpoint of convenience are too obvious to need comment. Its observance will also assist towards completeness.

Rule 6: *The experience or part of an experience selected for observation should not be too long, only a few seconds at the most.*

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[5] In his Meditations. See especially Meditation II, Everyman Ed., *Discourse on Method, etc.*, 90.

This rule requires one or two comments. It is impossible for even the most practised observer to remember with sufficient accuracy and completeness the very complex happenings of a longer period than a few seconds. But many of the experiences which we desire to have described and explained take more time to their occurrence. If we stop the experience in mid course and commence to report on the part that has just taken place, we have no adequate guarantee that the experience would have gone on to the end that we suppose. If we allow the experience to terminate naturally, the observer is apt to forget the temporal course, to omit elements, and even to insert factors that were not present. One partial solution is to emphasize descriptive appellation, but as we have seen, this is often far from adequate. No single solution is possible. The writer believes that actual interruption of an experience should be resorted to with great caution. In all such cases, the experience should *previously* have been allowed to run its natural course. In most cases a satisfactory compromise may be reached by selecting certain portions of the experience for particular attention and report, while relatively neglecting the rest. If this be repeated often enough to allow of particular attention to each temporal part of the experience, an accurate picture of the whole can be obtained.

Rule 7: *Avoid "putative recollection."* This is a very common error among beginners and is frequently committed by those who should know better. It is the sort of alleged observation which begins with "Well, I guess I must have had" a visual image, or what not. Logical inference has its place in psychology. Less primary than introspection, it is possible that it is more important. But logical inference has no place in observation. It is fatal to observation. Of course guessing is much easier than observing, but science is not to be founded on such a basis. It is no disgrace to reply to the experimenter's question with "I don't know." But nothing so openly betrays your ignorance and inexperience as the attempt to conceal it by guessing.

So much for a few hints on the "How?" of introspection. But the "What?" has been treated as if it were entirely obvious. Of course this is far from being the case. It is likely that the student will be first set to observing experiences predominantly sensational. What are you to say about these experiences? First, you must be prepared to make a comparative statement about their attributes. Secondly, you must be prepared to state the patterns of combination into which the elementary experiences enter. Now psychologists are not agreed as to just what attributes we can assign to the various kinds of sensations. As observers, therefore, we must keep an open mind. We must have in mind all the attributes which any competent person has alleged to belong to the sensation and see if we can find them. There are seven candidates which are commonly urged for the office of sensational attributes. These are quality, intensity, extensity, durance, local sign or order, temporal order, clearness. Of these the first two are obvious in every sensation. No one questions that every sensation has some particular quality. Your task in observing a relatively simple experience is to *identify* the several qualities which compose it. Intensity is another attribute which is obvious to our untutored observation.[6] Here our problem is more difficult than with quality. Intensity is an attribute which carries from just more than zero to a maximum, while quality is strictly speaking invariable.[7] Hence we must make some sort of comparative statement about the intensity; we must measure it in terms of some scale of intensities. This was once deemed strictly impossible, though we all make such reports whenever we compare the loudness of two sounds. The possibility of accuracy in such determinations is another matter and one which depends upon the development of experimental methods, among others upon the avoidance by the *O* of the stimulus error referred to above.

As implied above, quality and intensity are very simple attributes and are open to very unpractised introspection. This is not always the case with the others. Thus local sign, while easily identified in the field of touch, and without great difficulty in vision, as the not-further-analyzable factor which differentiates one touch from another, or one tiny speck from another precisely like it imaged on another part of the retina, —while easily identified in these cases, is with great difficulty to be found in others, if at all. For if it is found in other senses, it is in Watt's phrase " wrapt up in the complications and modifications which experiences produce upon one another." To discover an attribute thus wrapt up, familiarity with that attribute in other senses where it is more obvious is implied, and may even then be very difficult. Not infrequently we are obliged to state the derived, complex attribute which we find in the experience (such as its localization, which is derived, it is now commonly believed, from local sign and extensity in combination). Nevertheless we must in each case question our sensational experience for the presence of each of the seven attributes given above. Extensity or volume, the primitive "bigness" of a sensation, and durance or the primitive "lastingness" are, like intensity, attributes which vary from just more than zero to a maximum (whether within the range of one sensation or not appears to be an open question) and hence our problem is to measure them. In actual practice we are accustomed to measure the derived attributes based on these, extension and duration, and students sometimes find some little difficulty in limiting themselves to the simple attributes.[8] Local sign or order, and temporal order resemble qualities in that they do not vary from zero to a maximum. Yet these attributes, unlike quality, are quantitative, since they can be summed. Temporal order and durance are said by many psychologists to be derived, dependent upon the presence in consciousness of more than one sensation, and their uniform and necessary presence is denied. We may therefore question our experience as to whether these characteristics are present and if present as to whether they are simple.

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[6] Compare the illuminating discussion by H. J. Watt in the Introduction to *The Psychology of Sound.*

[7] This is to say, when the quality changes, we have a different sensation. This makes quality in some sense fundamental to the other attributes, and so it is, as men have recognized in naming sensations from the qualities.

Clearness is a characteristic of sensations whose systematic position is doubtful. These doubts need not concern us as observers. Every sensation has some degree of clearness and as introspectors it is our duty to determine how great this is.

There is yet another characteristic of many sensations, which is denied an attributive rank by most systematizers, but which should be reported on by the introspector in much the same way as the attributes. I refer to the hedonic tone or the amount of pleasantness or unpleasantness of the sensation. This point need not be labored, for it is the writer's experience that unpractised introspectors devote most of their energy to report on this factor.

We have then eight points on which to question our sensory experiences: quality, intensity, extensity, durance, local sign or order, temporal order, clearness, and feeling tone. At times, as was said above, the student will be unable to find these lying open to introspection, but will be obliged to report the presence of such derived attributes as show their presence indirectly. The two orders in particular are almost invariably found as spatial and temporal localization, derived attributes depending upon the co-operation of two or more experiences.

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[8] The student will probably find most help in establishing the distinction in W. James, *Principles of Psychology,* II, 1890, 135.

However satisfactory this may be for the systematizing psychologist in proving the presence of the primitive attribute, it is unsatisfactory from the standpoint of the second duty laid upon the observer of sensational experiences. Not only must we report upon the attributes of the sensations present in a given experience, but we must report the pattern or scheme of combination of these sensations. We want to know precisely what primitive attributes go to make up these more complex attributes. What besides extensity does enable us to localize? What aspects of an experience take a dominating place in consciousness? "A recent writer has declared that the 'texture' of qualitative perception, due to the 'massing' of its sensory elements,—it is difficult to find words to indicate precisely what is meant,—may, in certain spheres, be as important in creating apparent qualitative differences as is the quality of the single sensation."[9] Unfortunately little can be said to help the student in his effort to describe the pattern of these complexes beyond a renewed insistence upon the value of analysis, not as the end but as the beginning of this process.

It is time to give the student some encouragement. The number of things that seem to be required of him are many and the difficulties not slight. Were it not for one factor, the accomplishment of our task would be almost impossible. The intention to introspect, to attend to one's experience, materially facilitates its own task. Our powers of observing are heightened by direction of attention, especially with increasing practice and familiarity with the phenomena of the general kind to be observed. But in addition to heightening our power of observation, we get a sort of placing or classifying of the new experience in terms of familiar psychological rubrics or concepts. Sometimes the experience will be explicitly placed by means of a sentence in internal speech, sometimes there will be only a fleeting verbal gloss by means of which the new experience is assimilated to old and familiar classes. Often enough the chief instrument in such apperceptive classification (as it is called) is the name of the group to which the experience belongs. Thus Stricker writes: "When, after my experimental work, I proceed to its description, as a rule I reproduce in the first instance only words which I had already associated with the perception of the various details of the observation whilst the latter was going on. For speech plays in all my observing so important a part that I ordinarily clothe phenomena in words as fast as I observe them."[10]

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[9] E. B. Titchener, *A Text Book of Psychology,* 349, quoting E. Murray, Organic Sensation, in *Am. J. Psychol.,* xx, 1909, 446.

Apperception may take place in non-verbal ways, though perhaps introspective apperception is more effective if in words. Sometimes the experience is apperceived while it is running its course; more often, with the unpractised subject, it takes place immediately afterward.

Closely allied to this is a phenomenon which is probably most familiar in the field of hearing. You say something " back-end-to " and people laugh. Suddenly you realize with startling vividness what you have just said. It is not a peripheral thing like the after-images of vision and touch, which are more properly called after-sensations, yet it is more like these than ordinary memory or fancy images. Now under the influence of the intent to observe, a great proportion of our experience can be preserved in a memory after-image of this sort. Under experimental conditions it is highly reliable and greatly facilitates that completeness of report which would otherwise be so difficult.

It has been assumed in the foregoing that the reader is familiar with the general conditions necessary to any observation; care, accuracy, honesty, etc. The discerning cannot have failed to notice, however, the constant stress laid upon practice and the intention to observe the psychological facts. Indeed this intentional direction of attention, and the implied familiarity with the general nature of the phenomena to be observed, are the secret of valuable introspection. I began by saying that anyone can introspect. I conclude by urging that the value of one's introspection is almost directly proportionate to one's habitude. Anyone with a good memory and a sincere desire to improve can learn to introspect in a way which will be of distinct scientific usefulness.

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[10] Quoted by Wm. James in *Psychology, Briefer Course,* 1892, 309.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

XLIX. An Experimental Study of Cutaneous Imagery

By Catherine Braddock

Very little experimental work has been done upon cutaneous imagery, and that little has no more than a preliminary value. The present study was undertaken to find out (*a*) if cutaneous images of pressure, warmth and cold occur, and in that event (*b*) if the images are free or tied, that is, if they are capable of sustaining by themselves alone the whole meaning of a past perception, or if they appear only in the train of other images (visual, auditory, kinaesthetic) which have proved to be thus capable. The outcome is so surprisingly negative that it, too must be regarded as only provisional. We have some good observers, but only a rough method.

Experiments: Series I

Our object in a first series of experiments was to obtain a direct comparison of sensory with its corresponding imaginal experience. The sensory stimuli were areal (2 by 1 cm.); metal blocks, rounded at the corners, for warmth and cold; wood covered by felt for pressure. We used two markedly different degrees (weak and strong) of the three qualities. For weak cold the metal block was cooled in water of 10° C, for strong cold in ice-water; for weak warmth the stimulus was heated in water of 4° C, and for strong warmth in water of 10° C, above the temperature of the skin. The cutaneous surfaces investigated were those of forearm, upper arm, forehead and cheek. *O* sat with closed eyes, and the stimulus was set down for 1 sec. Chance determined the order in which the six stimuli should be given. The instruction, in the sensory series, ran: "I shall stimulate the skin [of your forearm, etc.]. When the stimulus is removed, describe the cutaneous experience as well as you can." We thought that the reports of this sensory series might help in *O*'s training, as showing him what to look for in the image; we thought also that, if there should be differences between image and sensation, the differences would stand out the more clearly for the immediately previous experience of sensation. We realised that our heavy pressure stimulated deeper-lying organs, and that our temperature stimuli aroused pressure as well as cold and warmth. It seemed best, in a first attempt, to take these complications into the bargain.

At the conclusion of the sensory series on a given surface there was a pause of 5 min., and an imaginal series began. The instruction ran: "I want you now to think of one of the sensory experiences of the last series. When I have named it, realise it as fully as you can, and then describe it. Think of [strong cold, etc.]." Chance again determined the order.

In general it was found unnecessary to present a sensory series for the various surfaces more than once. Three imaginal series were performed for the four surfaces, so that there were 12 series in all. As these continued two pressures, two warm pressures, and two cold pressures there were possible 72 pressure-images, 24 cold-images and 24 warmth-images.

Series II

In a second series we asked *O* to realise familiar situations in which an image of pressure, cold or warmth might be expected to appear. It seemed possible that, under these conditions, images might be aroused with greater ease. The instruction ran: "Realise the following situation imaginally, and describe it as well as you can, paying particular attention to [cutaneous pressure, etc.]. Think of yourself as"—and then a situation would be given. There were ten situations for the three kinds of experience. We used for cold: stepping into a cold bath, having a snowflake fall on the face, holding a snowball in the hand, feeling a key slip down the back, stepping out of a warm house on a frosty day, laying a finger on a cold radiator, taking too large a spoonful of ice cream, waking up at night to find the bedclothes slipped down, having an icepack on the forehead, feeling a dog's nose against the palm. For warmth: bending over an open fire, stepping into a hot bath, putting the hand in the oven, taking a pot off the range, sipping warm cocoa, stirring a pan of boiling sugar, laying the finger on a warm radiator, clasping the hand round an electric bulb, touching a hot-water bottle with the foot, having a pad of cotton wool bound over the back of the hand. For pressure we tried to find situations which should, so far as possible, rule out kinaesthesis: the feeling of a leaf falling on the back of the hand, the feel of fur against the skin, the feel of an insect on the neck, lying on a stone, the touch of a hand on the shoulder, the feel of the wind beating on the face on a gusty day, a hair-shampoo, being brushed down by a porter, receiving a handful of change from a conductor, the flutter of a moth on the arm.

Observers

There were five *O*'s: Dr. L. B. Hoisington (H), instructor in psychology; Dr. H. G. Bishop (B), assistant in psychology; Mrs. A. K. Whitchurch (W) and Miss C. Braddock (Br), graduate students; and X, an untrained *O*. H and B had recently had extended training in the observation of kinaesthetic imagery.

GENERAL RESULTS

(1) We cannot say positively, in the light of our results, whether images of cutaneous pressure, warmth and cold occur. If they exist, their appearance seems at best to be rare; and they are so interwoven with sensory experiences (changes in temperature and tension of the skin, kinaesthesis) and with visual and other images that they are very difficult to detect. The *O*'s differed greatly in their reports. W and B, in the majority of cases, were unable to obtain anything but very doubtful images; X, on the other hand, reported their occurrence readily. H and Br were also doubtful in a large proportion of their reports, but obtained at times what seemed like clear cases of

imagery. There may very well be individual differences. But there is no doubt that, in the experience of realising imaginally the former perception of stimulus or situation, meaning may be mistaken for process. Time and again H (our most practised *O*) would report that a former experience of pressure, warmth or cold had been adequately realised, and yet that he could not say whether an imaginal process in kind, a quality of pressure, warmth or cold, was present in the imaginal complex. H also frequently reported a tendency to say that he had obtained an image; he might even set himself to describe it; and then he would realise that some other image had carried the meaning of the cutaneous experience, and had thus suggested that the required qualitative image must be there. Br reported the same sort of thing.

W's and B's doubtful cases were largely of a different order. Many of them were reported as probably sensory and not imaginal, the sort of pressures and temperatures sensed when one attends keenly to a given part of the skin. Both *O*'s, however, and W in particular, reported cases like those of H and Br.[1]

(2) If cutaneous images occur, there is little doubt that they are tied and not free. Practically all the cases in which images appeared as free are cases in which the *O* reported a doubt whether his cutaneous experience was sensory or imaginal. Those cases, on the other hand, which approached nearest to true imagery (though we cannot vouch certainly even for them) were invariably tied either to visual or to kinaesthetic imagery.

Results in Detail

Pressure.—The reports of the *O*'s differ very greatly. H was more sure of his pressure-images than of the other two kinds. He and X found the images easy to get, and rarely failed to report them. W and B, on the other hand, were in most instances doubtful. Br in Series I regularly reported pressure-images, but became more and more doubtful of them in Series II; deep pressures (kinaesthetic) seemed to be involved. W and B often reported kinaesthesis, which appeared to serve as a substitute for cutaneous pressure.

All of H's reports in Series I contained pressure-images, many of them more than one. The reports began typically with a pressure-image, limited in extent and of brief duration, followed by visual imagery ("bit of lighter grey against a darker grey background") and eye-kinaesthesis, which together served to localise the pressure at the required spot upon the skin. "An experience realised in pressure-terms alone," H remarked at the end of the series, "is a generalised kind of thing, without concrete setting, whereas when the visual image is present it makes the experience definite and particular." The pressure-image usually recurred after the localising and particularising had taken place. Sometimes visual imagery or eye-kinaesthesis or auditory-motor processes might occur before the pressure-image appeared, and sometimes the visual and pressure images alternated several times before H was satisfied that his realisation was adequate.

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[1] It is plain that results like those of G. H. Betts (The Distribution and Functions of Mental Imagery, 1909, 45 f.) must be taken with many grains of salt. Nor is it of any use to improve one's questionary; the fault lies, not with the questionary, but with those who are called upon to answer its questions. X, our untrained *O*, gives consistently positive results (in full agreement with those of Betts), and yet it is certain that, whether or not imaginal processes were present, he was reporting meanings and not processes. We must in fairness add that much water has flowed under the bridges since 1909, and that Betts would hardly write to-day as confidently as he wrote a dozen years ago.

Here, then, there seems to be a clear-cut case of tied pressure-imagery, with visual imagery and eye-kinaesthesis serving as support. We noticed, however, at the beginning of Series II, that H did not (apparently) distinguish between pressure and kinaesthetic imagery; pressure was often reported as "meaning movement."[2] Since the other *O*'s frequently reported kinaesthesis as such, we thought the point worthy of further experiment. We therefore performed short sensory and imaginal series on subcutaneous pressures (etherised skin) and the sensations involved in raising the arm. In the imaginal series the deep pressures became 'surfacy,' bidimensional, though they were not necessarily localised at the surface of the skin. The results appear to show that, at any rate under these rough conditions, there is for H no difference, in process-terms, between subcutaneous and cutaneous pressure.

If this interpretation is correct, H's reports square very well with those of Br. Here the typical report is that of a visual image representing the stimulus, followed by a deep pressure, referred by the *O* to kinaesthesis. In the effort after full realisation of the perceptive experience these images alternated, the pressure coming nearer and nearer to the surface, until finally it appeared as a flash-like process on the skin itself. The visual image of the stimulus was necessary to *O*'s realisation of the corresponding perception. The flash-like pressures were (as has been said) at first accepted as cutaneous; later on the *O* became very doubtful of their validity.

B, on the other hand, reported typically pressures of large area and long duration, which seemed to be sensory in nature, and to resemble the pressures brought out by sustained attention to the skin. It is possible, however, that kinaesthetic imagery was involved; since, after a supplementary series on deep-seated pressures (similar to that taken with H), B remarked that all the pressures of Series I and II had extended a little below the surface of the skin. Kinaesthesis itself was often reported. B was reluctant, throughout the experiment, to commit himself to an imaginal report.[3]

W relied for realisation of her perceptive experiences mainly on visual imagery. She also reported a great deal of kinaesthesis. Her usual 'cutaneous images' were of the same sensory nature as B's. She remarked in both series that if there were no visual image there would certainly be no cutaneous; and in Series II she reported no cutaneous image that was not possibly sensory.

We thus have three possibilities as regards the image of cutaneous pressure.

(1) True images exist, but occur only as bound up with visual or kinaesthetic images. Evidence for this view may be found in the reports of H and X (visual support), and in many of the reports

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[2] In the situation of stepping into a cold bath, movement in the leg and shiver in the back were gross enough to be noticed by *E*, although H reported pressure only.

[3] We have tried to tabulate the reports, but have found the task impossible. The *O*'s 'hedge' and qualify from the start, and grow more and more doubtful as the experiment proceeds.

of Br (kinaesthetic support), and in a few reports of W (visual support).

(2) Deep-pressure images are mistaken for images of cutaneous pressure. This view accords best with Br's results, and in part with those of B. The meanings of skin-surface, localisation, and extension, are carried in visual terms.

(3) Sensory kinaesthesis is mistaken for imaginal cutaneous pressure, and together with visual imagery carries the meaning of skin-surface, localisation, extension. Evidence for this view may be found in the reports of X, W and B; to some extent also in those of Br.

It is possible to read H's results in the sense of (2) and (3), especially since we know that H was set to report qualities, and (as he told us later) avoided the term 'kinaesthetic' owing to its merely functional meaning. As Br and W became increasingly doubtful with practice, and as X's reports may be discounted, the evidence for (1) is very slender. We have no doubt, on the other hand, that (2) and (3) cover part of the facts; the only question is whether they are adequate to the whole.

Warmth and Cold.—All the *O*'s except X were uncertain of these images; every report has some measure of doubt attaching to it. H and W were less sure of cold than of warmth, Br was less sure of warmth than of cold. The reports fall into four more or less distinct groups: (*a*) a deep intense warmth or cold with something pricking or stingy about it; (*b*) a diffuse experience, like a breath of warm or cold air sweeping over a largeish area of the skin (fair intensity, long duration), which runs into sensory warmths or colds elsewhere in the body; (*c*) an experience like a puff of warm or cold air, small in extent, very weak in intensity, short in duration; and (*d*) an experience of pressure, with warmth or cold inherent in it. In the case of warmth there was a variant of (*b*), in which the warmth began in a small area and spread gradually in waves till it merged with the bodily warmth elsewhere.

The experience reported in (*a*) occurs but rarely (H, Br, W). It seems to be kinaesthetic in nature. Experience (*b*) is often reported by all *O*'s, and is typical for B. It seems to be obviously sensory; indeed, the qualification "perhaps sensory" is appended to many of the reports. The variant is also probably sensory, at least in some measure. It occurred typically in X's reports, always bound up with visual or pressure imagery.

There remain (*c*) and (*d*), in which if anywhere we shall find true images of warmth and cold. The experiences of (*c*) were extremely vague and elusive, "just a flash and then gone," so that the *O*'s found description almost impossible. Since the stimuli of Series I combined temperature with pressure, there was greater variety under (*d*): pressure might appear without warmth or cold, pressure might immediately precede or follow warmth or cold, pressure and warmth or cold might occur together, at roughly the same intensity and clearness, or pressure might be predominant, with a tinge of warmth or cold added to it. H and Br report all these modes of experience, though with the recurring doubt of the imaginal character of warmth and cold; W and B were too much in doubt to do more than report a possible temperature as given with the visual image or the pressure.

Visual images were important for all *O*'s except B, and were indispensable to W. In the latter part of Series I, H and Br tended to drop vision for pressure (kinaesthesis?), but the visual images were prominent again in Series II. If temperature images occurred, they were certainly tied, either to vision or to pressure. We did not experiment with radiant heat or cold.

Other Factors.—In both the sensory and the imaginal series H and W frequently reported the kinaesthesis of eye-movement, accompanied or followed by visual imagery. The report was less common with B and Br.

Auditory-motor (verbal) processes played an important part in H's reports. They often appeared at the beginning of his train of imagery, and carried a generalised reference to the previous experience. In the temperature series they might further carry the particular meaning of cold or warmth, and thus render it doubtful whether an imaginal temperature was experienced. They also carried the meaning of confirmation, acceptance or denial of the adequacy of a given process-complex as realisation of the former experience.

B relied largely upon auditory-motor processes; they might so fully mean the past experience that no other processes of any kind were reportable. In Series II B introduced auditory-motor images into the situations, in order by their aid to make his task easier. He reports more adequate realisations in this Series than in Series I, though his cutaneous imagery does not become more assured.

The other *O*'s have little to say of auditory-motor processes.

The Methods.—The method of Series I is a variant of that recommended by J. R. Angell.[4] Preliminary work with blunt points had led to very confused and unsatisfactory results; we therefore enlarged the area of stimulation, in order to give form and substance to the perception and a better foothold to the image. The method of Series II was suggested by the work of A. H. Sullivan on kinaesthetic imagery.[5] We have no doubt, in view of the competence of our four principal *O*'s, that either method would have detected the presence of free cutaneous imagery. None of these *O*'s, in fact, complained of the methods; recall and realisation were feasible enough; the difficulty lay in making sure of the image. Advance thus seems to depend rather on a sharpening of *O*'s *Einstellung* than on improvement of technique.

Conclusions

A conservative reading of our results leads to the following conclusion. Where *O*'s imaginal type tends to be one-sided (B, W), the imagery ordinarily employed comes up so readily and so dominantly that any possible traces of cutaneous imagery are obliterated; the *O*'s report substitutive imagery only. Where the type is generalised (Br, H), cutaneous imagery of pressure, warmth and cold occurs rarely in subordination to free imagery of vision or kinaesthesis.

This is the conclusion that Br and H themselves incline to accept. Both *O*'s doubt the occurrence of cutaneous imagery in the situations of everyday life; both are disposed to think that under the conditions of the experiment they sometimes obtained it.

There is, however, no proof of this position; and there is full proof of the trap set by meaning for the untrained or momentarily unwary observer.

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[4] Report of the Committee of the American Psychological Association on the Standardising of Procedure in Experimental Tests, 1910, 94 f.

[5] This Journal, xxxii., 1921, 54 ff.

L. THE INTEGRATION OF PUNCTIFORM COLD AND PRESSURE

By S. TUNG

The experiments described in this paper were undertaken to discover what psychologically results from the simultaneous stimulation of cold and pressure spots. So far as we are aware no previous attempt has been made to synthetize cold and pressure with punctiform stimulation. Some investigators, however, have said that the areal stimulation of the organs of these two qualities gives the experience of wetness. Helmholtz, for instance, remarks that "das scheinbar einfache Gefühl des Nassen, welches ein berührter Körper erzeugt, . . . aus dem des Glatten und des Kalten zusammengesetzt [ist]," [1] and Bentley has shown that under experimental conditions wetness may in fact result from stimulation with a dry smooth object (such as rubber sheeting) the temperature of which is either above or below that of the skin.[2] Thunberg, on the other hand, writes: "Eine Kälteempfindung, welche von einer Berührungsempfindung nicht begleitet ist, wird häufig als von einer Flüssigkeit verursacht aufgefasst, was daraus erklärt werden kann, dass beim Niedertauchen eines Körperteiles in eine Flüssigkeit der Drucksinn fast gar nicht erregt wird, dass also die entstehenden Temperaturempfindungen ungemischt sind. Im Zusammenhang damit steht eine eigentümliche Illusion von Nässe, welche entsteht wenn man die Stirn mit einem sehr kalten Gegenstande z. B. 20 Sek. berührt. Nach dessen Fortnahme dauert die Kälteempfindung noch einige Zeit fort und zugleich hat man den Eindruck, dass die Stirnhaut deutlich nass sei."[3] Fröbes says, even more emphatically: "Die Qualität 'nass' scheint nichts anders als einfache Kälteempfindung ohne gleichzeitige Berührungsempfindung."[4] The difference of opinion expressed by these authors can, of course, be settled only by experiment, and since we employed a punctiform and not an areal mode of stimulation, we are unable definitely to settle it. We may say, however, that under our conditions wetness (or, more accurately, a 'cold wetness') was found frequently to occur with simultaneous stimulation of pressure and cold spots, and that the experience was never observed when spots adequate only to their normal stimulus were excited singly. We now give a more detailed account of our methods and a more complete statement of our results.

Our first method was to stimulate with a cold-point spots on the surface of the fore-arm which responded both to pressure and to cold. We had planned to check the results thus obtained by stimulating in the same way cold spots that did not respond also to pressure, but we were unable to find spots of this kind because the intensity of pressure furnished by our apparatus was too great. As stimulus we employed a thermaesthesiometer with cold water running through the point. This was attached to a spring balance modeled after Thunberg's algesometer,[5] and the whole was mounted on a universal standard. The amount of pressure, as measured on a pressure balance, was 16 gr., with an mv. of 10% due to the difficulty of controlling adequately the weight of the water conduit. The temperature of the water varied between 18° and 21° C., and that of the room in which we worked between 21° and 26° C.[6] The point of the aesthesiometer was smoothly rounded and measured 1 mm. in diam. The fore-arm was placed in a warm plaster cast. The spots to be stimulated were permanently marked by tattooing. The observers were Professor Weld (W), Dr. L. B. Hoisington (H), Mr. S. Takaki (Ta), and the writer (Tu); Mr. Takaki served as *E* for the latter. The instruction given to the *O*'s was as follows: "I shall stimulate a spot on your arm, and you are asked to report the course, particularly with regard to quality and intensity, of the cutaneous experience."

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[1] H. Helmholtz, *Physiologische Optik,* 1866, 698; 1896, 845.

[2] M. Bentley, The Synthetic Experiment, this JOURNAL, xi, 1900, 414ff. See also E. B. Titchener, *Beginner's Psychology,* 1915, 47.

[3] T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen,* iii, 1905, 708.

[4] J. Fröbes, *Lehrbuch der experimentellen Psychologie,* i, 1917, 134. No authority is cited for this statement, but we suspect that it was taken from Thunberg's article. Thunberg does not mention Bentley's paper.

Results obtained by the First Method. In early experiments the course of the experience was, in general, described by all *O*'s as, first, a pressure of low intensity which quickly passed into a cold; this, after an interval, changed into another cold which then, gradually and with fluctuations, faded out. Later the two qualities of cold were distinguished, the first as a superficial 'wet' cold, the second as 'just cold,' deep and penetrating. Typical characterizations of these colds are as follows:

W. "I felt chill, like a piece of ice touching the skin, which first penetrated deeply then quickly spread out." "Wet was felt but it disappeared rather quickly, then a deep cold that was not in the least wet." "I had first the impression of wetness like a drop of cold water."

H. "At intermediate intensity I felt something like a piece of ice with a strip of cloth surrounding it. Wetness was not sharply felt except at the edge of the spot; it did not last long. After wetness was gone, just cold was left behind." "The wetness was like water; as I felt it less as the cold became more intense." "The wetness was on the surface; the cold beneath the surface of the skin."

Ta. "I felt something smooth wet and cold at the spot like ice melting; . . . after wetness was gone I felt simply cold like the coldness of metal."

The wet cold was reported in about 80% of the trials; in some instances cold did not appear at all; in others it was long delayed, as a result, we suppose, of a relative displacement of the cold-organ and the tattooed spot. It was a logical inference that the change from the wet to the dry cold might be due to the earlier adaptation of the pressure, and we found by rough experiments that the average adaptation time of pressure was in fact only ¼ that of cold. If the above inference is correct, we should be able to obtain a return of the wet cold by restimulation of the spot with a pressure-stimulus after the wet cold has gone but before the second cold has disappeared. We tried this experiment, and in some instances were successful. It was essential to success to employ a relatively weak stimulus (a hair), to apply it as close to the cold stimulus as possible, and to direct *O*'s attention to the cold; if the cold was not focal, a pressure was reported within an area of cold.

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[5] T. Thunberg, *Skand. Archiv.*, xii, 1902, 424.

[6] A lower temperature of the water was avoided because of the production of moisture by condensation on the metal point.

Our second method was that of simultaneous stimulation of discrete pressure and cold spots. For pressure we employed hairs of 3.0 and 4.2 gr/mm tension value; for cold it was essential to our method to excite the cold spot without touching the skin. In order to do this we found by trial that exceptionally low temperatures of stimulus were necessary; and the only one that proved adequate was obtained from a mixture of ether and carbon-dioxide snow.[7] This was managed in the following way: a Willyoung thermaesthesiometer, with thermometer removed, was clamped to a universal standard; it was first filled with the snow, and then the ether was added. When this was done the temperature of the mixture fell quickly to about —78° C., a heavy vapor streamed down perpendicularly from the instrument, and a thick frost accumulated on it. Since the vapor was felt as a 'damp cold,' and falling particles of frost as 'wet cold,' we were forced to wait until the violent stage of evaporation was over; thereupon the stimulus was ready for use. Our subsequent procedure was as follows: the cold stimulus was first lowered and adjusted in such fashion that the point of the instrument was directly over a cold spot. The distance above the surface of the skin varied from 2 mm. to 10 mm., depending upon the state of the mixture or upon the readiness of response of the cold spot. When *O* had reported a cold of good intensity, a signal was given, and a pressure spot in the immediate neighborhood of the cold spot was stimulated by the hair. *O* was instructed to describe the qualitative course of the cold after the signal.

Results obtained by the Second Method. We again frequently, although not invariably, obtained reports of 'wet cold.' At times, however, the pressure was not realised either as pressure or as anything else, and at others pressure was felt in an area of cold. These results seemed to be due either to a lapse of attention to the cold (a condition which our instruction was designed to meet), or else to the lack of a proper proportion of the intensities of cold and pressure; for example, we found that the pressure was most frequently not felt when the intensity of cold was strong, and that it generally appeared in a spatial pattern with cold when the cold was weak. We regret that we were unable, for want of time, to make a systematic study of this point. But it appears probable that the appearance of 'wet cold' is, aside from the mode of stimulation, conditioned both upon the greater clearness of cold and upon a certain relation between the intensities of cold and pressure. When the 'wet cold' occurred it came suddenly, with surprising distinctness, and lasted for a very brief interval. It was characterised as like a "melting flake of snow," "a drop of cold water," "a tiny particle of ice." The 'radiant cold' on the other hand was reported as "not punctiform but areal, less intense, and more penetrating than the 'wet cold;'" "it was like the 'dry cold' of the other experiment," "like the feel of a cool breeze without pressure, or the cold of evaporating ether."

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[7] We tried without success the apparatus of the first method with the point cooled by an ice-salt solution, a temperature cylinder also cooled by an ice-salt solution, a mica shield with small holes in it for receiving the pressure stimulus and for conducting cold from a block of ice, and an ice-ammonium chloride solution in the Willyoung apparatus. The department of physics in Cornell University kindly supplied the carbon-dioxide snow, and gave us the temperature of the mixture.

The fact that 'wet cold' may derive from a simultaneous stimulation of cold and pressure spots is, of course, only a single result. The duration of the experience was, under our conditions, so brief that our *O*'s were baffled in their attempts to give a qualitative description of wetness. But an investigation of the synthesis of pressure and warmth with punctiform stimulation is now being made in the laboratory, and a systematic study of Thunberg's illusion is also under way. These two inquiries should throw further light upon that problem.

LI. THE HERING COLOR-BLINDNESS APPARATUS AND THE NORMAL RAYLEIGH EQUATION

By M. WINFIELD and C. STRONG

In a previous article on the Hering Color-Blindness Apparatus reference was made to experiments then in progress on the normal Rayleigh equation, the matching of spectral red and green to yellow.[1] In this paper we report the results of those experiments. It was our purpose not only to test further the capabilities of the Hering instrument, but also to determine the range and form of distribution of the matches of a large number of *O*'s who have normal color vision. That there is a wide variation among normal *O*'s has, of course, been known since Rayleigh first performed his experiment,[2] but interest has centered upon the extremes of variation (the anomalous trichromats), and the central variations have for the most part been either ignored or else explained away as the result of 'errors of observation.'

Conditions and Procedure of the Experiment. The experiment was performed in a dark room, and the apparatus was illuminated by an artificial light of Gage's glass[3] as described by Cowdrick and Winfield.[4] The horizontal distance from the center of the roundel to the light chamber was 13 cm.; the vertical distance from the top of the table upon which the apparatus stood to the center of the roundel was 27 cm. It was found by test that a variation of from 2 to 12 volts of current below the 110 at our disposal did not affect the results of the experiment. *O*'s eyes were protected from the glare of the lamp by a screen.

In making the equation we place a green glass in the left (to one facing the open side of the instrument), a red glass in the floor, and a yellow glass in the right aperture of the light chamber. The red reflector is set at 20° and the yellow at 15° of arc;[5] the former is not moved throughout the experiment, and the latter is moved only when, after the match in hue has been made, *O* reports a difference in tint. The green reflector which conditions the variable stimulus is set to give R or G as *E* desires. *O* is then instructed to make the equation by turning the knob at his right hand (which controls the green reflector) to the right or to the left, as the case may be, until the match is made. In these experiments ten successive matches were required of every *O*, and the setting of the green reflector for every match was made according to the schema Rl, Gs, Rm, Gl, Gs, Rm, Gm, Rl, Gl, Rs, the capital letters signifying a setting of the reflector that would give R or G, and the small letters long short and medium distances from the approximate degree of arc that would give the match. In order to avoid the effect of local adaptation (which is an important source of variation in a single set of matches) *O* after making a match was required to rest his eyes for a few moments and then to verify, and if necessary to rectify, the equation before the scale-reading was recorded.[6] In all matches *O* used but one, usually the right, eye.

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[1] M. Cowdrick and M. Winfield, The Adjustment of the Hering Color-blindness Apparatus, this JOURNAL, xxx, 1919, 418.

[2] Rayleigh, Experiments on Colour, *Nature*, xxv, 1882, 64; On Defective Colour Vision, *Brit. Assoc. Rep.*, 1890, 728. For a review of subsequent experimentation see J. von Kries, in Nagel's *Handbuch d. Physiologie des Menschen*, 1905, 124ff., and in Helmholtz' *Physiologische Optik*, ii, 1911, 343ff.; J. H. Parsons, *Colour Vision*, 1915, 182ff.

[3] We have recently learned that not all roundels of Gage's glass have, as have those employed in this and former experiments in the Cornell laboratory, a ground surface. At the present time the manufacturers supply the glass as it comes from the press unless a ground surface is specified.

[4] *Op. cit.* They give incorrectly the horizontal distance from roundel to light chamber as 'about 30' cm.

[5] This setting of the red reflector was chosen because it permitted a green of excellent chroma at the one extreme of the variable. The 'mixing value' of R in this apparatus is much greater than that of G; *e.g.*, if the red reflector had been set at 40° of arc all the green

Results. Our results fall, according to the aim of our investigation, into two groups: those which concern the capabilities of the Hering Color-blindness Apparatus, and those which indicate the range and distribution of the matches of normal *O*'s. As regards the first of these, we found early in our experimentation that the apparatus, when employed under the conditions we have described, gives homogeneous results; but that, owing to the mechanical adjustment which controls the movements of the reflector, the units of measurement are too large. The adjustment is so 'coarse' that a change in the position of the reflector by less than a degree of arc, an amount that frequently destroys a match, is not easily made. The effect of this is, of course, to reduce the range of positions that for a single *O* will give a match; for example, 1% of our *O*'s gave the same reading 10 times, 2% 9 times, 19% 6 times, and 32% 5 times of a total of ten trials. This quantitative result is, however, conditioned in part upon the fact that *E* read the scale to the nearest degree, a procedure that, in view of other faults in the apparatus, seemed advisable. The size of the arc that carries the scale is so small that *E* could not read to less than ½°, and the distance of the pointer above the scale is so great that unless *E*'s eye is directly above the pointer the error of reading the scale is at least 1°. In the course of our experiments we reduced the error of observation to less than ½° by a five-fold extension and enlargement of both scale and pointer; but for the sake of uniformity we continued, as we had begun, to read to the nearest degree. In all other respects the instrument was entirely satisfactory for our purpose; it is easily managed, instantly ready for use, and if its position relative to the source of illumination is permanently fixed is always reliable.

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at our disposal would not have cancelled the red. Our experience has shown that a setting of the red reflector at 25° of arc would have given an R that could have been cancelled by all of our *O*'s, and the resultant yellow would have been of lighter tint and better chroma than that which we obtained in this experiment.

[6] In an extended series of observations with a single *O* who adapts slowly in the dark we found that general adaptation upon coming from a light into the dark room did not, after the first three or four minutes, make any sensible variation in the average equations.

As regards the distribution of the matches of normal *O*'s, we give the results of 100 cases. Many more than these were collected; but we have excluded not only those of the preliminary period before we had the apparatus and method of observation under control, but also 10 cases we found to be abnormal in the sense that they were either color-blind or color-weak. When once the final method, which we have described above, was determined upon, we began the collection of the 100 cases, accepting every *O* as he came, and continuing until the hundreth match was obtained. The *O*'s consisted of 36 men and 64 women taken as we found them from the graduate and undergraduate students and from the staff of the department of psychology in Cornell University. We believe that they as a group constitute a fair sample of a university population, and that every individual in the group has normal color-vision. The majority, however, were not tested specifically for color-blindness or color-weakness because indications of abnormality appear in the ordinary course of matching R and G to Y; abnormal *O*'s almost invariably give larger *mv*'s than normal, and they also give an average match that falls at the limits of (or outside) the normal distribution. We, therefore, tested for color-blindness or color-weakness only those individuals whose matches differed markedly from the average, whose *mv*'s were exceptional, and whose color-names in making the match were incorrect; the remainder we assumed to be normal. We give in the following table the distribution of the matches of these normal *O*'s.

TABLE I

DISTRIBUTION OF THE AVERAGE AMOUNTS OF G REQUIRED BY 100 OBSERV ERS TO EQUATE R TO Y

Degrees of Arc.........	45	46	47	48	49	50	51	52
No. of cases...........	1	7	18	23	27	17	6	1

Average = 48.48 Median = 49

The difference between groups represented in the table by apposed degrees of arc is, we believe, highly significant. In the first place, the units of measurement were, as we have seen, too large to admit of minute gradations. Furthermore, the *mv*'s from the averages of every ten matches were small; the average of all these was 0.7°, and their median value was 0.6°; 27% of the *O*'s had an *mv* of 0.5° or less, and only 15% of 1° or more. Finally, if only the conditions were kept constant, we found no greater variation in average matches of a single *O* from day to day. We believe also that, in so far as our experiments go, we have determined the limits of normality; we found no case, normal or abnormal, beyond the limit of 45° (*i.e.*, in the direction of less green); and although nine cases fell beyond 52°, they were all abnormal; only one abnormal, a protanope whose average match was 52°, came within the limits of the normal distribution. If, then, the difference between groups is a real difference, and if we have been successful in finding the limits of normality, the range of distribution becomes significant. The mode is at 49° and the limits are 45° and 52°; there is, therefore a greater range of distribution on the side of less green than on that of more green. In this respect the form of the curve is like one of 59 cases published by Donders, and consequently it may be that this form is typical.[7] But our own curve permits of another interpretation. It will be observed that, with the exception of the extreme groups, the difference in size of groups becomes progressively less as the mode is approached. Furthermore, the average of all matches is 48.48°, and this average falls between the two largest groups which combined equal half the cases. We might regard the curve, therefore, as broad and flat at the mode, and as falling away equally on the two sides. If, as von Kries suggests,[8] this variation among normal *O*'s has a physical cause, *i.e.*, is due to differences in macular pigmentation, there would seem to be no reason why the curve should not approximate to a normal distribution.

We do not wish, however, to overemphasize the form of distribution. The essential feature of our results is that the extent of variation among normal *O*'s has been brought into bold relief. The fact has, of course, long been known to everyone who has observed students, in demonstration of the first law of color mixture, match R and G to grey; but so far as our reading has gone von Kries is alone in realizing that the variation is distinctive enough to mark off the normal from the anomalous groups.

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[7] F. C. Donders, Farbengleichungen, *Arch. f. Physiol.*, 1884, 521.

[8] J. von Kries, in Helmholtz' *Handbuch der physiologische Optik,* ii, 1911, 345.

LII. THE AFTER-EFFECT OF SEEN MOVEMENT WHEN THE WHOLE VISUAL FIELD IS FILLED BY A MOVING STIMULUS

By WELLINGTON A. THALMAN

In his study of the after-effect of seen movement A. Wohlgemuth reports a series of 34 experiments whose results he uses to disprove current theories and to support his own theoretical construction.[1] One of these experiments, the 22d, is devised "to ascertain how the after-effect is affected if the whole visual field is filled by an objective movement." The outcome is negative: there is "a total absence of any after-effect." Wohlgemuth, apparently dissatisfied with this finding, repeats the experiment, with identical result. He accordingly concludes that "no after-effect is produced by an objective movement occupying the whole visual field."[2] Later, in his theoretical sections, he reviews and rejects all current theories "either for *a priori* reasons or on account of incompatibility with experimental evidence," and proceeds, on the basis of McDougall's theory of drainage, to the construction of a theory which shall be "in harmony with the ascertained facts."[3] But in relating this theory to the facts *he fails to consider the results of his own Experiment 22.*[4] Yet these results, if they stand, appear to contradict the theory, according to which the phenomenon is in no way conditioned upon size or spread of the stimulus but is wholly dependent on priority of entry and the consequent decrease of synaptic resistance. If the after-effect fails to appear when the whole field is stimulated, the theory must either be discarded or radically revised.

We hold no brief for the theory; but we were not either convinced by the outcome of the critical experiment, which we therefore decided to repeat. We hoped also, by extending the method, by requiring introspective reports of process as well as statements of meaning, and by improving the apparatus and technique, to contribute something toward a complete phenomenology of the illusion.

Observers.—We had two groups of *O*'s. The members of the first group were Mr. W. A. Andrews (A), and Mr. S. Takaki (T), graduate students in psychology; Mrs. I. G. Whitchurch (W), graduate scholar in psychology; Dr. F. L. Dimmick (Di), assistant in psychology; and Dr. K. M. Dallenbach (Da), instructor in psychology. We gave these *O*'s a preliminary practice-series that should bring them to the pitch of proficiency that Wohlgemuth's *O*'s had attained.[5] They were required to report on both process and meaning. The members of the second group, whom we shall quote only by number, were with two exceptions students in the summer session of 1920. These *O*'s approached the problem without special training, and were asked to report only upon meaning.

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[1] *Brit. Journ. Psych.,* Mon. Suppl. 1, 1911, 1-117.

[2] *Op. cit.,* 72, 113.

[3] *Op. cit.,* 98 f. For McDougall's drainage-theory see *Brain,* xxvi, 1903, 153 ff.

[4] The experiment is referred to by number on p. 106; but the "Experiments 20-22" there mentioned should undoubtedly be "20, 21," since we read immediately after of "these two [not three] cases," and since the separate discussion on p. 109 mentions only Exps. 20 and 21. The slip on p. 106 is unfortunate, as it led the author to leave Exp. 22 out of account.

Preliminary Practice.—We used in the preliminary work a demonstrational model of James' artificial waterfall,[6] with and without a fixation point, in both the vertical and the horizontal positions. The curtain was rotated by hand at two speeds, fast and slow, and the rotation was continued for 5, 10, 15, 20 or 25 sec. There were thus 40 possible experiments; and as every experiment was repeated 5 times, every *O* gave 200 reports upon the illusion. Observation was binocular, at a distance of 25 cm. An adjustable biting-board served to steady *O*'s head and aided him to maintain his fixation.

The results of this practice-series may be summed up as follows. (1) All *O*'s obtained the illusion of backward movement. (2) The after-effect became more pronounced as practice advanced. (3) The after-effect became more pronounced as the exposure grew longer and the rate of rotation faster. (4) The presence or absence of a fixation point made no difference in the illusion. (5) The after-effect was as marked with horizontal as with vertical movement of the stimulus.

Repetition of Wohlgemuth's Observations.—We next set up Wohlgemuth's apparatus and repeated his observations. The apparatus,[7] which consisted of an endless band of printed calico of alternating black and white stripes 5 mm. wide, 180 cm. broad and 110 cm. high, stretched between two rollers, was placed in a dark room and was so illuminated by daylight mazda lamps that no shadows fell upon its surface. The band was driven by an electric motor which, by means of a multiple speed-reducer, moved it downward at a velocity of 12 cm. per sec.[8] The times of stimulation were 30, 45 and 60 sec.[9]

No one of our *O*'s found the entire field of vision filled by the black and white surface, even though he stood so close that his nose actually touched the cloth. Always there was an unfilled margin. It may be that Wohlgemuth's *O*'s failed to report this fact; it may be that Wohlgemuth regarded it as of no consequence. Since, in his second attack upon the problem, he reduced the dimensions of the stimulus-field to a square of 80 cm. side,[10] it is probable that he aimed only at a relative filling of the field of vision. Wohlgemuth, again, used a fixation point throughout his observations.[11] It is clear that this point, with the wire that held it in place, further violates the conditions of complete objective movement. Our practice experiments had shown that the fixation point is unnecessary, and we therefore discarded it in part of our own work.

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[5] On the importance of practice see Wohlgemuth, *op. cit.*, 27, 110.

[6] W. James, *Mind*, O. S. xii, 1887, 517; *Principles of Psych.*, ii, 1890, 245.

[7] *Op. cit.*, 72.

[8] Wohlgemuth does not state his rate of movement. We chose that of 12 cm. per sec. because his experiments on the "influence of the velocity of the stimulating movement upon the after-effect" (Exps. 10-13, pp. 45-52) showed that the optimal rate lay between 6 and 12 cm. per sec. (see Fig. 8, p. 50). These experiments were performed with central vision. Our own experiment involves peripheral vision, and we therefore thought it best to take the upper limit.

[9] We used these long times, although the preliminary practice-series had proved them unnecessary, in order to conform with Wohlgemuth's procedure; see *op. cit.*, 72.

[10] *Ibid.*

We performed three series of experiments with this apparatus. In the first we sought exactly to duplicate his conditions, and therefore introduced a fixation point. A piece of wire carrying a small white square of 5 mm. side was let down from above and ended at the center of the field, about 1 cm. in front of the cloth. In the second and third series the fixation point was removed. In the second, the *O*'s were directed to fixate the surface of the moving field; in the third, they were directed to look through and beyond the cloth, to fixate at 'infinity.'

The following instructions were read to the *O*'s at the beginning of every observation-hour: "Take a position about 10 cm. in front of the center of the screen. When you have done this, close your eyes and wait until the experimenter says 'Ready,' 'Now.' On the 'Now' open your eyes and (*a*) keep your eyes steadily upon the fixation-point; (*b*) fixate the surface of the moving field; (*c*) look through and beyond the screen and fixate at 'infinity.' When the objective movement has ceased and the resulting phenomena have run their course, give a complete account of them."

While the *O*'s had their eyes closed the motor was started, and the screen was allowed to reach its full velocity before the 'Now' was given. At the end of the experiment the screen was instantly stopped; and when the after-effects, if any, had run their course, the *O*'s turned to the description.

Every experiment was repeated twice with every exposure time. The number of times, expressed in percentages, that an after-effect was reported, is shown in Table I.

TABLE I

SHOWING THE PER CENT. OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED WHEN WOHLGEMUTH'S APPARATUS WAS USED

	Observed		
		Without Fixation-Point	
O	With Fixation-Point	Near Fixation	Far Fixation
A	33%	100%	66%
Da	16%	100%	33%
Di	0%	100%	100%
T	66%	100%	100%
W	100%	100%	100%
1	50%	100%	100%
2	50%	33%	100%
3	66%	66%	66%
4	16%	0%	0%
5	0%	0%	0%
6	0%	0%	66%
Average	36.3%	63.6%	66.6%

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[11] *Ibid.*

The after-effect was variously reported as a "slow upward movement," a "rapid upward movement," a "reverse movement," a "rebound," a "jerk upward," a "recoil," a "spring up." The direction of movement in the after-image was always opposite to that of the objective stimulus. The rate, extent, and duration of the movement varied considerably: it was described as rapid, as medium, and as slow; it was local (around the fixation point), general (extending over the entire field), or intermediate between these extremes; it passed through a small angle and was reported as "upward about an inch," or through a large angle and was reported as "upward a foot," or it was intermediate; and it was of short, intermediate or long duration.

The apparatus as employed by Wohlgemuth, with a fixation-point, afforded on the whole the least favorable method for observation of the phenomenon. But even under these unfavorable conditions the *O*'s, on the average, reported its presence in more than one-third of the experiments. The individual differences between *O*'s was large. Of the trained *O*'s, W reported the after-effect in every experiment, wheras Di failed to observe it in a single case; of the untrained *O*'s, 1, 2, and 3 reported an after-effect in at least half of the experiments, whereas 4, 5 and 6 reported it in few or none.

The after-effect was noted in a larger percentage of the cases when the fixation-point was removed. The figures given in Table I show that an after-effect was, on the average, reported by all the *O*'s in about two-thirds of the cases.

There is very little difference in the results with near and with far fixation: an after-effect was reported in 63.6% of the experiments with near, and in 66.6% of those with far fixation. In these experiments the individual differences among the trained *O*'s was very small: with near fixation every *O* reported the phenomenon after every experiment; with far fixation Di, T, and W continued to report the after-effect after every experiment, whereas A and Da failed to observe it in ⅓ and ⅔ of the experiments respectively. The untrained *O*'s show greater individual variation. Some gave descriptions which compare with those of the trained *O*'s, while others, like Wohlgemuth's *O*'s, failed to observe an after-effect.

The effect of practice and training is clearly marked. Those *O*'s who had taken the practice series reported the after-effect more frequently and more consistently than the untrained *O*'s. Two of the untrained *O*'s. 1 and 2, gave reports and percentages which resemble those of the practised group. These *O*'s were both psychologists, and their general training in observation sufficed to differentiate them from the unpractised and untrained group.

In a number of the experiments in which no after-effect was observed the *O*'s stated that the objective stimuli fused as in color-mixing; the white and black lines disappeared, and the background became a uniform grey. Fusion was more frequently reported with fixation, when the eyes were strained and attention was concentrated upon the white square, than without.

We conclude from our repetition of Wohlgemuth's experiments (1) that the apparatus is unsuited to the problem. For (*a*) the entire visual field is not covered by an objectively moving stimulus; (*b*) in order to approximate this condition the *O*'s must stand so near the moving field that the black and white tend to fuse; (*c*) fusion is especially noticeable in experiments in which a fixation point, with the resulting strain of convergence and concentration of attention, is introduced; and (*d*) the presence of this point and of its wire support violate the conditions of complete objective movement; the point of clearest vision and maximal attention is stationary. We find (2) that even under the unfavorable conditions of Wohlgemuth's procedure the after-effect may be observed. The reports vary considerably, but correlate roughly with the training and experience of the *O*'s. Trained *O*'s and experienced *O*'s without special training report a reverse movement in about half of the experiments. We note (3) that if the problem of the after-effect of complete visual movement is to be successfully attacked we must change the apparatus: (*a*) the whole field of vision must be stimulated; (*b*) the fixation point must be eliminated; and (*c*) the distance of *O* from the moving field must be large enough to rule out eye-strain.

Principal Experiment.—The apparatus which we finally constructed consisted of a large cylinder 1.7 m. high and 1.25 m. in diameter.[12] On the inside we glued heavy white architect's paper on which had been drawn in carbon ink at intervals of 7 mm. black lines 7 mm. in width. The cylinder was supported from above by a shaft projecting from a ball-bearing swivel joint.[13] It was rotated by an electric motor at two rates of speed, which we shall hereafter designate as 'fast' and 'slow.' At the fast rate the cylinder moved with a velocity of nearly 60 cm. per sec., at the slow rate with a velocity of approximately 30 cm. per sec.[14] The times of stimulation were 5, 15 and 25 sec.[15] The apparatus was placed in a dark room, so that the illumination was under control. The cylinder was lighted from within by a daylight mazda lamp placed above and slightly in front of the *O*. There were no shadows, and the illumination was uniform over the whole field of vision.

The *O* sat upon a stool at the centre of the cylinder. A biting board, rigidly supported from the floor, was employed to insure a constant distance of 29 cm. between *O*'s eyes and the cylinder wall.

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[12] An apparatus of this form was used by J. Aitken in 1878 (*Proc. Royal Soc. Edin.*, x. 1878, 40 ff.; *Journ. Anat. Physiol.*, xiii, 1879, 322 ff.) with negative results. The cylinder was rotated about the *O* and then lifted; no after-effect was observed upon the surrounding objects. The sole uniform experience was a disagreeable, sickening effect. E. Budde employed the same apparatus in 1884 (*Arch. f. Anat. u. Physiol.*, 127 ff.). The experiences set up were so trying that the experiments were soon broken off.

[13] This is a part of the apparatus for localisation of sounds described by M. Bentley, this JOURNAL, xxiii., 1912, 509.

[14] The exact rates were respectively 57.3 and 34.1 cm. per sec. Cf. Wohlgemuth, *op. cit.*, 28, par. 8.

[15] We had found, in our preliminary experiments, that the short exposure times were as effective as the long to produce the after-image of movement. Nausea and dizziness, as the two earlier authors found (footnote 12), result from the stimulation of the cylinder; but they may be eliminated by shortening the time of exposure. With the intervals used, only two *O*'s reported these experiences; and in neither case were they sufficiently strong or unpleasant to force a withdrawal from the experiment. As the experiment progressed these *O*'s became habituated, and (like the other *O*'s) were unaffected even by the 25 sec. stimulation.

This distance, chosen after trial, proved very satisfactory; there was no noticeable eye-strain.

The cylinder was stopped by a brake operated from *E*'s desk by a rigid lever-arm 3 m. in length. The effect was absolute and instantaneous. Great care was taken with this part of the apparatus; and records by the graphic method showed that the arrest of motion was sharp and abrupt; there was no indication either of continued movement or of rebound when the brake was applied.

There was no fixation point. The *O*'s were directed in half of the experiments to fixate the surface of the cylinder and in half to look through and beyond the screen, *i.*., to fixate at 'infinity.'

We had, then, two velocities, a fast and a slow; three durations, 5, 15, and 25 sec.; and two fixations. We had also separate instructions for meaning and for process. The series were repeated 5 times.

The *meaning* instructions were as follows: "When the signal 'Ready' is given close your eyes. The cylinder will then be set in motion. When the proper velocity has been reached the signal 'Now' will be given. On the 'Now' open your eyes and (*a*) fixate the surface of the screen at a point directly in front of you; (*b*) look through and beyond the screen, fixate at 'infinity.'"

In the preliminary experiments you reported, among other things, an after-effect of various degrees of intensity, duration, and extent. You are now to characterize this perception as fully as you can. Designate the degree of movement in the after-image on the following scale:

0. no after-effect
1. after-effect poor
2. after-effect fair
3. after-effect good
4. after-effect very good
5. after-effect excellent

When the objective movement has ceased and the resulting phenomena have run their course, give an account of them."

In the instructions for process the same general *Aufgabe* regarding signals and fixation was laid down. The specific instructions for meaning were omitted, and the following sentences were added: "In the preliminary experiments you reported after-images of movement. In this experiment you are to concentrate upon description. When the objective movement has ceased and the resulting phenomena have run their course, describe the mental processes in strictly psychological terms."

Only the practised *O*'s observed in this part of the experiment. The order in which the series were presented was different for every *O*, as follows:

Series	Observer				
	A	Da	Di	T	W
Meaning, near fixation...........	3	haphazard	2	1	4
Meaning, far fixation............	4		3	2	1
Process, near fixation............	1		4	3	2
Process, far fixation............	2		1	4	3

Results.—The results of the experiments upon 'meaning' appear in Tables II and III.

TABLE II

SHOWING THE NUMBER OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED, DISTRIBUTED ACCORDING TO THE POSITION OF FIXATION, THE RATE OF THE OBJECTIVE MOVEMENT, AND THE DURATION OF THE STIMULATION

O	Fixation	Rate	Duration				Total Near or Far	Total Slow	Total Fast
			5	15	25	Total			
A	Near	S	2	3	4	9	13	20	7
		F	1	1	2	4			
	Far	S	3	4	4	11	14		
		F	1	1	1	3			
	Total		7	9	11	27			
Da	Near	S	5	5	5	15	28	28	27
		F	4	4	5	13			
	Far	S	3	5	5	13	27		
		F	4	5	5	14			
	Total		16	19	20	55			
Di	Near	S	1	1	4	6	12	9	7
		F	1	2	3	6			
	Far	S	0	1	2	3	4		
		F	0	0	1	1			
	Total		2	4	10	16			
T	Near	S	4	5	5	14	27	25	26
		F	3	5	5	13			
	Far	S	2	4	5	11	24		
		F	4	4	5	13			
	Total		13	18	20	51			

O	Fixation	Rate	Duration				Total Near or Far	Total Slow	Total Fast
			5	15	25	Total			
W	Near	S	5	5	5	15	30	30	30
		F	5	5	5	15			
	Far	S	5	5	5	15	30		
		F	5	5	5	15			
	Total		20	20	20	60			
Summary			58	70	81	209	110/99	112	97

Table II summarises the cases in which the after-effect was reported by the *O*'s according to the position of fixation, the velocity of the objective movement, and the duration of the stimulation.

Under our conditions a negative after-image of movement was perceived by all *O*'s. This result was corroborated by those of the unpractised group, which now comprised 12 *O*'s. A series of five experiments, with near fixation, fast velocity, and a 25 sec. exposure, was taken with every member of this group. The meaning instructions alone were used. Four *O*'s reported the perception of reverse movement in every experiment; three in 80%; one in 60%; three in 40%; and one in 20% of the experiments. It should be recalled that all investigators have found that the after-effect becomes more pronounced with practice, and that this group approached the problem without the slightest training.

The results obtained from the two groups of *O*'s in this part of the experiment indicate that an after-effect occurs when the whole visual field is filled by an objective moving stimulus. Compulsory conditions, however, were not obtained for every *O*.

In the practised group compulsory conditions were obtained for three *O*'s: W reported the after-effect in every experiment, no matter how (within our limits) the variables were arranged; T reported it in every case when the stimulation had been extended to 25 sec.; and Da in every case when the fixation was 'near' and the velocity 'slow;' also in every case, no matter where the fixation or what the velocity (within our limits), when the stimulus was extended to 25 sec.

Compulsory conditions were not obtained for A and Di. Their reports, however, indicate the approach of compulsion: an increase in time of stimulation is paralleled by an increase in reports of the after-effect. For the 5 sec. exposure these *O*'s report the after-effect in 7 and 2 cases respectively; for the 15 sec. exposure, in 9 and 4 cases; and for the 25 sec. exposure, in 11 and 10 cases. Had our variable conditions been slightly extended, or had the time of stimulation alone been lengthened,[16] it is very probable that we should have obtained compulsory conditions for them also. Owing to the short duration of the summer session it was impossible to carry the experiment further.

The summary at the foot of Table II shows that long stimulation, near fixation, and slow movement are, within our limits, the most favorable conditions for the perception of the illusion. As in the data of A and Di, so also in the results of the other *O*'s, the after-effect is most frequently reported when the exposure is extended to 25 sec. The summary also shows that the after-effect is reported 110 times with near fixation, 99 times with far; 112 times with slow rotation of the cylinder, 97 times with fast. The differences here are slight, and probably of little significance, since they are primarily due to the results of a single *O*: to those of Di in the matter of position of fixation, and to those of A in that of rate of rotation. Our conclusion is that position of fixation and rate of objective movement are irrelevant moments, and that the conditioning factor is the duration of stimulation.

These conclusions are confirmed by the *O*'s estimate of the cognitive clearness of the after-effect, which is presented in Table III.

These results parallel those of Table II. The *O*'s who report comparatively few cases of an after-effect place them for the most part at the lower end of the scale, whereas those who report many cases distribute them toward the upper end.

The results regarding the variable moments were likewise confirmed: the distribution upon the clearness scale for 'near' and 'far' fixation, and for 'slow' and 'fast' objective movement, shows no relevant differences, whereas the distribution according to the duration of stimulation shows a shift from the lower end for the short exposures to the high end for the long exposures. Even in the case of W, who reported the phenomenon in every experiment, the clearness of the after-movement shifts with the longest exposures to the highest degrees.

An analysis of the introspective reports yields the following results:

(1) The movement, given in the exposure and in the after-effect, is at times subjectified. Some *O*'s report that they revolved as on a swivel chair, and that the screen was stationary. The after-effect in these cases is merely a reversal, as in the rotary chair experiment, of the direction of the subjective movement. Examples are: (A) "Swimming sensations; I revolved in direction opposite to that of the cylinder. When the objective movement stopped, I rotated in the opposite direction;" (Di) "Pressures in head; feel of moving backward; this feeling kinaesthetic not visual;" (W) "Seemed as if I were doing the moving; felt as if I were moving to the right" (rotation of cylinder was to left).

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[16] Wohlgemuth found that short stimulations of 5 sec. or less (Exp. 31, p. 85) failed to produce an after-effect of movement. He used exposure times varying from 20 sec. to 3 min. The majority of his experiments were performed with times lying between 30 and 60 sec. Our preliminary experiments showed that it was possible to obtain the illusion with the shorter times. The obvious advantage of using these shorter exposures lies in the fact, mentioned above, that the disagreeable and distracting effects, nausea and dizziness, caused by the rotating cylinder, were reduced to a minimum.

TABLE III

Showing the Number of Cases in Which the After-Effect was Reported at the Various Levels of Cognitive Clearness, and a Distribution of the Cases According to the Position of Fixation, the Rate of Movement and the Duration of the Stimulation

O	Degree	Total No. Cases	Fixation		Rate		Duration		
			Near	Far	S	F	5	10	15
A	0	33	17	16	10	23	13	11	9
	1	10	5	5	8	2	3	4	3
	2	8	3	5	7	1	2	3	3
	3	5	3	2	4	1	2	1	2
	4	3	2	1	1	2	0	1	2
	5	1	0	1	0	1	0	0	1
Da	0	5	2	3	2	3	4	1	0
	1	23	15	9	13	11	11	7	6
	2	11	4	7	3	8	3	3	5
	3	12	5	7	8	4	1	8	3
	4	6	3	3	3	3	1	1	4
	5	2	1	1	1	1	0	0	2
Di	0	44	18	26	21	23	18	16	10
	1	13	9	4	9	4	2	2	9
	2	0	0	0	0	0	0	0	0
	3	2	2	0	0	2	0	1	1
	4	1	1	0	0	1	0	1	0
	5	0	0	0	0	0	0	0	0
T	0	9	3	6	5	4	7	2	0
	1	29	13	16	19	10	9	11	9
	2	19	13	6	6	13	3	7	9
	3	3	1	2	0	3	1	0	2
	4	0	0	0	0	0	0	0	0
	5	0	0	0	0	0	0	0	0
W	0	0	0	0	0	0	0	0	0
	1	3	2	1	2	1	3	0	0
	2	11	7	4	8	3	7	3	1
	3	3	1	2	3	0	1	1	1
	4	18	9	9	9	9	8	8	2
	5	25	11	14	8	17	1	8	16

(2) The black lines are at times seen in perspective; they are objectified, and appear as rods set at various distances from the eyes. All the *O*'s report experiences of this kind. Examples are: (A) "The lines stood out in perspective. Three in a group; that is, three were equally near, then the next three were back, next three close, and so on;" (Da) "At times the lines were seen in perspective, like black rods arranged at various distances in front of the observer;" (Di) "Lines stood in perspective;" (T) "The projection-effect was very

conspicuous; every second line seemed to stand out;" (W) "Once during the experiment there was a projection of the lines." "Twice during this experiment I noticed a projection; lines stood out in model-effect on grey background."

(3) The lines at times lose their shape and outline. Patterns of varying degrees of complexity appear upon the ruled surface. Simple changes (the lines become fringed and nodulated) and complex changes (large diamond and zig-zag effects) are of about equally frequent occurrence. The appearance of the simple and complex patterns can be correlated roughly with the slow and fast velocities. Examples are: (A) "The lines during motion became small squares, with vertical and horizontal cross-lines. These patterns appeared first at the center of the field of vision, and radiated until the entire field was covered;" (Da) "During the objective movement the field became spotted; the lines lost their regular outline;" "lines became nodulated;" (T) "Grey fringe to the right edge of the lines;" (W) "Lines became ripply;" "lines interwoven into complex pattern."

(4) Color effects are produced by the colorless stimuli. The hues are brilliant and filmy. They flicker and fluctuate. Sometimes they change as in the 'flight of colors;' sometimes antagonistic colors alternate; sometimes two colors (always antagonistic) appear together. Thus: (A) "The lines during motion became brilliantly colored;" (Da) "Left edge of lines blue, right edge yellow;" "lines alternately tinged with yellow and blue;" (T) "During objective movement the lines became wavy. The left side was colored, quality changing from yellow to red, to blue, to green, etc. The wavy figures instable, rapidly changing form and color;" (W) "During motion the lines became tinged with blue."

(5) The lines fuse as in color-mixing into a uniform background, in which case there is no after-effect; or into a flickering background as when the rotation of the color-mixer is too slow, in which case the after-effect is either not observed or if observed is reported as of low degree. Thus: (A) "Lines blended; there was no after-effect;" (Da) "Lines were nodulated; seemed to be stationary blinking spots" (no after-effect of movement was reported); (Di) "Faded to a shimmering gray." Di frequently reported this phenomenon; (T) "Lines became thicker in width at some points and thinner at others. Positions of thinness and thickness not constant, continually changing;" (W) "Toward end the whole thing became a mottled grey" (the after-image of movement was reported as poor).

(6) As Aitken and Budde discovered, stimulation from within the cylinder produces a widespread organic and muscular response. Dizziness and nausea are frequently reported, and tend to obscure the process-configuration. Examples are: (A) "Swimming in head;" (Da) "Strain sensation in muscles of eyes and pressure within head. Dizziness and an organic stir-up;" (Di) "Intense pressure in head, eye strain, and slight dizziness;" (T) "Very strong strain in eye muscles and dull pressure in head;" (W) "Very dizzy and nauseated, pressure sensations in head."

The occurrence of the after-effect can be directly correlated with the arousal of these phenomena. Di, who reports the after-effect least often, reports dizziness but once; W, who reports the after-effect in every experiment, reports dizziness most frequently. This correlation does not indicate, however, that 'dizziness' is essential to the appearance of the after-effect. No *O* reports dizziness every time that he reports the after-movement. It is only in the early experiments that the correlation, even with W, invariably occurs. It appears, therefore, that the optimal conditions for the after-effect are also optimal for the perception of dizziness. It is not until these perceptions became habituated that the process-configuration becomes clear.

(7) The after-effect is reported in precisely the same terms as in our previous experiments. The same perception, an after-image of movement opposite in direction to that of the stimulus, and differing only in extent, is obtained when the entire visual field is filled by an objective moving stimulus as when, with James' waterfall apparatus, the stimulus is limited to a small area of the retina.

(8) The introspections at first include kinaesthetic and organic complexes. As the experiments progress and the *O*'s become accustomed to the experiences, the inessential elements drop away, leaving a residue of pressure and visual sensations.

The pressure sensations within the head never entirely disappear. They are always present when the after-effect is reported.

The visual configuration reported by three of the *O*'s—A and Di, even in the experiments under process-instruction, do not get beyond statements of meaning—is a qualitative and temporal, and perhaps spatial, integration. Examples are: (Da) "Immediately after the objective movement ceased the field became momentarily a blur and grayish; then the vertical lines stood out distinctly again;" "The gray blur was of short duration. On the side of meaning movement rapid;" "When objective movement ceased, field seemed to be covered by a gray cloud; this lasted for a short time; then the vertical lines appeared, distinct in outline. On the side of meaning movement slow;" "When objective movement ceased there was a momentary gray blur, which was followed by distinct vision. On side of meaning a rebound;" (T) "When the objective movement stopped there was a grey; the lines became wider; and then they became distinct again;" "When the objective movement stopped I saw gray like a film;" "When the objective movement stopped the lines momentarily appeared clear in outline, then expanded to right. Over these lines were gray lines; these were indefinite and instable" (reported movement as of the 2nd degree); (W) "When movement ceased lines blurred into a gray;" "After objective movement ceased each line seemed to become broader; meaning, movement to right;" "When objective movement ceased a gray screen appeared; this seemed to move to right."

Summary

(1) We divided our *O*'s into two groups; the one group was given a practice series before the main experiments were undertaken, the other group approached the problem naïvely.

(2) Repetition of Wohlgemuth's work showed:

(*a*) that his apparatus is unsuited to the problem;

(*b*) that even under unfavorable conditions the after-effect is reported in a large percentage of cases by *O*'s of both groups. Individual differences are large, and the effect of practice and training is clearly evident;

(*c*) that the apparatus, if the problem of the after-effect of complete visual movement is to be successfully attacked, must be variously modified.

(3) Experiments with a large cylinder, constructed to meet the requirements of complete visual stimulation, showed:

(*a*) that the after-effect is observed when the whole visual field is filled by an objective moving stimulus;

(*b*) that position of fixation and rate of objective movement (within our limits) are irrelevant moments, and that the conditioning factor is the duration of stimulation;

(*c*) that the characteristic processes of the after-effect are pressure sensations within the head and a qualitative, temporal, and perhaps spatial integration of visual sensations.

(4) Wohlgemuth's failure to attempt a reconciliation of his theoretical views with the negative results of his Exp. 22 turns out to be of no consequence: not because the results of that experiment are unimportant, but because under strict conditions they are positive. As it happens, Wohlgemuth's two negatives amount to an affirmative.

BOOK REVIEWS

The religious consciousness: a psychological study. By J. B. PRATT. New York, The Macmillan Co. 1920. pp. viii, 486.

This very substantial and comprehensive addition to the growing literature of the subject furnishes new evidence of the pre-eminence of American scholars in the psychology of religion. The reader may find here in very clear and attractive form the results of this twenty-one-year old science. The chapters arrange themselves into four groups. The first four may be considered as introductory, dealing with the definition of religion, the nature of the psychology of religion, the significance of the subconscious and the relation of society and the individual. The next five chapters cover the topics and materials presented in several books beginning with Starbuck's. The discussions treat of the religion of childhood, adolescence, conversion and revivals. Six chapters follow which contain perhaps the most original and vital pages of the entire book. The subjects are belief in God and immortality, the nature of cults, and the phenomena of worship and prayer. The last five chapters give an exposition of mysticism and a criticism and estimate of its central principles and practices. Certain problems in each of these groups of chapters will be considered in this review.

The preface emphasizes the purpose of the author to give a description of the religious consciousness in a purely objective way without the bias of any point of view. This he succeeds in doing unless it is in certain passages touching upon the .work of some writers whose point of view is avowed and different. The definition of religion given is suggestive of the procedure. "Religion is the serious and social attitude of individuals or communities toward the power or powers which they conceive as having ultimate control over their interests and destinies." The attractiveness of this definition lies in the use of the term "attitude" instead of belief, and in the recognition, though somewhat faint-heartedly, of the social character of religion. The difficulty which the author has with this term 'social' gives the impression that it is forced upon his attention by the drift of current thinking, but without his full consent or satisfaction. He still regards the social as in some way opposed to the individual, but the relation is not clearly set forth. For example, he would seek the origin of religion "within the subjective needs of human nature," but "the religious consciousness inevitably considers its religion objective as well as subjective." Subjective here apparently means subjective to the individual, and objective means more objective than writers like Durkheim would imply by the word social. By thinking of the social as subjective the author fails to recognize the force which the expression "social consciousness" has for those writers whose use of it he criticizes. In connection with the definition it is indicated that the "power or powers" referred to may be regarded as equivalent to the "Determiner of Destiny." The latter expression is the more frequently used. The value of this term evidently lies partly in its vagueness. It is not intended to designate exclusively a personal object of the religious attitude, although it might comprehend that with the non-personal types. "To describe the workings of the human mind so far as these are influenced by its attitude toward the Determiner of Destiny, is the task of the psychology of religion."

The second group of chapters gives an excellent restatement of the development of religion through childhood and adolescence. The author holds with practically all present-day psychologists that there is no one religious instinct. He rejects the conception of childhood which holds that "Heaven lies about us in our infancy!" He says "*Earth* lies about us in our infancy," and the baby is a little animal. Family influence is the most potent in religious development, but all social contacts make their contribution. Adolescence is "the flowering time for religion." It is, however, full of ups and downs, of storm and stress. The individual gains in power and selfhood but at the same time experiences bewilderment and depression. The essential thing about conversion is the unification of character, the achievement of a new self. Many conversion-experiences are quoted from various religions and sects. Revivalism is criticized adversely for the most part, with an analysis of its methods of emotional, mob suggestion.

The third group of chapters introduces more new observations and discussions than any other part of the book. The two main questions raised concerning God are: why do people continue to believe in God? and what are the psychological factors that influence or determine the meaning of that term? It is observed that the idea of God is generally not very clear. A good deacon is quoted as confessing that his idea of God was "a kind of oblong blur." Sociological influences, such as the form of family and political control, have much to do with impressing the fatherly and kingly patterns upon the idea. The psychological influences are largely in the field of sense and imagination. Images, pictures, lights and sounds are conspicuous. Both sensuous and conceptual factors belong to the idea of God, though the vividness and definiteness of these factors vary greatly in individuals and in the different moods of the same person. There is found here a certain justification for the anthronomorphic image of God which so many persons have struggled to eliminate. The image may be merely symbolic, but it has great power over the emotions. The imaginative and the conceptual aspects of the idea of God, as of other ideas, are not necessarily in conflict but are normally, though with varying emphasis, present and vital. The reasons why people believe in God are put under four types, authoritative, reasoned, emotional and volitional. These types are illustrated by instances received through questionaries. Under the reasoned type appear the influences of the traditional arguments, the cosmological and the teleological. Others reason from the practical effects for good upon the lives of those who believe. The emotional type seems to be more numerous than any of the others. This includes cases of the "experience of God's presence," as among the mystics. The volitional type embraces those whose faith arises from a more or less deliberate "will to believe." The same four types of belief are cited with reference to immortality. Belief in immortality is regarded as coming earlier and as being less dependent upon instruction than belief in God. It is more a matter of feeling and will, of instinctive impulse toward self-preservation. There are, however, many people who do not care for immortality, and there seems to be a lessening desire for it through the last five hundred years because, partly at least, this world has become more attractive.

A good account is given of the cult and its function. The author's travels in India and his observation of the ceremonies of the Roman Catholic church have enabled him to appreciate the poverty and barrenness of Protestant services so far as symbolism is concerned. Accepting the genetic account of the writers who find the origin of the cult in social activities, Professor Pratt seems to fail to recognize the way in which the sense of the group may also supply the equivalent of the "Determiner of Destiny," and how such writers as Durkheim, Jane Harrison, and Cornford have shown the dependence of the cosmic upon the social consciousness. The latter point is dismissed too easily (p. 261). The use of images and idols in religious ceremonies comes out in a new light when viewed in reference to the value of these as means of enlivening religious faith and feeling. "The sensuous representation reinforces the reality-feeling. Much more may thus be said in defence of the practice of 'idolatry' than most of us have been brought up to suppose." Further, the activities of the cult have a similar value for the participant. "The feeling of these acts is a considerable part of the religious emotion." To be most potent in one's experience it must have been cultivated in childhood. An interesting comparison is made between the objective worship of the Roman Catholic church and the subjective worship of Protestantism. The Catholic church occupies itself with the worship of God. The Protestant seeks the salvation of souls. It is immaterial to the former whether a congregation is present, but Protestantism is dependent upon the presence of people. The candles of the Catholic churches are for God; the flowers of the Protestants are for the people to see. The author elaborates this contrast very convincingly. It is doubtful, however, whether he has not overstressed the subjective phase of Protestant services. Does any one "attempt to produce merely subjective religious effects?" Here an effect achieved with the group seems to be considered as much subjective as the emotional state of an individual. But it often happens that the common will of an assembly deliberating upon measures of public welfare communicates itself to the individuals as an objective, super-individual judgment. It does not seem quite exact to designate that consensus of opinion and feeling as "subjective." The author admits that "objective worship of the sort that aims to please the Deity is a thing of the past" (p. 308). His description of objective worship other than this is not quite clear. He surely does not mean that men can worship "cosmic forces." It is likely that the author has not yet spoken to his own satisfaction on this point. Having so qualified his "Determiner of Destiny" as to render it unable to take the rôle of God as commonly conceived, he does not adequately provide for the central factor in religious worship. The same problem concerning the meaning of the word subjective affects the discussion of prayer. "If it be true both that the subjective value of prayer is very great and also that it is the only value which prayer possesses, this latter fact should be assiduously kept secret."

Professor Pratt gives more space to mysticism than is found in any other general treatise on the psychology of religion. Mysticism is defined "as the sense of the presence of a being or reality through other means than the ordinary perceptive processes or the reason." Two types are distinguished, the mild and the extreme. Extensive quotations are cited to set forth their characteristics. The sense of presence is the largest factor. This is often most vivid when the mystic is alone on the hills or in the woods. It is susceptible of cultivation; and the literature of mysticism is much occupied with the technique and discipline by which its goal is reached. Pratt recognizes the fact that many mystics are abnormal and that their quest for this great experience has brought the illusion of attaining it. Still he does not share the views of Murisier and Leuba in this respect, but holds that mysticism has positive value and adds assurance and vividness beyond ordinary religious experience. In the discussion of mysticism, as in other parts of the book, the author shows his close affinity to William James.

The style is clear and entertaining. The use of first-hand material secured by questionaries adds concreteness and vividness, but there can be little scientific value in trying to employ the method of percentages on 170 replies concerning prayer or belief in God! The abundant references and extensive footnotes open into attractive elaborations of the discussions in the text. While scarcely prepared in the manner of a text-book, the book will undoubtedly be used for that purpose and as collateral reading. For the general reader and for the teacher and preacher of religion it affords one of the most adequate treatments of the subject to be had. It is critical and discriminating, but it is also decidedly friendly and constructive.

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Das jüdische Volk und seine Jugend. By S. BERNFELD. Wien, R. Löwit Verlag, 1920. 149 pp.

The first half of this book is devoted to a negative criticism of the aims and results of European education. Since the child is looked upon as an adult 'in the rough,' whom it is the function of the educational process to polish into adulthood, the results of European education are bound to be an unnatural forcing of the child's development and the destruction of the cultural possibilities inherent in youth.

The second half of the book gives a vivid presentation of a system of education to prevail half a century hence in Palestine, which is conceived as possessing by that time the seat of a Jewish commonwealth. The national constitution provides for the complete support and control by the state of the entire youth of the country from birth to the age of twenty. The young people are concentrated in settlements in different parts of the country, under the guidance of a psychologically and medically trained corps of educators, whose office is merely to observe the development of their charges and to be ready to aid them in their spontaneous efforts to acquire any particular discipline or skill. Within these communities the great thinkers and artists of the country, pursuing their work in a congenial atmosphere and drawing interested groups around them, create centers for cultural growth.

This Utopia can only be realized in a country whose productivity, necessarily high, accrues not to the individual but to the state, and among a people who appreciate youth for its own sake,—therefore in a Jewish Palestine. S. FELDMAN

NOTES

A NOTE ON "VOCALITY"

In a recently published paper,[1] Dr. Weiss attempts to redefine the 'attribute' of vocality. He seeks to identify judgments of vowel-character with that attribute which is describable in terms of mellowness and shrillness, and asks all investigators to use the name "vocality" for this characteristic.[2] It would seem advisable to examine critically the foundations upon which such a classification may be based.

A psychophysical study of the tonal attributes brings forth evidence, cumulative rather than direct, that judgments of vowel-quality are not made upon a purely attributive basis, but result from the observer's experience in the use of spoken language.[3] This Dr. Weiss tacitly admits when he says that "when a subject is asked to discriminate the vowel character of a fork tone, the reaction is one that is already well established in the ordinary (speech) habits of the individual."[4] But he regards these habits as merely selecting the conventionalized points in an already existent continuum, which may be described either in vocal terms or in terms of mellowness and shrillness. Moreover, these conventionalized points occur in an octave relationship, because the process of conventionalization picks, as the vowels of any language, combinations of mellowness and shrillness that are separated by equal sensorial steps, corresponding with logarithmic intervals in the series of physical frequencies. The octave-relationship is, then, to be regarded as manifestation of the Weber-Fechner Law.[5]

My own study of the tonal attributes shows that the limen for vocal judgments does not follow Weber's Law.[6] Of the tonal attributes, volume is the only one that gives indication of following this law.[7] A comparison of the differential limens for volume and for vocality shows that the former are uniformly the larger of the two, even though the vocal judgment is more difficult and less immediate than is the judgment of volume. This relationship, of course, precludes the possibility that the vowels are judged upon a basis of volume, that is to say, upon the basis of the characteristic of pure tones that obeys the logarithmic law.

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[1] A. P. Weiss, The Vowel Character of Fork Tones, *Amer. Jour. of Psychol.*, 31, 1920, 166 ff.

[2] *Op. cit.*, 191.

[3] G. J. Rich, A Study of Tonal Attributes, *Amer. Jour. of Psychol.*, 30, 1919, 131 ff. Although Dr. Weiss' paper appeared later than mine, it would seem that he wrote it at an earlier date.

[4] *Op. cit.*, 192 f.

[5] *Op. cit.*, 190 f.

[6] G. J. Rich, *op. cit.*, 139.

[7] *Op. cit.*, 149 ff. Also A Preliminary Study of Tonal Volume, *Jour. of Exper. Psychol.*, 1, 1916, 13 ff.

When he invokes the Weber-Fechner Law as an explanatory principle,[8] Dr. Weiss narrows his conception of vocality to that of a characteristic which shows the octave-relationship found only by Köhler. The trend of recent experimental work tends to show that the vowels of a language (Dr. Weiss' 'conventionalized vocality combinations') seldom occur an octave apart.[9] Köhler's experimental situation was such that, as Stumpf has suggested,[10] his observers may well have tended to judge as the 'pure vowels' the same tonality occurring in successive octaves. This would seem to be a sufficient explanation of the relationship found by Köhler, especially in view of the subsequent lack of verification.

On the other hand, vowel-character is considered by Dr. Weiss as a continuum which is to be described in terms of mellowness and shrillness, terms which are descriptive of pitch-brightness. In so far as the limens for pitch-brightness are in all cases lower than the vocal limens, this attribute may serve as the basis for the perceptual judgments of vowel-likeness.[11] The successive character of the qualities that form the pitch-brightness series is definitely in line with what is seemingly the only constant factor in the results of the many investigations of vowels, the order in which the vowels follow one another as one ascends the musical scale.[12] But the possibility that vocal judgments are based upon pitch-brightness does not necessarily *identify* whole vowel-series within a range of little over an octave. *Op. cit.,* 133. them with the latter attribute. The very fact that the limens for vowel-character and for pitch-brightness are vastly different, both in magnitude and in course,[13] is evidence that the vocal judgment is not made singly and unequivocally upon a basis of pitch-brightness, that is to say, it is not merely a judgment of pitch-brightness in a 'conventionalized combination.' There is not at present sufficient evidence to warrant the assertion that the vocal perception (or 'habit') is a single attributive continuum describable in non-vocal terms, in terms of one or more of the attributes of pure tones.

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[8] The Weber-Fechner Law would explain fully as satisfactorily the occurrence of the pure vowels (the 'conventionalized vocality combinations') at any intervals other than the octave, *e.g.,* at intervals of fifths, thirds, etc.

[9] D. C. Miller, *The Science of Musical Sounds,* 1916. H. Schole, Über die Zusammensetzung der Vokale U, O, A, *Arch. f. d. ges. Psychol.,* 13, 1918, 12 ff. C. Stumpf, Die Struktur der Vokale, *Sitzungsber. d. Preus. Akad. d. Wissensch.,* 1918, I Halbband, 333 ff. G. J. Rich, A study of Tonal Attributes, *Amer. Jour. of Psychol.,* 30, 1919, 131 ff. Dr. Weiss' own experimental results fail to show an octave relationship.

[10] C. Stumpf, Über neuere Untersuchungen zur Tonlehre, *Ber. ü. d. 6 Kong. f. exper. Psychol.,* 1914, 305 ff.

[11] G. J. Rich, *op. cit.,* 138 f; 144 f; 154 ff; 164.

[12] Thus, unpractised observers may report, in proper succession, the

[13] See Footnote 11.

CONSCIOUSNESS IN THE SIAMESE TWINS

Bolton's account of the Siamese twins in 1830 (G. B. Bolton, *Philos. Trans.*, 1830, 177-186) suggests an overlapping of consciousness which is not unlike the overlapping of two hysterically separated consciousnesses. It will be recalled that the twins, Chang and Eng, were possessed of entirely distinct personalities and sensory equipment; only when the band uniting them was punctured by a needle at its very middle did both shrink, whereas half-an-inch to either side the prick brought response in only the one or the other. Nevertheless Bolton thought he found some community,—an overlapping "unconscious" and a distinct "conscious" perhaps he would have said to-day! He wrote:

"They always fall asleep at the same moment, and it is impossible to wake one without also arousing the other. When they were at Boston, Doctor Skey, Surgeon General to the British Army, entered their bed-room at midnight on three successive nights when both were asleep: on each occasion he touched one and was answered by the other, both awaking at the same instant, inquiring why they were disturbed.

"The experiment has also been repeated in this country, and with the same result. On my tickling one of them, the other told me to desist, though he stated that he did not feel the touch, and it was quite clear that he could not see me tickle his brother.

"On their voyage to England one of them had the tooth-ache, during three days and nights, and suffered great pain, with loss of sleep, which last evil was shared by his brother, both remaining awake. On the 16th of December Mr. Hale went into their bed-room when they were asleep. Eng was restless and tossing about in bed, while Chang was screaming. He awoke them, and on inquiring what ailed them, Eng replied that he was dreaming about his mother, and Chang said that a man was cutting off the long hair from his head. These different dreams appear to have occurred simultaneously" (p. 184).

E. G. B.

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THE STIMULUS-ERROR*

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The purpose of this paper is to discuss the " stimulus-error," to indicate something of its history (though limits of space will preclude more than a bare outline), to add something by way of definition (since definition has remained implicit and there are some who do not understand this term), to enquire, at the level of the scientific experiment, into the significance of the attitude which is thus styled an " error " (relying as much upon experimental observation and as little upon epistemological conviction as is possible), and to arrive, if may be, at an evaluation of the stimulus-error or stimulus-attitude in its relation to the psychology of the present day. This is not so large an order that it does not need filling. Some psychologists put out of court experiments that involve the stimulus-error; others refuse to see any ' error ' at all and discount the works that stress this ' merely epistemological ' distinction. And when we seek a sanction for the one view or the other, we are at a loss whither to turn, for the " stimulus-error," although it has a long history, has been left to make its way without any very formal introduction.

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* The present paper is the outcome of a promise to deal specifically with the nature of the stimulus-error, especially with its relation to psychological measurement and psychophysics. *Cf.* the discussions of the present writer in The logic of the normal law of error in mental measurement, *Amer. J. Psychol.,* 1920, 31, 1-33, esp. 27ff.; and in The control of attitude in psychophysical experiments, *Psychol. Review,* 1920, 27, 440-452, esp. 447f., 449 and note.

The Stimulus-Error

Undoubtedly much of the confusion and disagreement has been brought about by the term itself: "stimulus-error." It implies something that is right and something that is wrong, defending one position and impugning another. It serves, and was intended, to throw two positions into contrast, to insist upon an important distinction that is often overlooked; yet does not stop with definition, but goes on to pass a judgment. In this dual function of the phrase there has been both an advantage and a disadvantage. To those who accept both the implied distinction and the explicit evaluation the notion has been exceedingly useful, for it has enabled them, not only to separate the methodological sheep from the goats, but also to dispense with the goats,—a telescoping of procedure that is convenient and economical. On the other hand, those psychologists who have staked their fortunes on the goats of stimulus are not to be reformed by being found in the way of the "stimulus-error." They simply deny the "error" and in so doing miss the more fundamental distinction between opposing positions that must be made out before judgment can be passed upon either. We ourselves must not be thus misled, whatever our ultimate judgment may be.

This implied opposition, which we must now bear clearly in mind, is the fundamental opposition in psychology—or between psychologies—of mental process and meaning, of content and object, of *Beschreibung* and *Kundgabe*. Titchener, who is responsible for the term "stimulus-error," puts the case thus:

> "We are constantly confusing sensations with their stimuli, with their objects, with their meanings. Or rather—since the sensation of psychology has no object or meaning—we are constantly confusing logical abstraction with psychological analysis; we abstract a certain aspect of an object or meaning, and then treat this aspect as if it were a simple mental process, an element in the mental representation of the object or meaning. . . . We do not say, in ordinary conversation, that this visual sensation is lighter than that, but that this pair of gloves or this kind of grey paper is lighter than this other. We do not say that this complex of cutaneous or organic sensations is more intensive than that, but that this box or package is heavier than this other. We do not even say, as a rule, that this tonal quality is lower than that, but rather that this instrument is flat and must be tuned up to this other. Always in what we say there is a reference to the objects, to the meaning of the conscious complex. It is not the grey, pressure, tone, that we are thinking of; but the grey of leather or paper, the pressure of the box, the pitch of the violin. . . . What is more natural than to read the character of the stimuli, of the objects, into the 'sensations' with which certain aspects of the stimu-

lus or object are correlated? . . . This is what Fechner did. . . . [He] transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology."[1]

We commit the stimulus-error if we base our psychological reports upon objects rather than upon the mental material itself, or if, in the psycho-physical experiment, we make judgments of the stimulus and not judgments of sensation. At the more complex levels we may make a similar error, a 'meaning-error,' which consists of describing objects, reporting meanings, stating *Kundgabe,* instead of describing mental process or giving *Beschreibung.* We can not, however, in this paper, extend the discussion to include this complex level, but must content ourselves with the conviction that whatever applies in the controversy between judgment of stimulus and judgment of sensation, applies also to introspection and its rival, the statement of meaning. We may concede that the psychophysical experiment in its simplicity represents the ideal ultimate in the psychological experiment, where control of conditions and adequacy of observation are maximal; and that we should be glad to reduce all psychological observation to this degree of rigor at least. At any rate any extension of this discussion to the 'higher' processes must wait, for the history and application of the stimulus-error are at the level of psychophysics, and the interpretation of the "stimulus-error," up to which we are leading, shows most clearly here.

The Quantity Objection and the Stimulus-Error

It is not surprising that a psychophysics, which seeks to establish the relation between the mental and the physical, should emphasize the distinction between sensation and stimulus. What is surprising is that the opponents of psychophysics should have raised this very distinction for the confounding of psychophysics and should have claimed that the psychophysical relationship (the logarithmic relation of the Weber-Fechner law) was an artifact created, not by the attempt of the psychophysicist to distinguish between sensation and stimulus, but by his confusion of the two. Yet such is the substance of the " quantity objection " to psychophysics, which had later to be met with the psychological sense-distance by Müller, Titchener, and others, who thus turned the tables and brought the argument for the distinction between mental and physical material to the support of a Fechnerian psychophysics. This was a long and tedious battle, and one might have expected that the resultant emphasis upon the two-fold nature of psychophysics would have determined the psychophysical universe for a time. On the contrary, however, the confusion between sensation and stimulus persisted. Cattell was fathering a psychology of the stimulus, and it was in the tradition of the work of Fullerton and Cattell that Urban did his experiments. Now that behaviorism has come into vogue, it is not apparent that we do not have two kinds of psychophysics—a psychophysics of process that gives, as Fechner wanted, the correlation between mental and physical data, and a psychophysics of behavior that seeks to identify response with its stimulus. That this psychophysics of stimulus-and-response needs also, if it is to be scientific, to take account of the error that has been called the " stimulus-error " is the thesis of the present paper; but the thesis must wait upon the perspective of the preface.

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[1] E. B. Titchener, *Experimental Psychology,* II, i, 1905, p. xxvif.; *cf. Text-book of Psychology,* 1910, 202f. Titchener first uses the term stimulus-error = "R-error" in *Exper. Psychol.,* II, ii, 1905; see pp. lxiii, 198f., 203ff., 207, 219, 223, 230f., 262, 450. For Titchener's further use of the term, see *Text-book,* pp. 218, 350, 398 note, 522. J. v. Kries characterized the objectifying attitude as a "source of error:" "Wenn man aber diese Quelle des Irrthums ausschliesst und möglichst an den objectiven, als Reiz dienenden, Vorgang gar nicht denkt . . . ;" *Vtjschr. f. wiss. Philos.,* 1882, 6, 275; and Titchener seems to refer to this discussion as a sanction for the term "stimulus-error."

A clear recognition of the distinction between mind and body, between consciousness and objects, was the key-note of Fechner's position. There was for him at least this dualism in the universe, which may be regarded from one standpoint or the other. The case is not unlike, Fechner argued, the Ptolemaic and Copernican worlds. The geocentric and heliocentric solar systems are different systems, and we may at pleasure take either point of view that we choose. The worlds remain distinct. Or the matter is like a circle, which may be viewed from the inside or the outside. In the one case we see only concavity, in the other only convexity. Such a dualism can be resolved only by the law of relationship that holds between its two aspects, and, just as the relationship between concavity and convexity can be stated geometrically for the circle, so the logarithmic law resolves the dualism of mind and body. There is no doubt, therefore, that Fechnerian psychophysics stands or falls according to its success in distinguishing between measurements of mind and measurements of body, or between sensation and the object of sensation, the stimulus.[2]

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[2] G. Th. Fechner, *Elemente der Psychophysik,* esp. 1889, I, 1-12.

The vigorous opposition that developed to Fechner's psychophysics took its stand firmly upon the distinction between mind and body, but denied the possibility of a quantitative correlation between the two on the ground that mind was not possessed of magnitude and that mental measurement was an impossibility. This argument came to be known as the "quantity objection" and was the main source of opposition to quantitative psychology in the eighties and nineties of last century. Introspection, the objection runs, does not show that a sensation of great magnitude ever contains other sensations of lesser magnitude in the way that a heavy weight may [supposedly] be made up of a number of smaller weights. "Our feeling of pink," said James, "is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow-candle in itself."[3] "This sensation of 'gray,'" remarked Külpe, "is not two or three of that other sensation of 'gray.'"[4] "A blue surface," Ebbinghaus commented, "is something other than a green, but the latter has in itself, apart from memory of the colors, nothing of the doubleness or threefoldness of the green. . . . A low tone sounds different from a high tone, and in like manner a loud tone different from a soft."[5] In other words increase of magnitude in no sense means increase of complexity. A sensation is just itself no matter what its degree. The tone produced by many instruments in unison is not of itself composed of more units than is the tone from a single string, nor is the tone of many vibrations per second more complex than the tone of few vibrations. In this form the objection seems obvious enough. Sensational magnitude is certainly not multitude, and intense sensations are not integrated of more sensory stuff than are weak.[6] How then was psychophysics to defend itself?

Its immediate defense was a display of the factual material. Here were the experimental measurements. If they were not observations of the magnitudes of sensation, what were they?

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[3] W. James, *Principles of Psychology,* 1890, I, 546.

[4] O. Külpe, *Outlines of Psychology,* tr. 1896, 45.

[5] H. Ebbinghaus, *Z. f. Psychol.,* 1890, 1, 323.

[6] Titchener mentions as raising the quantity objection: G. E. Müller in 1878, Exner in 1879, Stadler in 1880, Zeller in 1881, Boas and F. A. Müller in 1882, Stumpf in 1883, Tannery in 1884, Elsas in 1886, Grotenfelt in 1888, James, Münsterberg, and Ebbinghaus in 1890, Sully in 1892, Külpe in 1893, Wahle in 1894, Meinong in 1896, Höfler in 1897, and Lehman in 1902. See Titchener, *Exper. Psychol.,* II, ii, pp. xlviii-lxiii.

To this question the raisers of the "quantity objection" replied that psychophysicists had created an artificial mental magnitude by a confusion of the sensation with the stimulus, that is to say, they had committed in their experimental work the "stimulus-error." This was a serious charge against a discipline that depended for its existence upon a sharp distinction between the mental and the physical. Let us see how the accusers dared to raise it.

Von Kries put the matter clearly:

> "An illusion is thus very easily brought about by the fact that one tends in general to estimate objective values (measurable in objective terms) according to the sensation. If one, however, excludes this source of error, and in so far as possible thinks not at all of the objective process serving as a stimulus, then one must necessarily admit that a quantitative relation does not exist between the different parts of an intensive series. This fact is most obvious to us when we do not attempt objectification as, *e.g.,* in pain. Whatever it is called, a pain exactly ten times as strong as another does not admit of such absolute statement."[7]

Ebbinghaus, somewhat later, was even more explicit:

> "In general one designates the brightness of a flame or a surface as 10 or 12 times another brightness, and could just as easily, it appears, designate a loud tone as the double or treble of a soft tone. But what occurs here is no longer an immediate sensation or an immediate judgment of sensations, but depends upon the introduction of experiences. We can readily experience, and we do every day experience, the fact that the arousal of a brightness or a loudness depends upon a diversity of just those physical things or processes that in limited number call forth the impression of darker or softer. In order to have an impression of greater brightness for a surface, one can increase the number of gas-flames illuminating it; in order to strengthen a tone, one multiplies the instruments carrying it. Such experiences with respect to the causes of sensations we have always in immediate view, and we believe that we have the numerical characteristics that always attach to the one occurring without anything further in the other. It is psychologically difficult to get rid of them, just as it is difficult not immediately to see in a grass-green apple its sourness. But if one succeeds in the perfectly possible separation of the thought context, then it is clear that, as the bare visual impression of an apple has no sourness in it, similarly the bare impression of brightness does not consist of the multiplicity of candles upon which, of course, it frequently depends."[8]

We have already seen what fifteen years later, Titchener had to say in the same vein and how, although defending mental measurement, he makes the charge of the stimulus-error against Fechner. And there were many others.

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[7] J. v. Kries, *loc. cit.*

[8] H. Ebbinghaus, *op. cit.,* 323f.; *cf. Grundzüge der Psychologie,* I, 1905, 71-79.

Exner put forward the general argument in 1879;[9] and Boas in 1882.[10] Tannery said in 1883: "It is the objective study of the excitation and its variations that leads to this definition of number that measures the sensation. At bottom it is by excitation that sensation is defined."[11] On epistemological grounds both F. A. Müller[12] and Meinong[13] concluded that mental magnitudes, unlike physical, were indivisible. And long before any of these, Brentano, the father of modern intentional psychology, had said: "If one measures, as Fechner did, the intensities of colors, tones, *etc.,* then one is measuring the intensities of physical phenomena. The color is not the seeing, the tone is not the hearing, the warmth is not the sensing of warmth."[14]

Nevertheless this still seems a surprising charge to bring against Fechnerian psychophysics. If the fundamental task of psychophysics is the discovery of the relationship between the hitherto unrelated body and mind, is it not astonishing that psychophysics should have confused the two, the two whose very separateness was the *raison d'être* of psychophysics? Yet the critics stuck to the point and were at pains to show the readiness with which these incommensurables did duty, the one for the other. Ward, pointing out that the psychophysical limen expressed in terms of stimulus was physical quantity, concluded: "There is no trespass harder to avoid than that across the lines dividing the subjective and objective aspects, and none more disastrous to the offender."[15] Other writers urged the same point, and Külpe even brought the prevalence of objectification into an experimental study.[16] It is no wonder then that objectification was thought of as a source of error and that Titchener coined for it the term "stimulus-error."

The Answer to the Quantity Objection

The fundamental and final answer to the quantity objection was Weber's law: $S=k \log R$. In so far as the relationship had been observed, no amount of explaining could explain it entirely away. It might be that the function was not exactly logarithmic or that it held only within certain limits; it might not be certain just what was the nature of S, or of R; but the unescapable fact was that there were an S and an R, which were covariant, and which were not identical since the mode of variation of the one was not the mode of variation of the other. To charge the stimulus-error and say that S was contaminated by R was not enough, since the confusion of S with R was not enough to explain the discovery of this difference in variation. The psychophysicists, therefore, had the stronger position, and had only to show where the difference actually lay. There seem to be five ways of accounting for the difference and thus of establishing psychophysics.

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[9] S. Exner, Hermann's *Handbuch der Physiologie,* 1879, II, ii, 242.

[10] F. Boas, *Pflüger's Arch.,* 1882, 28, 568f.

[11] J. Tannery in J. Delboeuf, *Eléments de psychophysique,* 1883, 138.

[12] F. A. Müller, *Das Axiom der Psychophysik,* 1882, 46-56.

[13] A. Meinong, *Z. f. Psychol.,* 1896, 11, 81-133, esp. 96ff.

[14] F. Brentano, *Psychologie vom empirischen Standpunkte,* 1874, I, 91.

[15] J. Ward, Mind, 1876, O. S. 1, 460.

[16] O. Külpe, Ueber die Objectivirung und Subjectivirung von Sinneseindrücken, *Philos. Stud.,* 1902, 19, 508-556.

1. Systematically one may argue for a physiological interpretation of Weber's law, as Müller did.[17] Excitation varies somewhat as does the logarithm of the stimulus. No one doubts that excitation may have magnitude, and thus the quantity objection is met. Moreover the logarithmic relation between physical dependents is not unknown.[18] Excitation, however, does not happen to be open to immediate observation, so we must observe its correlate sensation. We deal therefore with stimulus and sensation, which we must keep apart, avoiding the stimulus-error; and we escape from the formal objection that sensation does not have magnitude by making it a mere qualitative indicator of excitation which must have magnitude.

2. Wundt's psychological interpretation of Weber's law meets the quantity objection by the introduction of *Merklichkeitsgrade*. Sensations do not have magnitude, but if they did the matter would be irrelevant to psychophysics. It is apperception that gives a quantitative aspect to mind; there are degrees of noticeableness to sensations or to the differences between them.[19] The sensation scarlet is not more than the sensation pink, but is more noticeable than the pink; and the difference between a scarlet and a pale pink is more noticeable than the difference between the scarlet and a rose. To introspection it is just as obvious that apperception has degree as it is obvious that sensation has not, and it is between these

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[17] G. E. Müller, *Zur Grundlegung der Psychophysik*, 1878, 224-403.

[18] Müller, *loc. cit.;* Ward, *op. cit.*, 452-466; Titchener, *op. cit.*, II, ii, 66f. The autocatalytic theory is more recent: T. B. Robertson, *Monist*, 1909, 19, 372ff., 384f.

[19] Wundt's theory passed through successive stages and no brief statement does it justice. For summary and discussion, see Titchener, *Exper. Psychol.*, II, ii, pp. lviff., lxxivf., lxxxff., 69f.; for summary and genesis, p. lxxxii. note.

Merklichkeitsgrade and the physical values of the stimulus that the logarithmic law holds.

3. What was Fechner's answer to the quantity objection? To deny the stimulus-error, which the quantity objection implies. In the *Revision* he wrote:

> "One must take care not to try to count relations that exist in the physical realm between physical units as existing within the mental province because they can be grasped only by the mind; for in so doing one loses the ground for distinction between the two provinces. Even the physical, within which the relations exist, must be grasped by our minds in order to exist for us and to be spoken about. Yet we discriminate on the basis of this community between outer and inner phenomena as between two provinces, and have to distinguish properties as belonging in the one or the other, not merged with each other or interchangeable, as might sometimes seem to be the case under a philosophical point of view. At any rate psychophysics takes this point of view and thus avoids confusion. Whenever something like a relation, a change, a difference, a unity, or a fusion, *etc.*, appears as characterizing the physical or psychical world, it is abstracted from the province of the one or the other, or it is counted into the one or the other province; it may occur just as readily in psychology as in natural science. Hence it is perfectly possible that the pitches should be represented in one province by something in the other province without our identifying the two. The relations of periodicity between vibrations, which occur as the psychophysical representatives of melodic and harmonic sensations, are the most obvious; the one is something very different from the other."[20]

In other words it is all in our point of view. We can judge the stimuli or we can judge the sensations; and, according as we do the one or the other, we constitute for ourselves the physical or the mental world. It is nonsense to assume that, because we make judgments of physical phenomena in building up natural science, this natural science is a science of judgments and therefore mental. The two are distinct, and the discovery of the difference that is summarized by Weber's law attests the distinction.

To the writer of this paper it seems that Fechner's argument, turned a different way, becomes at least as invincible as any of the other ways out of the difficulty. The trick for escaping the force of the quantity objection, when directed against mental phenomena, is to turn it upon the stimuli themselves. Suppose sensations of weight do not under observation exhibit magnitude; what of the physical weights themselves? To physical observation ten grams is *a* weight and one gram is *a* weight; it is only in common sense, which is assuredly not physics, that ten grams is ten one-gram weights. Because physical phenomena, like mental, are referable to objects, is no excuse for reading the objects into them. Physical weight is as little the number of objects in the scale-pan as mental weight is the number of weights in the hand. The physical quantity is just as simple and unitary as the mental, and if sensation lacks magnitude so must stimulus. We can hardly, however, deny measurement to physics, and it thus appears that the quantity objection is not valid either against the measurement of sensation.[21]

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[20] Fechner, *Revision der Hauptpunkte der Psychophysik,* 1882, 5f.

4. Undoubtedly the most general way of meeting the quantity objection while saving mental measurement is by the substitution of the sense-distance for the sensation magnitude. Historically this conception dates from Delboeuf's *contraste sensible*. It is not necessarily incompatible with any of the foregoing accounts of mental measurement and is endorsed essentially by Wundt, Boas, Stumpf, Ebbinghaus, James, Meinong, Höfler, Stout, and G. E. Müller.[22] It is the basis of Titchener's quantitative psychology, where it finds its clearest exposition.[23]

This position holds that sensations, although they do not possess magnitude, may lie within a continuum, and that, although we can form no quantitative estimate of any sensation, we can nevertheless estimate the relative degree of separateness of two sensations within the continuum. Sensations are simply themselves and are not summed of various numbers of increments; the distances between these sensations, however, do vary and can be estimated in amount. The simplest case of mental measurement occurs when, for a series of three sensations, A, B, and C, occurring in a continuum, we estimate the sense-distance AB as equal to the sense-distance BC. Here we have measurement, for we have laid off the unit AB=BC twice in the distance AC, and it is the correlation of such estimated sense-distances with the corresponding values of stimulus that gives Weber's law.

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[21] The more thorough exposition would show that magnitude and measurement are systematic matters and are not found immediately at the observational level of science. The confusion is not unlike that of the systematic 'sensation' with the observational 'attribute:' *cf.* Sensation and system, *Am. J. Psychol.*, 1915, 26, 258-267, where Titchener makes this point. On the other hand, it is hardly fair to physics to say: "No sensation is a sum of sensation-parts or of sense-increments; no sensation is a measurable magnitude. Fechner has transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology:" Titchener, *Exper. Psychol.*, II, i, p. xxvii.

[22] *Cf.* Titchener, *op. cit.*, II, ii, p. cxxxiii.

[23] Titchener, *op. cit.*, II, i, pp. xxi-xxvii; ii, pp. cxvi-cxliv.

5. All the foregoing modes of meeting the quantity objection are successful without sacrificing the possibility of mental measurement; the fifth mode of defense consists in joining the enemy. We can give up the measurement of mind, substituting the measurement of sensitivity or of capacity-for-discrimination. Fullerton and Cattell give us our orientation here.

They declare, in the first place, that both sensation magnitudes and sense-distances are undiscoverable:

> "If an observer can, in fact, estimate quantitative amounts of difference in sensation, apart from association with known quantitative differences in the stimuli, a relation between mental and physical intensity can be determined. The writers, however, agree in finding that they cannot estimate such quantitative differences in sensation in a satisfactory manner. We can indeed say when one weight seems approximately double another, but this is doubtless because we have often lifted first one volume, and then two, and the like. But we cannot say when one sound seems twice as loud, or one day twice as hot as another. We have made experiments to see how nearly different observers would agree in adjusting one shade of light midway between two others, and have found hesitation in coming to a decision and great divergence of opinion. Most men will think that a just king is happier than a tyrant, but few will agree with Plato in considering him 729 times as happy."[24]

What is left? The observed *stimuli*[25] and errors of observation incurred in observing the stimuli.[26] There is no constant just noticeable difference nor threshold.[27] We have only errors of observation as we fail to observe an actual difference in the stimuli or, less often, observe a difference that is not there. These errors can be treated under the ordinary calculus of probabilities and follow the normal law of error.[28] The amount of the average error is always determinable and it increases with the magnitude of the stimulus.[29] It is the law of the dependence of the average error of observation upon the magnitude of the stimulus that Weber's law seeks to state, although the mathematical form of Weber's law is actually incorrect.[30]

Such a quantitative psychology of error is of necessity a psychology of capacity,[31]—of the capacity of the organism to respond correctly to stimuli. This point of view has since developed into the statistics of mental tests and of behaviorism, which is a psychology of stimulus and response. It is a point of view for which there is no stimulus-error since quantitative judgments can be made only of the stimulus, and it is one that touches other sciences very closely since it concerns itself with observation, the method of every science.[32]

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[24] G. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences,* 1892, 20.

[25] Fullerton and Cattell, *op. cit.,* 9ff., 20, 153, *etc.*

[26] Fullerton and Cattell, 14ff.; Cattell, *Am. J. Psychol.,* 1893, 5, 287ff.

[27] F and C, 11, 150; Cattell, 288f.

[28] F and C, 12ff.; Cattell, 285ff.

[29] F and C, 23ff., 153f.; Cattell, 290ff.

[30] F and C, 24ff., 152.

[31] *Cf.* Titchener, *Exper. Psychol.,* II, ii, p. cxxxiv note.

The Two Psychologies

Thus it becomes evident that the answers to the quantity objection have divided along the traditional cleft in psychology. We have not only a psychology of datum and a psychology of capacity, but we have quantitative psychologies of datum and of capacity.[33]

The quantitative psychology of datum ["the given"] insists upon a truly mental measurement. When further it correlates these mental measurements it is the true psychophysics. It may answer the quantity objection in any of the first four of the five foregoing ways, because any one of those four insists upon or explains the existence of mental quanta. It is a part of the larger psychology that is variously called structural or introspective, the psychology of process or of *Beschreibung.*

The quantitative psychology of capacity admits the quantity objection and denies—or at least ignores—mental quanta. This psychology sees no distinctively mental measurement, but undertakes the physical measurement of bodily response as a function of the physical quantities of the stimulus. There is no sharp epistemological line discernible between this sort of measurement and other physical measurement, and it thus meets the requirement of modern behaviorism that psychology interpenetrate physical science without sensible demarcation.[34] The psychology of capacity is also the psychology of mental tests[35] and of Urban's psychophysical experiments.[36] These latter seem strange mates, but the influence of Cattell is apparent in both. If the germ of the tests was in Galton, nevertheless it was Cattell, in the interests of the psychology of individual differences, who planted it in American soil where it has brought forth fruit abundantly. Urban's origin is less apparent. It is trivial to remark that he began his psychophysics in the laboratory that Cattell had founded years before. The evidence is internal and not explicit, but the seeker after information can find it.[37] In the broad, we may add, the psychology of capacity may become a functional psychology and deal, when it is experimental, with *meanings* and *Kundgaben* instead of mental processes and *Beschreibung.*

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[32] F and C, 9; Cattell, 285.

[33] *Cf.* O. Külpe, *Vorlesungen über Psychologie,* 1920, 5ff.; Titchener, *Am. J. Psychol.,* 1921, 32, 108-120.

[34] J. B. Watson, *Psychol. Rev.,* 1913, 20, 177; *J. Philos., etc.,* 1913, 10, 427; *Psychology,* 1920, vii.

[35] See note 31 above.

[36] The present writer has already had occasion to refer to F. M. Urban's position: E. G. Boring, *Am. J. Psychol.,* 1920, 31, 27f., esp. note 77; *Psychol. Rev.,* 1920, 27, 446f. It is well to repeat that no reference is intended here to Urban's later position, which involves an acceptance of the epistemology of Mach and Avenarius; see Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.,* 1913, 30, 113-152, esp. 113, 124f., with notes; *cf.* also *Am. J. Psychol.,* 1913, 24, 274.

As a term the " stimulus-error " is the property of the psychology of datum. It characterizes the attitude of the psychology of capacity as being concerned observationally with the stimulus and admonishes against it. Of course the psychology of capacity ignores this admonition, for in making observations of the physical stimulus it is simply fulfilling its self-appointed task. The " stimulus-error " is no valid charge against this psychology, nor does it raise within it a question of right and wrong. No more can be expected of either psychology than that it hold to its premises.

In practice there is a difficulty, however. No matter how distinct the two psychologies may be, psychologists can scarcely hold strictly to the one or to the other. The psychologist of the datum can not be expected to attack a new perception without recourse to judgments of stimulus or of meaning,[38] and statements of meanings moreover may themselves become the object of psychological investigation.[39] As to whether the psychology of capacity might remain wholly faithful to the stimulus it is not so easy to say. This attitude is more natural and it is possible to complete entire experiments without once trespassing upon the realm of mental process. On the other hand, the functional psychologies for all they have to say of the inadequacies of structural psychology, seem usually unable to complete the systematic mental picture alone, and a recent system of behaviorism has drawn unhesitatingly upon the psychology that it seeks to supplant.[40] In general what is fundamental to the one can not be ignored by the other, and on this account the writer of this paper would urge the attention of the psychologist of capacity to the "stimulus-error."

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[37] On psychophysical judgments and random events, compare Fullerton and Cattell, *op. cit.*, 12ff., 23ff., with Urban, *The Application of Statistical Methods to the Problems of Psychophysics,* 1908, 17f. On the meaning of the just perceptible difference, compare F and C, 11, and Cattell, *op. cit.*, 288f., with Urban, *Stat. Meth.*, 70. On mental measurement compare F and C, 20, 152f., and Cattell, 293, with Urban's ignoring of the issue in *Stat. Meth.* [Or could he have thought that Titchener had settled it for the sense-distance? *Cf. Psychol. Bull.*, 1912, 9, 245.] On judgment as directed upon stimulus, compare F and C, 20, and Cattell, 293, with Urban, *Stat. Meth.*, 5, 17, *Psychol. Rev.*, 1910, 17, 27ff. On recording degrees of assurance, compare F and C, 11, 151, with Urban, *Stat. Meth.*, 5ff. On the relation of psychophysics to physics, compare F and C, 151, Cattell, 285, with Urban, *Psychol. Rev.*, 17, 243f.

[38] *Cf., e.g.,* L. B. Hoisington, On the non-visual perception of the length of lifted rods, *Am. J. Psychol.*, 1920, 31, 114-146.

[39] *Cf., e.g.,* H. P. Weld, Meaning and process as distinguished by the reaction method, *Titchener Commemorative Volume,* 1917, 181-208.

The Effect of the Stimulus-Error

If we are now to urge upon the psychology of capacity the avoidance of the stimulus-error, it is a fair demand that we state first the probable penalty that is incurred by a failure to accept our advice. Here we can not stand upon the epistemological ground that psychology observes mental processes and not stimuli, and that judgments of stimulus are therefore *a priori* inadmissible. This historical warning against the stimulus-error does not apply to the psychology of capacity which protests against a scientific dualism and deals by preference with stimulus and response. What we have to show is rather that the stimulus-error works against the establishment of the univocal correlations between stimulus and response that a psychology of capacity demands, that it interferes with the prediction of the response for a given stimulus. Here the ground is broadly scientific: we are dealing with the constancy of experimental conditions and the reproducibility of results.

When we go frankly to the literature, however, asking just what in numerical terms may be the effect in mental measurement of allowing judgments of the stimulus or of instituting them, we meet at first disappointment.

The psychology of datum is set to avoid, rather than to measure, the stimulus-error. It tells us where the stimulus-error is most insidious, *viz.*, in judgments of supraliminal sense distances.[41] We may have trouble with the limens; we are almost sure to have it in comparing large sense-distances. Sometimes a special technique is necessary to avoid the error. In Martius' experiment on the apparent size of objects at different distances from the eye, all the stimulus habits for the estimation of the size of objects in everyday life are appealed to, and a special method is required "in removing the initial tendency of the observers to reflect on the actual size of the comparison rod in relation to the standard rod."[42] In Angell's experiment on intensive distances between sounds one would expect concrete reference to the stimulus to enter much less readily; on the contrary, however, the observers tend to judge the height of fall or the angle of fall of the stimulus, and to neglect the mental datum.[43] For this error special remedies are presented. Müller urges that the amount of intensive difference be taken as the *Kohärenzgrad,* the *Leichtigkeit des Kollectivaufgefasstwerdens* of the two sounds.[44] Titchener suggests letting the observer blunder into the stimulus-error and then rescuing him by individualized treatment, his protocols in hand.[45] These experiments are striking cases, but even the psychologists who have no special measures of reform to offer cry out against the evils of judgments based upon secondary criteria, upon associates of the processes judged, or upon surrogate processes.[46] A surrogate, they complain, can even render incommensurables artificially commensurate, as seems to be the case when the intensities of weights and noises are compared in terms of *Spannungsempfindungen.*[47]

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[40] Watson, *Psychology, from the Standpoint of a Behaviorist,* 1919.

[41] *Cf., e.g.,* Titchener, *Textbook,* 218.

Now it is not likely that there could be so much smoke without some fire; yet we are still at a loss to estimate the amount of danger. It would be reasonable for these psychologists of datum to seek to avoid stimulus for no other reason than that they are interested solely in "mind," but it is not to be supposed that the matter would have been taken so seriously had the quantitative results, and Weber's law which is dependent upon them, been unaffected by the kind of judgment given. Indeed this belief came to the surface when Grotenfelt, in defense of Weber's law, accused Merkel of the stimulus-error and inclined to the belief that results that followed Merkel's law might possibly indicate that they were based upon judgments of stimulus.[48] The data that we wish, however,—the comparison in quantitative terms of results of judgments of process—are, in the earlier history of psychophysics, lacking.

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[42] G. Martius, Ueber die scheinbare Grösse der Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder, *Philos. Stud.,* 1889, 5, 601-617, esp. 605f.; *cf.* Titchener, *Exper. Psychol.* II, ii, pp. 262f.

[43] F. Angell, Untersuchungen über die Schätzung von Schallintensitäten nach der Methode der mittleren Abstufungen, *Philos. Stud.,* 1891, 7, 414-468, esp. 438.

[44] G. E. Müller, *Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik,* 1904, 237f.

[45] Titchener, *Exper. Psychol.,* II, ii, p. 198; *cf.* also pp. 203f., 230.

[46] *Cf.* Fechner, *Elemente,* II, 318ff.; H. Neiglick, *Philos. Stud.,* 1888, 4, 41; Angell, *op. cit.,* 438; W. Ament, *ibid.,* 1900, 16, 173; G. E. Müller, *op. cit.,* 241; J. Fröbes, *Z. f. Psychol.,* 1904, 36, 259.

[47] H. Münsterberg, *Beiträge zur experimentellen Psychologie,* III, 1900, 56-122, esp. 98ff.

(The psychologists of capacity, we may note in passing, are not to be asked for this comparison. Cattell and Fullerton denied the possibility of mental measurement. They can not therefore be asked for its comparison with any form of physical measurement.)

On the basis of recent literature, however, there is something to be said, in answering this question, for the case of lifted weights, and very much to be said for the case of the limen of dual cutaneous impression. We may mention the lifted weights at once and reserve the two-point limen for the next section.

Friedländer undertook a comparative study of lifting weights under different *Einstellungen*.[49] He employed a "*G-Einstellung*" in which the attention was directed upon the lifted object (*Gegenstand,* hence *"G"*), and an *"A-Einstellung"* in which the object was abstracted from (hence *"A"*) and the attention directed upon the sensory aspect of the experience (*Druck-, Spannungs-, Kraftempfindungen*).[50] Here we should expect to find the results we are seeking, for the *G-Einstellung* is the stimulus-attitude, the attitude demanded by a psychology of capacity and called the "stimulus-error" by the psychology of datum; and the *A-Einstellung* in its various forms is the process-attitude which avoids the "stimulus-error." There is not the least doubt that the two attitudes give different results. "Differential sensitivity on the whole is somewhat finer for the G-series" for a standard weight of 500 g., Friedländer tells us, but a standard of 1200 g. may give a finer discrimination under the *A-Einstellung*. The data unfortunately are for one observer only. They are based on too few cases,—41 series after the practice-effect was presumably constant. The observer did not always succeed in maintaining the required attitude, for the stimulus-attitude was difficult for him (!) and he sought to make his finer discriminations under the *A-Einstellung*. The resultant psychometric functions are not smooth ogives; one just barely misses inversion in its central portion.[51] We are not yet ready, then, to generalize as to the exact effect of attending to the stimulus in making psychophysical judgments. All we can say is that there is an effect, that a shift in the observational attitude alters the numerical results significantly. We shall not dare, therefore, if we wish to predict response from stimulus, to leave attitude out of account.

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[48] A. Grotenfelt, *Das Webersche Gesetz und die psychische Relativität,* 1888, 111f.; *cf.* Titchener, *Exper. Psychol.*, II, ii, pp. lxxviiif., 219.

[49] H. Friedländer, Die Wahrnehmung der Schwere, *Z. f. Psychol.,* 1920, 83, 129-210, esp. 187-193.

[50] Pp. 133ff.

My colleague, Professor Fernberger, has recently completed similar experiments in the Clark Laboratory. He had three observers, and extended his series through many fractions to take account of progressive practice and to give an adequate number of cases. His resultant ogives are smooth and regular, and his procedure seemed calculated to yield all that could be desired methodically. He gets differences for the different attitudes,—more striking differences in some cases than Friedländer's. More than this I can not say in advance of the publication of his results. Perhaps in his final analysis he will discover a generalization, which is not apparent to casual inspection of the functions and constants. The results indicate unquestionably that an alteration of attitude by instruction may result in an alteration of the psychometric functions, which is significant in the mathematical sense of being many times its probable error, but which for a given observer is unfortunately quite unpredictable. Attitude may be very important even when we can not say just why.

Fortunately the case of the two-point limen is less mysterious.

The Stimulus-Error as Equivocal Correlation

The limen of dual impression upon the skin furnishes the case for which we are looking. We know, not only that judgments of stimulus may here make a difference in the quantitative results, but we know further how great this difference may be and something of its conditions. We are in a position, moreover, to generalize from these facts with some assurance and to assert that the effect of the " stimulus-error,"

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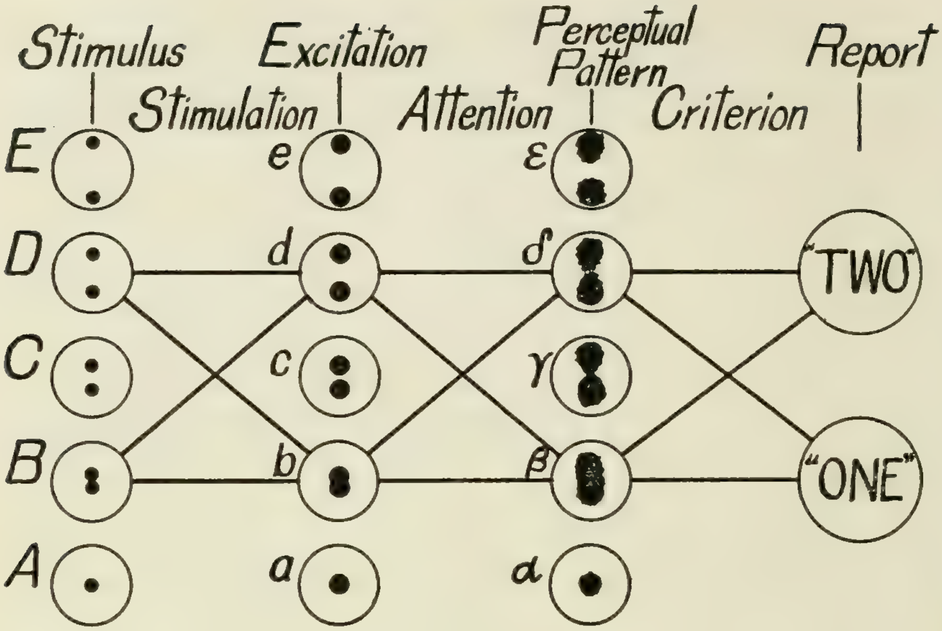
[51] In fact it is not even clear that Friedländer's cautious generalization is not in part an artifact. If we compute the data of Table 10 by Urban's procedure for the *Konstanzmethode*, we get:

	A-Einstellung	G-Einstellung
Av. measure of precision (h)	0.0138	0.0147
Interval of uncertainty (grams)	35.39	37.13
Point of subjective equality (grams)	500.8	509.9

There is not much difference in precision or discrimination by this method. The striking difference is in the effect of attitude on the point of subjective equality.



from the point of view of a psychology of capacity, is—under similar conditions, at least—to render the correlations between stimulus and response equivocal and thus to jeopardize the rigor of conclusion that science demands.

We may proceed to the point by reference to the visual schema of the figure reproduced herewith. The diagram is intended merely to assist in the analysis of the factors involved and not as an actual picture of neural or psychophysical fact.

One deals in determining the two-point limen with a series of stimuli, *A, B, C, D, E,* pairs of stimulus-points at different separations, with perhaps a single point, *A,* at the extreme of the series. From the work of numerous investigators[52] we know that there is a similar series of perceptual patterns, α, β, γ, δ, ϵ, which passes, with approximate regularity, from a sharp point to a blunt point, to an oval, to an elongated oval, to a double-paddle, to a dumb-bell, to two separated points. We may best think of these perceptual patterns as the process material of the psychology of datum, but they exist also for the psychology of capacity as inscrutable "middle terms." Intermediate between stimulus and process we are accustomed to assume some sort of excitatory process, *a, b, c, d, e.* Of these there must also be a series, and knowing little about them, we may picture them as somewhat like the perceptual processes. Finally there is the verbal report which is generally limited experimentally to the words *One* and *Two.* It seems, moreover, that we keep within the limits of scientific good sense if we say that stimulus, excitation, mental process, and report form a dependent series, and that each is the resultant of the preceding.

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[52] G. A. Tawney, *Psychol. Rev.,* 1895, 2, 585-593; V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes,* 1898, 6; M. Foucalt, *L'illusion paradoxale et le seuil de Weber,* 1910, 122-145; E. J. Gates, *Am. J. Psychol.,* 1915, 26, 152-157; and numerous other references cited in these articles.

If now we choose for special consideration the stimuli *B* and *D,* we may note that they most frequently, perhaps, give rise to the dependent series '*B-b-β-One*' and '*D-d-δ-Two,*' but we must note further that cross-connections are possible.

Stimulus gives rise to excitation by way of stimulation. Here variation may tend either in the direction of fusion or of separation. On the forearm, for example, the angular shifting of stimulus toward the longitudinal axis may result in fusion and *D* may give rise to *b;* if the rotation is toward the transverse axis, *d* may follow upon *B.*[53] Location as well as angular orientation also yields these differences. Near the elbow *D* gives *b,* say; a little further down *B* may give *d.*[54] It is also possible that the chance impingement of the stimulus-points upon multiply innervated spots in the skin gives rise to multiple excitation (*B* to *d*) and that the stimulation of less complexly innervated spots may lead to a simpler excitation (*D* to *b*).[55]

At the next phase of our series excitation culminates in perceptual pattern under the selective action of attention. These terms sound strange of course to the psychologist of capacity, but we may ask him to accept them or to find substitutes for them. They stand for factors that affect the correlations with which he is working, and he must take scientific account of them, whatever he calls them.

It is experimentally demonstrable that under inattention potential *Twos* become *Ones;* one stimulating point or the other catches the attention and thus withdraws it, as it were, from the other.[56] The converse occurs in the well-known *Vexirfehler,* the paradoxical judgment, where two points close together or even a single point give a perception of duality. Henri and Tawney referred this phenomenon in part to attention, and its dependence upon certain suggestive factors seems to indicate that it is of this order.[57] Griesbach and others supposed that fatigue reduced the limen,[58] but Friedline has shown that the effect of fatigue is operative only among the perceptual forms at the lower end of the series, say α and β. Here fatigue acts in the direction of fusion and may perhaps be another factor that operates within the attentive phase.[59] Thus the perceptual pattern is not wholly dependent upon conditions at the periphery, the mode and place and nature of the stimulation. Certain more central factors also come into play and justify us in adding to the cross-connections B-d and D-b the connections b-δ and d-β.

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[53] *Cf.* Tawney, *Philos. Stud.,* 1897, 13, 170; *cf.* Boring, *Quart. J. Exp. Physiol.,* 1916, 10, 23.

[54] *Cf.* Henri, *op. cit.,* 26f.; *cf.* Boring, *loc. cit.*

[55] Henri, *op. cit.,* 64ff.; Boring, *op. cit.,* 89-93, esp. 93.

[56] *Cf.* A. Brückner, *Z. f. Psychol.,* 1901, 26, 54f., 60; Boring, *op. cit.,* 88f.

Finally now the perceptual pattern issues in a judgment in accordance with criteria of judgment that have been established. The available factor here lies in the setting up and the preservation of these criteria. Left to himself there is no guarantee of what the subject will do, as the studies of Titchener,[60] deLaski,[61] and Friedline[62] all show. Perhaps the most normal criteria for sophisticated adult subjects would be such that the perceptual δ of our chart would lead to the judgment *Two* and the perceptual β to the judgment *One*. It is a reasonable inference that McDougall's savages in Torres Straits, under the competitive incentive to do well, assumed such criteria that even our β would have led to the judgment *Two* for plainly β is larger than the pattern which a single point most often gives.[63] An overzealous subject can interpret perceptual patterns lying very low in the series as meaning the presence of two points upon the skin; in fact, as Friedline has shown, he can do amazingly well under the influence of both practice and incentive, provided he is not fatigued.[64] On the other hand the sophisticated subjects of the laboratory, and, it may be, therefore the Englishmen, whom McDougall compared with the savages, tend to define *Two* by reference to a perceptual pattern higher in the scale.[65] We should hardly have been bold enough to have predicted this result; to the writer, however, it seems reasonable enough now that it has been pointed out. Is it not to be expected that the savage would try to " do well " by discriminating as finely as possible and that the sophisticated person would try to " do well " by discriminating as accurately and consistently as possible, though less finely? In any case the point is that the acceptance of a criterion is an unavoidable experimental condition in determinations of cutaneous spatial sensitivity, and that the criterion must therefore be controlled, since when uncontrolled it gives uncertain significance to the verbal responses *Two* and *One*. In fact it appears that the apparent limen for cutaneous duality may be very much more than quartered by a variation of criterion, and it seems further that the conflicting results in the literature with respect to the effect of fatigue and the effect of practice upon the limen are to be explained in this way.

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[57] Henri and Tawney, *Philos. Stud.*, 1895, 11, 394-405, esp. 403ff.; Tawney, *ibid.*, 1897, 13, 186-198; Henri, *Raumwahrnehmungen*, 61-66.

[58] H. Griesbach, *Arch. f. Hygiene*, 1895, 24, 124ff.; but see the summary in C. L. Friedline, The discrimination of cutaneous patterns below the two-point limen, *Am. J. Psychol.*, 1918, 29, 415-418.

[59] Friedline, *op. cit.*, 411f., 418f.

[60] Titchener, On ethnological tests of sensation, *etc.*, *Proc. Am. Philos. Soc.*, 1916, 55, 206-215.

[61] E. deLaski, On perceptive forms below the level of the two-point limen, *Am. J. Psychol.*, 1916, 27, 569-571.

[62] Friedline, *op. cit.*, 405-415, esp. 405f., 408f., 411ff.

[63] W. McDougall, *Rep. Cambridge Anthropol. Expedition to Torres Straits*, 1903, II, 189-193; *cf.* Titchener, *op. cit.*

[64] Friedline, *op. cit.*, 408f., 414f.

This leads to a conclusion. If only the end-terms of stimulus and response are controlled a univocal one-to-one correlation between stimulus and response is not possible. In the terms of the diagram, both the stimuli *D* and *B* may condition the response *Two*. There are eight paths leading to *Two*, four from *D* and four from *B*. If we consider the other factors of the schema, the situation is enormously complicated. There are over a hundred modes of connection from *A, B, C, D,* and *E.* to *Two*. Certainly the actual possibilities must be legion. At this level of work the best we can do is to remain in the dark and to deal with relative frequencies, yet relative frequencies do not yield the predictive correlation that science demands. The only way to get out of the dark would be to study the effect of stimulation, of attention, and of criterion by taking hold of these dependent series at their intermediate points, thus providing ourselves with a more complete knowledge and control of the entire psychophysical situation. Now the psychologist of capacity habitually controls stimulation, the various adjustments of the stimulus to the sense-organ, but the psychologist of datum also controls by instruction both attention and criterion. He does not, to be sure, reach the ideal of 100% certainty in the prediction of the response to a given stimulus, but he is able greatly to increase the precision of these stimulus-and-response correlations. On the other hand, the failure to control the attitudinal factor implied in the acceptance of a criterion, and the attentional factor, again and again results perforce in an equivocal determination of these responses, which is nothing more nor less than a "stimulus-error."

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[65] McDougall, *op. cit.*, 192 and note; Titchener, *op. cit.*, 211.

In the psychology of capacity, then, the danger of the "stimulus-error" reduces to the danger that judgments of stimulus will prove scientifically equivocal. Experimentation will show when they are and when they are not. When they are equivocal, the problem is soluble by refinement in the control of conditions. The modern technique for the control of attention and attitude is a method that satisfies scientific standards of accuracy of prediction better than any available substitute. To shut our eyes to this technique in the absence of a substitute would be to refuse to accept scientific methods that have already yielded practical results. Certainly if the psychologist of capacity is to be a successful experimenter, joining hands with the biologist and physicist, he must in some way take account of all the means of experimental control that have been demonstrated as essential to the securing of accuracy.

Summary

Scientific psychology in its inception assumed a distinction between mind and matter and the separate existence of observable mental data and observable physical data. Fechner's psychophysics sought to measure the mental data and to establish their correlation with related physical data. The opponents of this point of view raised the *quantity objection,* arguing that mind is not possessed of magnitude and is therefore not measurable. Most of these objectors were attacking only the quantitative status of psychology and seeking to establish it as an essentially qualitative, but mental, science. Other objectors preferred, however, to keep psychology quantitative by conceiving of it as physical, as the psychology of the *capacity* of the organism for response to stimulus. The older psychology met the quantity objection by showing that the nonexistence of mental magnitude does not preclude mental measurement, and then sought to protect itself against incursions of the physical observational attitude of the psychology of capacity by styling that attitude the *stimulus-error.* The implication would be that a psychology of capacity does not need to avoid the stimulus-error, but rather should cultivate it. The thesis of this paper is, however, that recent researches have shown that the observational attitude which is directed upon the stimulus—the attitude of the stimulus-error—may sometimes lead to equivocal correlations of stimulus and response which, because equivocal, are unscientific. In the case of the limen of dual impression upon the skin, for example, a psychology of capacity must make use of introspective data if it is to attain its own ideals.

THE ILLUSORY PERCEPTION OF MOVEMENT ON THE SKIN[1]

By Anna Kellman Whitchurch

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The object of the present investigation was to determine the elementary conditions of the perception of cutaneous movement. Wertheimer[2] and his successors have found such conditions for the perception of movement in the field of vision; and Dimmick[3] has shown a specific existential correlate of the perception of visual movement, namely, the grey flash. Our problem, then, is to find out whether the perception of cutaneous movement can be aroused by successive stimulation, and in that case what is its existential correlate.

Benussi and others[4] have already done much work upon the arousal of cutaneous movement by successive stimuli operating at long distances apart. Benussi stimulated at distances ranging from 4 cm. to the distance between the fingers on the two outstretched arms. Burtt gives reports from stimulations 8 to 16 cm. apart, and his apparatus allowed distances from 2 to 18 cm. These distances, however, favor the arousal of visual associations; and minimal distances are for that and for other reasons to be preferred. Incidental reports by von Frey and Metzner,[5] apparently overlooked by later experimenters, prove that such distances may be employed.

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[1] From the Psychological Laboratory of Cornell University.

[2] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych.*, 61, 1912, 161ff.

[3] F. L. Dimmick, An Experimental Study of Visual Movement and the Phi Phenomenon, this Journal, 31, 1920, 317ff.

[4] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. ü. d. VI Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59. *Cf.* also H. E. Burtt, Tactual Illusions of Movement, *Jour. Exp. Psych.*, 2, 1917, 371.

Von Frey and Metzner have given the only reports of work on the skin directly comparable with that of Wertheimer in vision. They were trying to find the best interval in which to distinguish as different the responses of two adjacent pressure spots successively stimulated. They used as stimuli hairs or blunt needles which rested on the skin for 1/9 sec. The intervals between stimulations were 1/9, 1/6, 1/3, 1, 4/3, and 2 sec. Near the end of their paper this statement appears: "When two points are stimulated, the difference of the two stimuli is either recognized as such, or (especially with the shorter intervals) is interpreted as a shift (*Verschiebung*) of the stimulus, as a brushing (*Streichen*) of it over the skin." The only numerical data given show that, with an interval of 4/3 second, 13 out of 29 stimulations aroused, in some form, the perception of movement.

Experimental Procedure

The present work was begun in the summer of 1920. The four *O*'s who served were: Miss C. Braddock (B), graduate scholar in psychology; Dr. K. M. Dallenbach (D), assistant professor of psychology; Dr. L. B. Hoisington (H), assistant professor of psychology; Mrs. A. K. Whitchurch (W), graduate scholar in psychology.[6]

The skin on the left fore-arm of the *O*'s was tested with von Frey hairs of various tension-values until a hair was found which could be felt upon the normal skin, but was too weak to arouse subcutaneous sensations when the skin was lightly etherized. The tension-value of the hair finally selected was 7.9 gr./mm. Two hairs were cut to this standard. They measured in length 40 and 42 mm. respectively.

Five groups of pressure-spots, ranging from 6 to 9 in number, on every *O*'s arm were isolated and stained, so that the same spots could be tested every day. During this preliminary testing one spot was stimulated only once in an hour. The stimulating hair was held 1 mm. above the pressure-spot, and after the "Ready, now" signal was touched to the spot. The *O*'s were asked to report on the intensity, quality, and extensity of the sensation. Two reports were taken when the spots were stimulated singly, and two when one spot was compared with another. The introspections were worked over, and the spots most alike were selected and again reported upon, until the first numbers chosen narrowed down to 3 or 4 in a group. Finally we were able to select two adjacent spots in every group which the *O*'s reported as like the spots in every other group. The selected spots were then permanently tattooed with a tiny glass tube just under the hair. The mark was distinct and lasting. The distances between the spots in the various groups were as follows (in mm.): B (1) 2.5, (2) 3, (3) 2.5, (4) 3, (5) 4; D (1) 5, (2) 3.6, (3) 3.2, (4) 3.8, (5) 3; H (1) 4.7, (2) 3.4, (3) 4.7, (4) 3.1, (5) 2.4; and W (1) 2.8, (2) 5.5, (3) 3.2, (4) 2.1, (5) 3.8.

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[5] M. von Frey and R. Metzner, Die Raumschwelle der Haut bei Successivreizung, *Zeit. f. Psych.*, 29 1902, 161ff.

[6] When W observed, B acted as experimenter.

Manual Experiments

O sat at a low table with his arm in a plaster cast. The adjacent hairs in the five groups were stimulated in turn. The duration of the stimulus was $250 \pm 25^{\sigma}$. The intervals, marked by a silent pendulum, were 750, 1000, and 1250^{σ}.

O worked under two sets of instructions. The 'process' instruction was: "I shall stimulate the skin on your arm. Describe the cutaneous perception aroused in purely psychological terms. The stimulus will be repeated so that you may complete your description." The 'meaning' instruction was: "I shall stimulate the skin on your arm. Characterize the cutaneous perception aroused, fully, in any commonsense terms you wish to use. The stimulus will be repeated so that you may complete your report."

The manual experimenting continued until every *O* had given 50 reports under the 'meaning' and 50 under the 'process' instruction with the 750 and the 1250 intervals. With the 1000 interval, every *O* made 100 reports under each set.

Results of Manual Experiments

The reports fell under headings like those which have been obtained in the work on vision: (1) full movement, in which the stimulus moved from the 1st to the 2nd spot stimulated; (2) bimembral movement, in which the 1st and 2nd stimulus each moved; (3) unimembral movement, in which either the 1st or the 2nd stimulus moved; (4) intramembral movement, in which the stimulus at one or both spots moved within the area of stimulation; (5) pure phi, in which the movement had no qualitative, intensive, or extensive beginning and ending, but was a uniform 'brushing' over the cutaneous surface.

Table A sums up the results of this preliminary work. The columns headed M show the percentage of reports under the 'meaning' instruction; those headed P the percentage of reports under the 'process' instruction. The intervals are given in sigma.

The reports under the two instructions are closely parallel. Long before the completion of the series the *O*'s had settled into a stable manner of reporting.

The 750 interval is the best of the three for arousing the perception of full movement. Even with this interval, however, the percentages are very low. Since optimal conditions had not been found, and since the lowest interval was the best, we began a new series of experiments with shorter intervals. Henceforth we applied the stimuli by means of apparatus.

Mechanical Stimulation: Series I

The apparatus was an adaptation of Benussi's kinohapt.[7] The intervals and durations of the stimuli were controlled by a Leipsic time-sense apparatus.

Both cutaneous and 'subcutaneous'[8] stimuli were employed. The cutaneous stimuli were two horse-hairs whose tension-value was 4.85 gr./mm., and which measured in length 10 and 9.8 mm. respectively. The subcutaneous stimuli were coarser hairs whose tension-value was 10.25 gr./mm.; they measured 3 and 2.85 cm. respectively. The duration of the stimuli was 150σ. The intervals of this series were 100, 250, 500, 750, and 1000σ. The adjacent spots in two groups on every *O*'s arm were now stimulated exclusively.

The cutaneous (C) and subcutaneous (S) stimuli were used alternately as follows: (1) C 100, 12 M-reports on spots *a-b*, 12 P-reports on spots *a′-b′;* (2) S 250, 12 M-reports on spots *a-b*, 12 P-reports on spots *a′-b′;* (3) C 500, 12 M-reports on spots *a-b*, 12 P-reports on spots *a′-b′;* and so forth. At the end of the ten groups which this order necessitated, the experiments were continued by reversing the order. Thus after no. 10 came (11) S 100, 12 P-reports on spots *a-b*, 12 M-reports on spots *a′-b′*. The practice-effect was thus evenly

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[7] For a description of the kinohapt, see V. Benussi, *Arch. f. d. ges. Psych.*, 29, 1913, 385.

[8] We use this shorthand term to designate the stimuli which affected both cutaneous and subcutaneous end-organs.

TABLE A

Manual Experiments: Percentages

		750		1000		1250	
		M	P	M	P	M	P
Full Movement	B	18	14	18	19	4	2
	D	4	4	10	16	14	2
	H	48	30	27	34	0	0
	W	14	24	23	24	2	2
	Av.	21	18	19.5	23.3	5	1.5
	M.V.	13.5	9	5.5	5.8	4.5	.8
Bimembral Movement	B	4	2	5	0	2	2
	D	0	0	0	0	0	6
	H	4	0	10	8	2	0
	W	2	10	13	15	6	10
	Av.	2.5	3	7	5.8	2.5	4.5
	M.V.	1.5	3.5	4.5	5.8	1.8	3.5
Unimembral Movement	B	12	26	9	21	42	38
	D	12	8	26	11	30	20
	H	34	54	32	34	48	42
	W	56	38	38	30	36	48
	Av.	28.5	31.5	26.3	24	39	37
	M.V.	16.5	14.5	8.8	8	6	8.5
Intramembral Movement	B	22	12	17	7	12	12
	D	14	0	3	6	0	4
	H	2	0	13	4	20	14
	W	10	6	8	6	22	6
	Av.	12	4.5	10.3	5.8	13.5	9
	M.V.	6	4.5	4.8	.9	7.5	4
Phi	B	0	2	1	1	4	0
	D	6	0	10	1	2	2
	H	2	2	0	1	0	0
	W	12	10	3	6	0	2
	Av.	5	3.5	3.5	2.3	1.5	1
	M.V.	4	3.3	3.3	1.9	1.5	1
No Movement	B	44	44	50	52	36	46
	D	64	88	51	66	54	66
	H	10	14	18	19	30	44
	W	6	12	15	19	34	32
	Av.	31	39.5	33.5	39	38.5	47
	M.V.	23	26.5	17	20	7.8	9.5

distributed. Several minutes elapsed betwen the stimulations of the same spot. At the end of every 12 reports *O* took his arm from the cast for a time, in order that there might be no fatigue. Usually *O* made reports on one group of experiments in an hour, and never on more than two. Every *O* made 24 reports under the 'meaning' and 24 under the 'process' instruction for every one of the intervals employed.

Results of Series I

The results of the first series with mechanical stimulation are summarised in Tables B I and B II. The columns M and P show the number of reports of the different types of phenomena given in each group of 24 experiments.

With both cutaneous and subcutaneous stimuli the 100σ interval gives the greatest number of full movements. Next to the full movement in frequency comes the unimembral movement. The number of full movements is too small to indicate optimal conditions, and another series was accordingly planned.

The 'process' and 'meaning' reports run closely parallel, as is shown by the averages of the groups.

TABLE B—I

Cutaneous Mechanical Stimulation: Series I: Absolute Numbers

		100		250		500		750		1000	
		M	P	M	P	M	P	M	P	M	P
Full Movement	B	9	6	3	4	0	0				
	D	8	10	3	0	0	0				
	H	6	8	7	5	1	0				
	W	13	14	0	0	0	0				
	Av.	9	9.5	3.3	2.3	.3					
	M.V.	2	2.3	1.9	2.3	.4					
Bimembral Movement	B	0	0	0	1	0	3	2	0	1	0
	D	0	0	0	0	0	0	0	0	2	2
	H	7	5	6	6	9	7	0	0	0	0
	W	8	5	5	7	8	8	2	1	0	1
	Av.	3.8	2.5	2.8	3.5	4.3	4.5	1	.3	.8	.8
	M.V.	3.8	2.5	2.8	3	4.3	3	1	.4	.8	.8
Unimembral Movement	B	5	8	6	4	10	8	5	6	4	7
	D	12	12	14	9	5	1	4	3	3	0
	H	10	11	10	11	8	9	9	10	9	8
	W	3	5	13	15	14	14	7	8	7	7
	Av.	7.5	9	10.8	9.8	9.3	8	6.3	6.8	5.8	5.5
	M.V.	3.5	2.5	2.8	3.3	2.8	3.5	1.8	2.3	2.2	2.8
Intramembral Movement	B			1		3	5	13	7	9	7
	D			5		0	0	0	0	1	1
	H			0		4	2	5	3	5	5
	W			2		0	0	3	2	3	2
	Av.			2		1.8	1.8	5.3	3	4.5	3.8
	M.V.			1.5		1.8	1.8	3.8	2	2.5	2.3
No Movement	B	9	8	14	15	11	8	4	11	10	10
	D	3	2	2	15	19	23	20	21	18	21
	H	1	0	1	2	2	6	10	11	10	11
	W	0	0	4	2	2	2	12	13	14	14
	Av.	3.3	2.5	5.3	8.5	8.5	9.8	11.5	14	13	14
	M.V.	2.9	2.8	4.8	7.7	6.6	6.6	4.5	3.5	3	3.5

With the 100 interval, B reported phi in 1 M and 2 P; D in 1 M.

TABLE B—II

Subcutaneous Mechanical Stimulation: Series I: Absolute Numbers

		100		250		500		750		1000	
		M	P	M	P	M	P	M	P	M	P
Full Movement	B	10	13	2	3						
	D	14	10	18	11						
	H	15	13	7	7						
	W	15	15	11	10						
	Av.	13.5	12.8	9.5	7.8						
	M.V.	1.8	1.4	5	2.8						
Bimembral Movement	B	0	1	0	0	3	4	2	0	1	0
	D	2	3	6	8	2	0	2	0	0	0
	H	4	0	4	5	0	1	1	1	0	0
	W	4	6	3	5	6	9	4	2	0	0
	Av.	2.5	2.5	3.3	4.5	2.8	3.5	2.3	.8	.3	
	M.V.	1.5	2	1.8	2.3	1.8	3	.9	.8	.4	
Unimembral Movement	B	5	3	12	11	7	4	8	9	0	4
	D	2	4	0	1	12	20	7	6	2	0
	H	4	8	10	10	10	9	8	11	9	8
	W	5	3	10	8	9	7	13	15	7	4
	Av.	4	4.5	8	7.5	9.5	10	9	10.3	4.5	4
	M.V.	1	1.8	3.3	3.5	1.5	5	2	2.8	3.5	2
Intramembral Movement	B	0	0	0	0	2	3	6	5	3	4
	D	0	2	0	0	2	0	0	0	1	5
	H	0	0	2	1	5	8	5	4	4	5
	W	0	0	0	0	2	1	2	2	12	13
	Av.		.5	.5	.3	2.8	3	3.3	2.8	5	6.8
	M.V.		.8	.8	.8	1	2.3	2.3	1.8	3.5	3.2
No Movement	B	9	7	10	10	12	13	8	10	20	16
	D	5	5	0	4	8	4	15	18	21	19
	H	1	3	1	1	9	6	10	8	11	11
	W	0	0	0	1	7	7	5	5	5	7
	Av.	3.8	3.8	2.8	4	9	7.5	9.5	10.3	14.3	13.3
	M.V.	3.3	2.3	3.6	3	1.5	2.8	3	3.9	6.3	4.3

With the 100 interval, D reported phi in 1 M.

Series II

The intervals chosen for the next series were 25, 50, 75, and 100σ. The arrangement of the groups and the procedure were as before.

The results are summarised in Tables C I and C II. The 100σ interval gives the greatest number of full movements with cutaneous stimulation, and the 75σ interval with subcutaneous stimulation. Again the 'process' and 'meaning' reports parallel each other.

A group of 24 experiments with each stimulus in each of the intervals 125 and 150σ was made as a part of this series with *O*'s H and W. Full movements were reported in the following cases: C 125, H 9M and 8P, W 8M and 6P; C 150, H 5M and 4P, W 4M and 4P; S 125, H 6M and 4P, W 5M and 3P; S 150, H 6M and 6P, W 5M and 4P.

TABLE C—I

CUTANEOUS MECHANICAL STIMULATION: SERIES II: ABSOLUTE NUMBERS

		25		50		75		100	
		M	P	M	P	M	P	M	P
Full Movement	B	0	0	19	23	17	16	14	19
	D	5	1	9	6	7	8	11	9
	H	2	6	10	4	11	11	14	15
	W	0	0	10	13	14	15	16	14
	Av.	1.8	1.8	12	11.5	12.3	12.5	13.8	14.3
	M.V.	1.8	2.1	3.5	6.5	3.3	3	1.4	2.8
Bimembral Movement	B			0	0	0	0	0	0
	D			0	0	0	0	0	0
	H			4	4	2	3	4	3
	W			2	0	4	3	4	5
	Av.			1.5	1	1.5	1.5	2	2
	M.V.			1.5	1.5	1.5	1.5	2	2
Unimembral Movement	B	1	0	0	0	0	3	2	2
	D	12	8	12	14	11	6	10	9
	H	5	5	9	11	7	7	6	6
	W	1	1	3	4	6	6	4	5
	Av.	4.8	3.5	6	7.3	6	5.5	5.5	5.5
	M.V.	3.8	3	4.5	5.3	3	1.3	2.5	2
Intramembral Movement	B	3	4	3	0	2	0	2	0
	D	0	0	0	0	0	0	0	0
	H	0	0	0	0	0	1	0	0
	W	2	0	0	0	0	0	0	0
	Av.	1.3	1	.8		.5	.3	.5	
	M.V.	1.3	1.5	1.1		.8	.4	.8	
No Movement	B	20	20	1	1	5	5	6	3
	D	7	14	2	4	6	10	3	6
	H	17	13	1	5	4	2	0	0
	W	21	23	9	7	0	0	0	0
	Av.	16.3	17.5	3.3	4.3	3.8	4.3	2.3	2.3
	M.V.	4.3	4	2.9	1.8	1.9	3.3	2.3	2.3

With the 25 interval, D reported phi in 1 P; with the 50 interval, B reported it in 1 M, and D in 1 M.

Types of Perception

The reports arising from the cutaneous and subcutaneous stimulations are so nearly alike that we shall for the most part confine our discussion to the former and shall mention the ' subcutaneous ' reports only where differences occur. The reports under the ' meaning ' and ' process ' instructions will not be treated separately. They will be grouped together in the following discussions, and throughout ' meaning ' will be dealt with first.

(1) *Full Movement.*—In all cases of full movement, the stimuli are felt as two touches with movement between them from *a* to *b*. The type appearing most often is that of the dumb-bell pattern. The stimulus touches, lifts, and moves in a narrow line to a new position. In ' process ' the first impression is a round or oval bit of extent. The impression

TABLE C—II

SUBCUTANEOUS MECHANICAL STIMULATION: SERIES II: ABSOLUTE NUMBERS

		25		50		75		100	
		M	P	M	P	M	P	M	P
Full Movement	B	1	5	5	2	11	14	15	17
	D	7	6	6	1	18	12	11	15
	H	0	0	3	3	16	15	10	10
	W	0	0	2	0	19	18	17	15
	Av.	2	2.8	4	1.5	16	14.8	13.3	14.3
	M.V.	2.5	2.8	1.5	1	2.5	1.8	2.5	2.1
Bimembral Movement	B			0	0	0	0	0	0
	D			0	0	0	0	0	0
	H			0	2	0	2	3	1
	W			0	0	1	2	1	0
	Av.				.5	.3	1	1	.3
	M.V.				.8	.4	1	1	.4
Unimembral Movement	B	0	0	1	6	1	5	0	0
	D	11	13	16	21	0	0	9	2
	H	2	1	9	9	6	7	11	13
	W	4	2	4	4	4	4	6	9
	Av.	4.3	4	7.5	10	2.8	4	6.5	6
	M.V.	3.8	4.5	5	5.5	2.3	2	3.5	5
Intramembral Movement	B	7	0	2	3	2	1	1	2
	D	0	0	0	0	1	0	0	0
	H	0	1	3	1	0	0	0	0
	W	0	0	0	0	0	0	0	0
	Av.	1.8	.3	1.3	1	.8	.3	.3	.5
	M.V.	2.6	.4	1.3	1	.8	.4	.4	.8
No Movement	B	16	19	16	13	10	4	8	5
	D	6	5	2	2	5	12	3	7
	H	22	22	9	9	2	0	0	0
	W	20	22	18	20	0	0	0	0
	Av.	16	17	11.3	11	4.3	4	2.8	3
	M.V.	5	6	5.8	5.5	3.3	4	3.9	3

With the 100 interval, D reported phi in 1 M.

extends in time into another oval or circular bit of experience like the first. The following reports are typical:

H (C 100 M) The hair touched lightly and began moving at once. As it moved it pressed more lightly and almost lifted off. Then, as it moved on, it pressed harder, and came down fairly lightly at the end. It moved faster at the beginning than it did in the middle.

H (C 100 P) The process began limited in extent. It increased in one direction to fairly extended, with slight contraction ending in a little spread of extent. It began as neutral pressure, becoming increasingly like contact until it was almost pure contact, with a rapid shift to pure neutral pressure. It was fairly weak, diminishing in intensity during the extent of the process, and becoming more intense at the end. The whole was very short.

The next type is much like the dumb-bell pattern, save that the movement does not start from the center of the first touch, but rather begins at the edge. In 'process' the first impression is described as static. Then there follows a growth from the edge.

H (C 100 M) A hair touched and rested; and then something started to move from the edge of the first touch and moved a little way, rested, and lifted off.

H (C 100 P) The process was limited and static for a bit; then growth began from the edge and became fairly extended. At first it was a dull neutral pressure, quickly shifting to a brighter pressure, and becoming neutral at the end. It was fairly weak, and dropped to weak. It was of short duration.

D (C 100 P) The process began as a contact fused with a wiry prick, immediately followed by a vague, diffuse, large pressure sensation localized differently.

In a third type the touches at beginning and end are of the same size as the moving touch between. There is no narrowing during the movement. In 'process' the terminals are marked by differences in quality and intensity, but not by a spread of extent. The first may be a pressure rather intense, merging into a spreading that is less intense, but of continuous extent, and ending in another intense pressure of the same extent.

W (C 100 M) A touch moved smoothly across my arm. It pressed harder at the beginning and ending than it pressed in the middle of its course. It was the same breadth all the way.

W (C 100 P) The process began, limited in extent, and spread in time, becoming weak during the spread but not contracting, and increasing in intensity as it became more extended. It began as good contact, and remained contact throughout. There was no change in extent at the terminals. The whole was short.

D (C 100 P) The process was neutral pressure, the area of which seemed to expand to another neutral pressure.

B (C 100 P) The process was a contact changing in intensity along a line.

The *O*'s describe the extension of the process in various other ways. It is called a 'flowing,' an 'extending in time,' a 'diffusing in time.' At first the word 'flow' was frequently used, but during the later reports it does not appear. Two *O*'s have said that 'flow' does not describe truly the depth of the sensation. Even when the intensity is weak, the growth seems to have the feel of something deeper than 'flow.' 'Ooze' has sometimes been used. H usually described the process as 'growth;' B and W used the term 'spread' most frequently; D spoke of 'extension and diffusion,' 'sensory continuum in time,' 'progression in time.'

With cutaneous stimuli the terminals, with few exceptions, are contact or neutral pressure. The same terms usually describe the subcutaneous experience; occasionally this is called 'drag' in the reports of H and W. In a few cases the subcutaneous stimuli produced a 'thicker' feel in the spread than did the cutaneous. In nearly all cases, except those just mentioned, the reports for the two stimuli are exactly alike.

(2) *Full Movement in an Arc.*—Occasionally the *O*'s reported a full movement in the shape of an arc. The first touch became larger in some one direction, and at the coming of the second it seemed to be pulled round to that position. In 'process' this type is described as a pressure becoming extended irregularly, and spreading around one side to another impression.

D (C 75 M) A touch went around in an arc to a new position.

B (C 100 M) The hair moved on my arm in the form of an arc, and rested in another place.

H (C 100 P) The process began limited and spread in extent out around one side, becoming narrower and ending in a slightly greater extent. It was neutral pressure, then contact with a shift to neutral pressure. It was fairly weak, decreasing in intensity during the extent, and increasing to fairly weak at the end. It was of short duration.

B (C 100 P) A contact diffused in a half circle and fused with another contact.

No *O* reported the arc movement more than five times in all the experiments performed. It occurred only when full movements were reported. Unimembral and bimembral movements did not take the arc form.

(3) *Bimembral Movement.*—In the bimembral movement the stimuli are felt as two touches which move, but which have a spatial and temporal interval between them. In nearly all cases *a* moves toward *b,* and *b* moves away from *a* to a resting place. Occasionally *b* touches and then moves on and off without resting. In 'process' the experience is described as two pressures or contacts, separated spatially and temporally, both diffusing in time.

B (C 500 M) A hair touched me and moved a bit. It lifted off, and came down moving into a new place.

H (C 100 P) The first process began very limited and grew in extent to slightly extended. It was a neutral pressure quality fading out, and it was very short. The second process was limited and shrank in time to very limited. It was neutral pressure with a hint of contact at the beginning, and it was weak and short.

D (C 100 P) Two neutral pressures occurred successively in time and were localized differently in space. The first became diffused in time, and the second likewise.

(4) *Unimembral Movement.*—In the unimembral movement, sometimes the first stimulus moved and the second was stationary, but more often the second was the moving member. Stimulus *a* touched and was gone; *b* touched while moving away from *a* to a new position. In 'process' one of the impressions was said to spread or extend in time.

H (C 100 M) A hair touched me fairly lightly and lifted off. It touched again in another place, moving a short distance. It rested and lifted off.

W (C 100 P) First there was a contact limited and very weak. Near it in space was a contact spreading in time to a larger extent and becoming more intense during the spread.

H (C 100 P) The process began limited, and grew slightly in extent; it was dull neutral pressure becoming pure pressure in the extent. It was fairly weak, with the intensity dropping off. It was very short. The second process was very limited neutral pressure, and was fairly weak and short.

There were other reports more rarely given. Sometimes only one stimulus was felt, and it moved to a stopping place, or began as static and moved off. In 'process' such experiences were called "a spreading contact growing more extended and more intense as it spread," or "a contact which spread and faded out."

(5) *Intramembral Movement.*—The intramembral movement is "the feeling of movement as if the hair were held in an unsteady hand," or "a wiggling of the hair against the skin." In 'process' the experience is described as "a slight shift of the intensive pattern in time."

D (C 250 M) There were two touches successive in time and localized differently in space. The first quivered as if held in an unsteady hand.

B (C 500 P) There were two contacts differently localized and successive in time. The second had changes of intensity in time; otherwise it was just two.

W (C 1000 P) The first process was a contact which fluctuated in intensity in the touched area. The second process was pure contact, very limited and short.

H (C 100 P) The first process was very limited and rather a brightish pressure of moderate intensity and very short. The second process was a fairly limited neutral pressure quality. There was a shift in time of the intensive patterns from moderate to fairly weak. The whole was short.

(6) *Intramembral plus Unimembral Movement.*—In the manual experiments and the first series of the mechanical stimulations there were reported some cases of combined perceptions of intramembral and unimembral movement. Sometimes one stimulus gave the perception of intramembral movement, and the other that of unimembral movement. Occasionally one stimulus would begin its movement as intramembral, and then extend into unimembral movement. These reports do not appear in the last series, in which the reports of unimembral and intramembral movements became uniform and clear-cut, with no hint of confusion or addition.

(7) *Phi Movement.*—The phi movement occurs infrequently. A typical 'meaning' report is: "It felt as if a moving some-

thing flicked against my arm." In this perception we do not have the feeling of two touches, or of one touch moving to a new position. In 'process' there were no distinct terminal experiences; the pressure is called "a weak, spreading contact, a bit more intense in its middle portion." Most of the pure phi-reports occurred in the manual experiments, and came early in the series.

(8) *Backward Movement.*—In the manual experiments and in the first series with the mechanical stimulations there were a few reports of unimembral and bimembral movement in which the stimulus seemed to move backward. That is, one impression was felt; and the second, instead of moving away from it to a new position, would be felt some distance away moving toward the first. H and W reported this perception three or four times in the manual experiments, but not afterwards. B reported it twice in the manual experiments; four times in Series I with the mechanical stimulations, and not at all in Series II. D reported backward movement three times in the manual experiments; in Series I of the mechanical stimulations he gave no such reports; in Series II he reported it 24 times (6.25% of all the reports in that series).[9]

(9) *No Movement: Two Points Distinct.*—In nearly all cases in which no-movement was reported, the perception was of two points touched successively in time and having spatial separation. Sometimes the one impression was more diffuse than the other, or differed from it slightly in quality; usually the two were reported as alike. This type of perception occurred in all experiments with intervals above 50^{σ}.

(10) *No Movement: Conjunctive.*—A report made frequently with the 25^{σ} intervals belongs to the no-movement group, though it indicates something more than merely two touches which did not move. The second stimulation was added to the first, or placed beside it. Sometimes the second grew larger after it had appeared, and again it came as a diffuse, ill-defined touch. In 'process' one contact or pressure had added to it another, and there was little separation of the two in space and time.

D (C 25 M) There were two light touches which came separately, but whose areas overlapped.

H (C 25 P) The first process was very limited. It was a weak contact and very short. Beside it came another process of pressure quality, with a fringe having in it a hint of contact. It was of very weak intensity and very short.

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[9] D's observation of backward movement evidently became a matter of habituation. Very occasionally in this series he reported triple pressure; as, for instance, pressure at *a* followed by a pressure at *b* which moved back toward *a* and then ended with increased intensity.

H (C 25 P) The process was very limited and was added to. It became fairly limited in one dimension. It was of neutral pressure quality with a fringe of contacty pressure. It was fairly weak. The whole process was fairly extended and fairly weak.

(11) *No Movement: Superpositive.*—Occasionally the two stimulations seemed to be successive in time but to fall upon the same spot.

W (C 25 M) There were two touches, first one and then another beside it covering it. The two were almost successive in time.

H (C 25 P) The first process was very limited, and while it was still present another limited process covered it. The second was more extended, but there was perfect fusion of the two. Both were good neutral pressure quality, of weak intensity. The first was very, very short. The second was slightly longer.

D (C 25 P) The first process seemed obscured by the second. I was conscious of the first, but it was absorbed by the second, which was big and which gradually became bigger.

It will be noted that whereas B, H and W have a large number of reports falling into the no-movement group in the 25^σ interval, D has but few.[10]

Distance of Movement

Under the ' process ' instruction the *O*'s never reported how far the stimulus moved in full movement, or how far apart were the members in part movement. In ' meaning ' the perceptive distance in the larger intervals, as occasionally reported, was always greater than it was in reality, and varied according to the interval. With the 1000^σ interval it was reported as 1.5 to 2 cm. As the intervals shortened, the distance decreased to 4 or 5 mm. at the 100^σ interval, and at the 25^σ interval the stimulations were side by side.

Rate of Movement

Rate of movement, like distance, was reported only under the ' meaning ' set. Sometimes during the manual experiments the *O*'s said that the movement went " quick as a flash " or " rapidly." Sometimes in the full movement reports one part of the movement was said to move more quickly than another. In bimembral movements, the *O*'s occasionally said that " the second went quicker than the first." Aside from instances where one part of a movement could be compared with another in the same perception, the *O* was uncertain of the rate, and hesitated to report it. During the mechanical stimulations the reports of rate dropped out. If questioned about it, the *O*'s said: "I am not sure," or "I can't report on it."

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[10] See footnote 9.

Stimulations of Long and Short Duration

After the series with the duration of the stimulus at 150^{σ}, four other series (2C and 2S) of 24 experiments each were performed with H and W. In these series the 75 and 100 intervals were used, and the durations were 75 and 200^{σ}. No full movements were reported by W with duration at 200^{σ}; H gave such reports in 6M and 4P at C 100. With duration at 75^{σ} with C 100, H reported full movement in 14M and 17P, and W in 14M and 15P. These reports are almost the same in number and content as those with the duration at 150^{σ}. With C 75, H had no full movements and W had 5M and 8P. With S 75 neither H nor W reported full movement. With S 100, H had 8M and 2P full movements, and W had 8M and 8P. Since the intervals longer and shorter than 150 proved to be not so good as 150, and in only one case compared favorably with it, no farther experiments were performed with them.

Experiments Upon Non-Adjacent Spots

A short series of 24 experiments with each stimulus was performed upon non-adjacent spots. The spots *a-c* and *a′-c′* were used. H and W again served as *O*'s. The stimulations were made with the 75 and 100^{σ} intervals. The duration was 150^{σ}. H reported no movement during any of the experiments. W in S 75 had 8M and 12P unimembral movements. The movement always occurred with the second member. With C 100, W reported 12M and 14P unimembral movements, also at the second member. The movements were very short. There was just a hint that the second stimulus slid a very short distance into place. In 'process' the second stimulus had a very slight diffusion in time.

Cutaneous Versus Visual Movement

Dimmick under his best conditions obtained full movement with 'process' instructions in 90 to 93% of his reports, and with 'meaning' instructions in 81 to 91%. We under the most favorable conditions obtained on the average with C only 13.75 (57%) M-reports and 14.25 (59%) P-reports out of a possible 24; and with S, 16 (67%) M and 14.75 (61%) P. It seems from a comparison of these figures that the cutaneous perception of movement is less fundamental and compulsory than the visual. There was, it is true, a difference in the plan of the two investigations, since Dimmick's stimulus *a* in his regular series always bore a fixed spatial relation to his stimulus *b;* whereas the spatial relations of the spots stimulated by us varied from group to group. Our stimulus *b* might lie above or below *a,* to right or to left of *a.* It is difficult to say how far this difference influenced the results; but it seems from what indications we have that the influence was slight. (1) A special C-series made with H, in which the same two spots *a-b* were stimulated in the same order in successive observations to the point of exhaustion, yielded no increased percentage of full movements. (2) The conditions of our mechanical stimulation approximated those of Dimmick's experiments. After the first two or three stimulations of two spots at the beginning of a group of twelve experiments, *O* knew when the second stimulation came that it was in the same place as before, and so learned to expect it at that place. H, who observed in Dimmick's work, said: "I was not conscious of expecting the second stimulation in the place where it came, but when it did come I had the feeling that it was where it ought to be and where it had been before." H further remarked that the expectancy was "the same sort of thing as in Dimmick's experiments." W, who also observed with Dimmick, verifies these reports.

Only one *O,* B, reported associated visual images, and she only in the early stages of practice. H and W declared that visual images did not appear; and D when questioned said that he had them occasionally, but that they were never a determining factor in his perceptions.

The aberrant types of perception, the arc-movement and the backward movement, seem to be conditioned upon some directional emphasis within the tactual spread. Ordinarily the spread of sensation is roughly circular or oval. If, however, some lateral segment of *a*'s spread is unusually intensive, the arc movement necessarily follows; and if *a*'s spread is uniformly weak, and *b*'s is strongest over a segment that points toward *a,* the perception of backward movement follows. We have no means of knowing how these local emphases arise, but the *O*'s reports show that they appeared and formed the basis of judgment. There is, of course, nothing parallel in Dimmick's results.[11]

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[11] Dimmick tried to get backward movement by artificial emphasis and failed: *op. cit.,* 331.

Conclusions

(1) We have succeeded in synthetizing the perception of cutaneous movement, from two separate and successive stimulations, under conditions that parallel Wertheimer's synthesis of visual movement.

(2) The optimal conditions for the arousal of the perception are: with cutaneous stimuli, a duration of 150$^\sigma$ and an interval of 100$^\sigma$; with stimuli that also affect the subcutaneous organs, a duration of 150$^\sigma$ and an interval of 75$^\sigma$.

(3) (*a*) The most important conditions of the perception, under our conditions, are the interval elapsing between the successive stimuli and the adjacency of the spots stimulated. (*b*) The duration of 150$^\sigma$ is best for arousing the perception of full movement; the number of full movements aroused with longer times is much lower. The same thing is true also of the duration of 75$^\sigma$ except in one instance, namely C 100, where the number of full movements aroused compared favorably with those reported when the duration was 150$^\sigma$. (*c*) The stronger of our two intensities, the 'subcutaneous' stimulus, yields a slightly larger percentage of full movements with its best interval, 75$^\sigma$, than does the cutaneous stimulus at its best interval, 100$^\sigma$.

(4) Whereas Dimmick was able to secure the perception of visual movement, under optimal conditions, in practically 100% of his trials, we found that the perception of cutaneous movement appeared, under the same conditions, in only 57 to 67% of our observations. Since, so far as we can discover, there is nothing in the arrangement of the two experiments that accounts for this difference of result, we are forced to the conclusion that the cutaneous perception is less fundamental and compulsory (in Dimmick's sense) than the corresponding visual perception.

(5) Support for this view is found in a comparison of the existential correlates of the two perceptions. Dimmick's grey flash is an integration of quality with time; space is not involved. The existential correlate of the cutaneous perception appears, on the other hand, to be an integration of quality, time and cutaneous extent. While Dimmick's *O*'s reported a curtain or film of shimmering, liquid, live grey, our *O*'s (in part the same as Dimmick's) report a pressure diffusing, growing, extending, in time. The integration appears to be of a higher order, and therefore less stable and inevitable.

(6) We regard it as entirely premature to theorise from the results of Wertheimer and Dimmick in vision and from our own results in touch to ‘ the ’ perception of visual or cutaneous movement. It may very well be that the singular number must be replaced in this and similar connections by the plural. ‘ Movement ’ is, after all, a gross meaning that may be carried by several existential correlates ; or, to put the same thing in other words, an organism whose outfit of sense-organs is as complex as our own may be adequate to the ‘ perception of movement ’ by way of several different primary integrations.

SOME QUALITATIVE ASPECTS OF BITONAL COMPLEXES[1]

By Carroll C. Pratt

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The present investigation neither pretends to a resolution of the problem of fusion in its systematic context nor to a new determination of the order of degrees of fusion. It was initiated under the conviction that there were within reach a point of view and a method, which would single out for separate examination certain phenomenological characteristics which enter into the total impression set up by two tones when presented simultaneously to the organ of hearing. The literature on tonal fusions, although rich in experimental studies undertaken from various angles of approach, is nevertheless far from being in a settled state. The suspicion is hard to escape that much confusion has resulted from a failure to take into consideration the fact that any judgment upon the nature of fusion is inevitably ambiguous unless directed upon one univocal characteristic of the tonal impression.

A review of the literature would be unnecessary iterance in the present article.[2] Many of the controversies to which Stumpf's classic volumes gave rise are due to Stumpf's ambiguity of definition and procedure, or at best, to a misunderstanding on the part of others of his exact concept of fusion. It is not surprising that misunderstandings should arise. As a rule Stumpf displays a predilection for calling fusion "dasjenige Verhältnis zweier Inhalte, speciell Empfingdungsinhalte, wonach sie nicht eine blosse Summe sondern ein Ganzes bilden."[3] Nevertheless by way of amplification, Stumpf refers to fusion as "dasjenige Verhältnis zweier Empfindungen, in folge dessen" the total impression of the more perfect fusions "immer mehr dem Einer Empfindung nähert und immer schwerer analysirt wird."[4] Of course such aspects of fusion are secondary, but Stumpf uses them in his experiments as criteria for fusion. Again Stumpf calls fusion "eine unveränderliche Eigentümlichkeit des Empfindungsmateriales, welche immer noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden, und welche gerade nachdem die Analyse vollzogen und die Töne deutlich als zwei erkannt sind, ebenfalls erst in sich selbst bemerkt werden kann."[5] "Wie sich dies ausnimmt, muss man eben hören und kann es Niemand klarmachen, der nicht hören oder Gehörserscheinungen nicht beobachten kann."[6] Stumpf throws in another confusing implication when he remarks that "der Zusammenklang zweier Töne nähert sich bald mehr, bald weniger dem Eindruck Eines Tones, und es zeigt sich, dass dies um so mehr der Fall ist, je konsonanter das Intervall ist."[7] Wundt[8] has brought the reproach against Stumpf of creating a purely logical concept of fusion, in spite of Stumpf's manifest efforts to base his inductions on phenomenological experience. Others,[9] accepting the Stumpfian doctrine on its theoretical side, or, on the contrary, dispensing with it, or at times modifying it, have centered their interest in the search by repeated experiment and observation for a more stable order of degrees of fusion. Still others[10] doubt the justification for introducing a unique notion like fusion into the psychological system and attempt to account for the phenomenon in terms of analysability, unitariness, harmony or what not, or else to work out an entirely new definition of fusion.

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[1] From the Psychological Laboratory of Clark University.

[2] For a recent review of the literature, see W. Kemp, Methodisches und Experimentelles zur Lehre von der Tonverschmelzung, *Arch. f. d. ges. Psychol.,* 1913, 29, 139-181.

[3] C. Stumpf, *Tonpsychologie,* 1890, ii, 128.

[4] *Loc. cit.*

Kemp's experiments are an attempt to separate out the factors which may enter into the phenomenon of consonance. The results indicate the operation of the following four *Merkmale:* fusion in the sense of that "unverändliche Eigentümlichkeit welche immer noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden;" pleasantness and unpleasantness; *sinnlicher Wohlklang; sinnliches Zusammenpassen,* a sort of relationship by virtue of which the components of an interval seem to belong or go together; and *harmonisches Zusammenpassen,* the need of resolution (*Auflösungsbedürfniss*).[11] Unfortunately, however, Kemp gives no clue as to the structural nature of these *Merkmale:* an omission regrettably common in work on the psychology of fusion.

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[5] *Ibid.,* 127f.

[6] *Konsonanz und Dissonanz,* 1898 44.

[7] *Ibid.,* 35.

[8] *Physiologische Psychologie,* 1910, 6 Aufl., ii, 124ff.

[9] E.g., O. Külpe, *Grundriss der Psychologie,* 1893, 289ff.; A. Faist, Versuche über Tonverschmelzung, *Zeits. f. Psychol.,* 1897, 15, 102ff; Th. Lipps, *Leitfaden der Psychologie,* 1909, 3 Aufl., 98ff; Tonverwandschaft und Tonverschmelzung, *Zeits. f. Psychol.,* 1898, 19, 1ff; W. Kemp, *op. cit.,* 153; T. H. Pear, The experimental examination of some differences between the major and minor chords, *Brit. J. Psychol.,* 1911, 4, 56ff.

[10] Wundt, *Phys. Psychol.,* 1910, ii, 121; F. Krueger, Beobachtungen an Zweiklängen, *Philos. Stud.,* 1900, 16, 307ff; Die Theorie der Konsonanz, *Psychol. Stud.,* 1906, 1, 305ff; 1907, 2, 205ff; 1909, 4, 201ff; 1910, 5, 294ff; P. Natorp, *Göttinger, Gel. Anz.,* 1886, 145ff; 1891, 789ff; H. Cornelius, Ueber Verschmelzung und Analyse, *Vierteljahrsschr. f. wiss. Philos.,* 1892, 16, 404ff; E. Buch, Ueber die Verschmelzung von Empfindungen, besonders bei Klangeindrücken, *Philos. Stud.,* 1899, 15, 1ff, 183ff.

[11] Kemp, *op. cit.,* 181-203.

The present experimental investigation was begun in the fall of 1919 and continued until the spring of 1921 in the Psychological Laboratory of Clark University.

PRELIMINARY SERIES

The *O*'s who took part in the experiment were G, Dr. E. G. Boring, who worked with knowledge of the problem; F, Dr. S. W. Fernberger; L, Dr. L. D. Boring; Y, Dr. M. Yokoyama; and B, Miss M. Bates. All were highly trained in observation. B was the most "musical" *O,* but found no difficulty in abstracting from musical knowledge. L had absolute recognition of single pitches but had no musical recognition of intervals. Y had but slight "ear" for occidental music and could not recognize intervals musically at all. G and F had no musical recognition of intervals.

Three Stern variators (210-400, 310-600, and 410-800 d.v.) were used as stimuli throughout the experiments. They were placed side by side on a wooden support 80 cm. from the floor on one side of the experimenting room. *O* sat with his back toward the source of sound at a distance of 1.5 m.

Preliminary experiments were conducted to determine roughly the rank order of degrees of fusion for the five *O*'s on the basis of rapidity of analysis, a method suggested by Külpe.[12] A Bergström chronoscope, placed alongside the variators and connected with the reaction key on the arm of *O*'s chair, was used to measure reaction times. The octave, fifth, fourth, sixths, thirds, tritone, sevenths, and seconds were used. The order of presentation was varied from time to time, and occasional *Vexirversuche* of single tones were thrown in. Fifty reactions upon each interval were taken.

The following typewritten instructions were read by *O* at the beginning of each session: "Following the 'ready' signal, you will be presented with (*a*) single notes, and (*b*) two-note complexes. The duration of the stimulus will be 0.5 sec. You are requested in each case to react upon the key as soon as you have decided whether the sound which you hear is that of one note or two notes, stating afterwards whether the judgment was 'one,' 'two,' or 'doubtful.' In case of the latter, indicate, if possible, the direction in which the judgment inclined."

Proceeding on the uncritical assumption that degrees of fusion are inversely proportional to rapidity of analysis, a scale of degrees of fusion was calculated from the reaction times of each *O*. The rank orders thus obtained were utilized in the next step of the preliminary experiments.

The purpose in this second part of the preliminary experiments was to present tonal complexes to *O* which could not readily be analyzed into separate components and to secure from *O* qualitative descriptions of the impressions thus set up. Although Stumpf[13] insists that the phenomenon of fusion still persists after analysis is complete, it was deemed worth while to forestall analysis at least in part by combining the intervals which had yielded less quickly to analysis in the preceding experiments into triads that could be but very imperfectly separated. (*E.g.*, the first six intervals which resulted from the foregoing experiments for L were in order of difficulty of analysis: octave, fifth, fourth, major third, minor sixth, major sixth. These intervals were combined into the following triads: d-g^b-a, major third and fifth; c-g-c, fifth and octave; d-g-d, fourth and octave; d^b-f-b, major third and major sixth; a-f-a, minor sixth and octave; b-a^b-b, major sixth and octave; d-g-b, fourth and major sixth; and c-e-c, major third and octave. Similarly for the other *O*'s.)

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[12] *Grundriss der Psychologie,* 1893, 239.

[13] *Tonpsychologie,* ii, 127f.

In these experiments the stimuli were allowed to sound for one sec. *Vexirversuche* of two-note complexes were occasionally interspersed. The instructions were similar to the previous experiments, except that the phrases "two-note" and "three-note complexes" were substituted for "single notes" and "two-note complexes." It was not long before the judgments and the length of reaction times furnished *E* with sure indication that certain of the triads were submitting very imperfectly to analysis. Judgments of "two" for three-note complexes were very frequent; or the time of reaction was often so great that *E* lost count of the number of swings of the pendulum across the 2000σ scale. *E* secured introspective descriptions of the impressions aroused by these more imperfectly analyzable triads. The qualitative data thus obtained put *E* in the possession of valuable descriptive material for experiments to be mentioned presently.

It now appeared desirable to adopt a direct method of observation.[14] The method of paired comparisons provided a suitable form of procedure. Every one of the twelve intervals was paired with the eleven other intervals six times, making thus $(12 \times 11)/2$ comparisons x 6 repetitions = 396 judgments in all. There were 11 comparisons x 6 repetitions = 66 total possible preferences for any one interval. The intervals were arranged in haphazard order, with the precaution that no interval should be presented twice in succession. The components for the intervals were all included within the range of a major 9th, from b^b of the small octave (234.66 d.v.) to c^2 of the twice-accented octave (528 d.v.). With an eye to the time-error, the order of presentation of the intervals within a pair was reversed in alternate series. It was possible to secure about 100 judgments in each experimental session.

The following instructions, derived from the Stumpfian *Folgeerscheinung* of *Einheitlichkeit,* were read by *O* at the beginning of every session: "Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of unitariness, regardless of other possible characteristics of the impressions. Report 'first' or 'second' according as the first or second impression is more unitary. If there is no difference between them, report 'same.'"

These instructions proved to be highly equivocal for all *O*'s, for they soon discovered that the meaning of unitariness might be carried by quite different sensory data present in the total tonal impression. The judgments were made only with extraordinary difficulty and reported with still greater reluctance, for *O* was peculiarly loth to trust his judgment when faced with several processes all of which were possessed of potential contexts for unitariness in nearly equal degrees of mediacy.

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[14] On direct *vs.* indirect methods in work on tonal fusions, see Kemp, *op. cit.,* 163-179.

The situation thus created is interesting in the light of recent attention to the "stimulus-error" in psychophysics.[15] Ordinarily a careful control of attitude is necessary to secure univocal determination in psychological experiment. The experiments of George[16] indicate the ease with which equivocal criteria may arise with inconstancy of attitude, and the recent work of Friedländer[17] and Fernberger[18] with lifted weights makes clear the effect of this inconstancy upon the objective results which psychometric functions represent. The manner in which the limens for dual impression upon the skin may vary under attitudinal shifts has become well established in cutaneous psychophysics.[19] This development simply serves to give impetus to the current urge in experimental work for a sharper delineation of observational attitudes, to the end that a more univocal correlation may be arrived at between the terms in the total chain of experience. This chain, following Urban's[20] schema, may be represented by $\alpha, \beta, \gamma, \ldots$ for physical conditions (stimulus-situation), $a, b, c, \ldots$ for physiological processes of nerve excitation, and $A, B, C, \ldots$ for contents of consciousness. Since similar terms in these three systems, as β, b, B, are not in experimental practice univocally correlated, it becomes necessary to discover what factors in the $A, B, C, \ldots$ system are correlated, e. g., with β in the stimulus-situation; and it may turn out that the conscious correlates of β are A, C, M, and P, depending on the nature of the *Aufgabe* which is operative in the stimulus-situation. If the Aufgabe is not set carefully by instructional determinants, *O* may judge now upon one, now upon another, of the A, C, M, P, factors, so that it becomes scientifically absurd to speak of a correlation. Hence "the total *Aufgabe* under which the subject judges must be made definite in instructions, and must be more fully determined by means of repeated characterizations by the subject of his attitude and procedure. This latter check is important since much of the subject's instruction is apt to be a self-instruction."[21]

In the series of experiments under consideration, it became apparent that the *O*'s were laboring under an equivocal instructional determinant. Let it be supposed, for instance, that the tonal intervals in the stimulus-situation had among other conscious correlates the factors B, D, L, and Q, all of which were potentially associated with the contexts B', D', L', and Q', carrying the meaning for unitariness.

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[15] See especially E. G. Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 1920, 27, 446ff., 449; The stimulus-error, *Amer. J. Psychol.*, 1921, 32, 449ff.

[16] S. S. George, Attitude in relation to the psychophysical judgment, *Amer. J. Psychol.*, 1917, 28, 1ff.

[17] H. Friedländer, Die Wahrnehmung der Schwere, *Zeits. f. Psychol.*, 1920, 83, 129ff.

[18] S. W. Fernberger, An experimental study of the "stimulus-error," *J. Exp. Psychol.*, 1921, 3, 63ff.

[19] E. B. Titchener, Ethnological tests of sensation and perception, *Proc. Amer. Philos. Soc.*, 1916, 55, 206-215; E. deLaski, Perceptive forms below the level of the two-point limen, *Amer. J. Psychol.*, 1916, 27, 569-571; C. L. Friedline, Discrimination of two cutaneous patterns below the two-point limen, *ibid.*, 1918, 29, 400-419; and especially Boring, The stimulus-error, *Amer. J. Psychol.*, 1921, 32, 449ff.

[20] F. M. Urban, Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.*, 1913, 30, 116-118.

[21] Boring, *Psychol. Rev.*, 27, 449.

Under the *Einstellung* laid down by the instructional determinant for unitariness, *O* would have a wide and unchecked range for equivocal judgments which would issue in rank order of degrees of unitariness of which the scientific significance would be small.

CHART 1

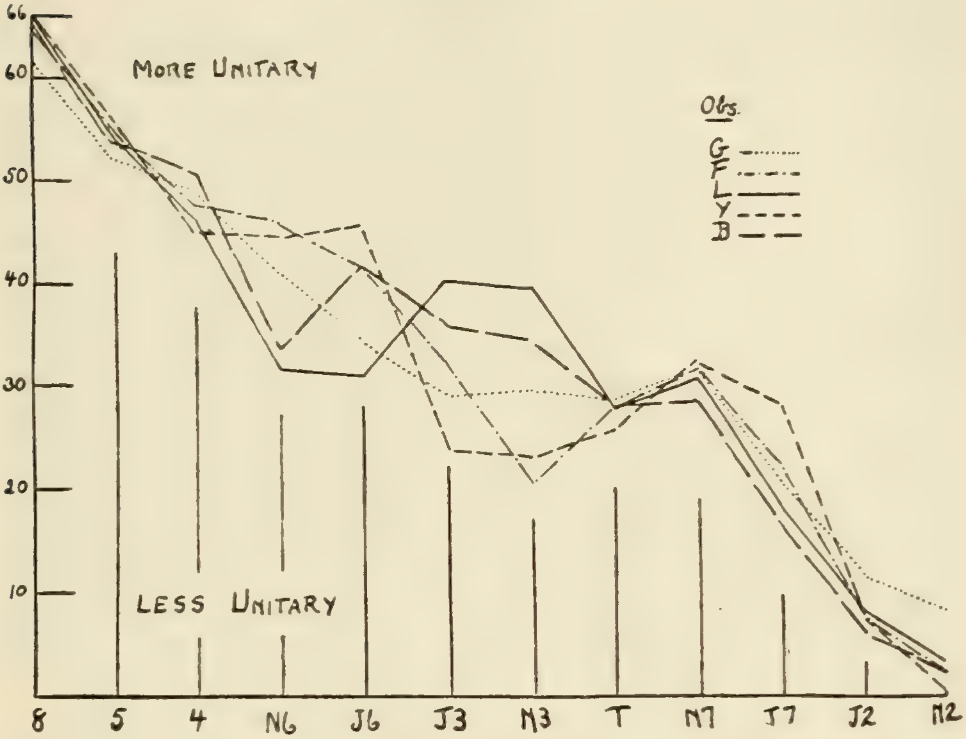

CHART 1. Rank-order of tonal intervals under instructions for 'unitariness' (see text). Method of paired comparisons. Ordinates represent number of discriminations for every interval for five *O*'s. Abbreviations: 8 = octave, 5 = fifth, 4 = fourth, N6 = minor sixth, J6 = major sixth, J3 = major third, N3 = minor third, T = tritone, N7 = minor seventh, J7 = major seventh, J2 = major second, N2 = minor second.

In Chart 1 are shown the curves derived from judgments of unitariness. In all the charts the intervals on the abscissa have been arranged in the same order for ease of intercomparison. The numbers on the ordinate represent the frequency of preferences. The curves of Chart 1 make clear the rather significant fact that *O*'s can take up a stimulus-attitude toward mental experience and come out with a surprising degree of uniformity. The net returns to psychology of such agreement of results are small, however, inasmuch as no light is thrown upon the mental processes involved in the judgments.

At the end of this series of experiments, the *O*'s were asked for introspective characterizations in which should be noted the criteria which had entered into or cut across the judgments of unitariness. These reports, together with the qualitative data secured from the earlier preliminaries, furnished the material out of which grew the experiments now to be considered.

PRINCIPAL SERIES

The method and procedure in the following series were the same as in the foregoing experiment. The only difference lay in the instructions given to *O*. From the introspective data at hand were selected those factors in the *A, B, C,* . . . system of tonal mental processes which approximated the phenomenological level of experience. It was hoped by means of instructions and self-checking on the part of *O* to limit the judgments to a single factor, and thus come out with rank orders of which the significance would be more certain. Certain of the factors employed as criteria of judgment were common to the introspective reports of all the *O*'s; others, especially those of a contextual nature, followed the idiosyncrasies of individual differences and could be used as instructional determinants only for particular *O*'s. Before beginning work on a new criterion, one observational session was devoted to practice judgments, in order that *O* might familiarize himself with the new factor and become sufficiently *eingestellt* to take up a similar attitude in subsequent sessions. Whenever, during the series, *O* noted a lapse in attitude or found it impossible to make a judgment in accordance with instructions, he was expected to report the fact to *E*. At the end of every series *O* was requested to give a descriptive characterization of his attitude and basis of judgment.

Auditory Processes

Smoothness-roughness.—The first criterion to be used had as its sensory basis the 'smoothness' and 'roughness' of the auditory impressions. This factor had been frequently reported in all the introspections, and was easily taken up and followed through by all the *O*'s. The instructions for this series read as follows:

> "Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of 'smoothness' and 'roughness,' regardless of other possible characteristics of the impressions. Report 'first' or 'second' according as the first or second impression is 'smoother' or less 'rough.' If there is no difference between them, report 'same.' Whenever you fail to live up to instructions or find it impossible to make a judgment on the basis of the above criterion, report 'failure.'"

The purpose of such an instruction was to get *O* univocally determined in the direction of auditory 'smoothness-roughness' and away from other possible factors in the tonal impression.

The descriptive characterizations point to ‘roughness’ as the positive category in this series, and to ‘smoothness’ as the absence of ‘roughness.’ ‘Roughness’ is a temporal-intensive pattern in which intensity varies in time, whereas complexes in which intensity seems relatively constant are ‘smooth.’ Comparison-judgments between complexes in which the variation of intensity is rapid and those in which intensity variations are slow (minor thirds and major seconds *vs.* minor seconds) embarrassed some of the *O*’s. This difficulty they usually resolved by judging as ‘smoother’ those complexes in which the intensity variations are more rapid.

CHART 2

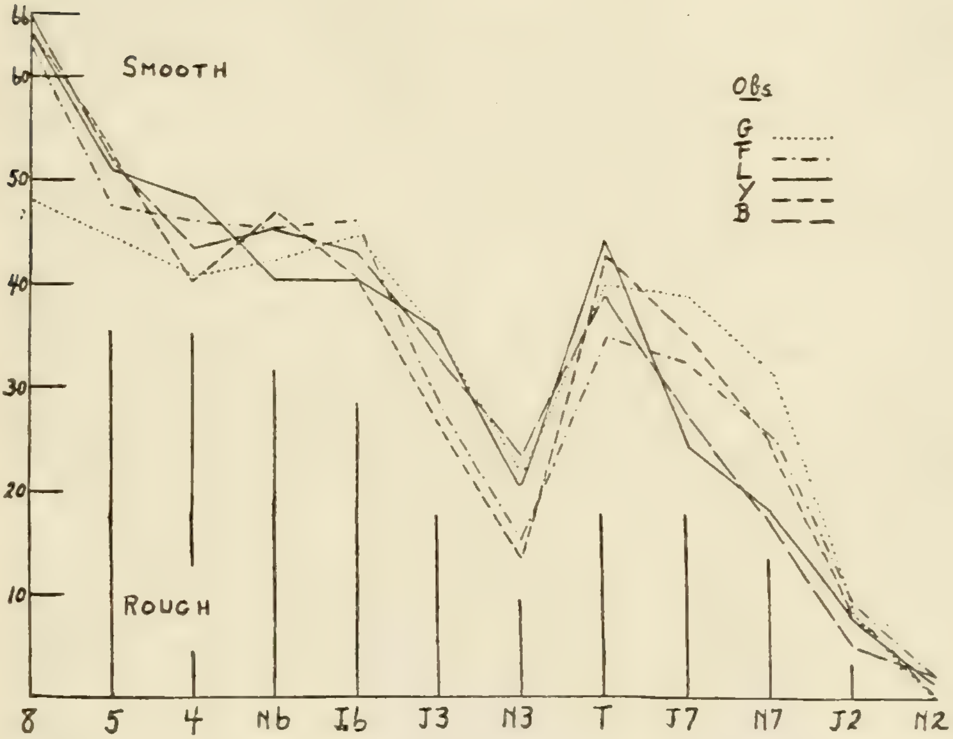

Chart 2. Rank-order of tonal intervals under instructions for ‘smoothness-roughness’ (see text). Curves for five *O*’s.

Chart 2 indicates the rather striking uniformity of rank orders obtained under this attitude. It will be noted that ‘smoothness’ and ‘roughness’ vary with the physical distance of the components of the interval: the octave, fifth, fourth, sixths, tritone, minor seventh, and major third all come higher on the scale than the minor third and the seconds. The relatively low position of the major seventh must be due to the presence of overtones which cut across the factor of distance.

Simplicity and Complexity.—The next two criteria of judgment to be adopted took their root in the general phenomenon of analysability. The possibility of taking up two attitudes in this respect made it advisable to follow through separate series. The instructions in the first series, as, in point of fact, for all subsequent series, were worded like the instructions for 'smoothness-roughness,' except in those phrases intended to determine *O* in a new direction:

> "Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of 'simplicity.' By 'simplicity' is meant the degree of approximation to singleness of auditory core, *i. e.,* unanalysability of the impressions. Report 'first' or 'second' . . . " etc.

The instructions for the second series read:

> ". . . In each case you are to compare the two impressions with respect only to degrees of 'complexity,' *i. e.,* you are to state in which of the two impressions the components separate most during presentation. . . ."[22]

The introspective data for the 'simplicity' series indicate that the criterion here involves difficulty of analysis or independent variability of clearness of components in time, which occurs under a fairly active attitude (=attentional attempt at analysis). This is to say that those complexes in which it takes longer for the total impression to split up into its separate components, or in which one component dominates the other in clearness, are judged as more 'simple' inasmuch as they approximate more nearly to singleness of auditory core.

In the 'complexity' series, on the other hand, the judgments are made under a more passive attitude. *O* reported merely upon the relative "analysedness" of the impressions as given during presentation of the stimulus, and not upon 'analysability.'

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[22] During the practice period for this series it was discovered that the *O*'s were often uncertain in their judgments because of the change which the impressional contents for judging underwent during the formulation of the judgments in the course of the immediate memory after-images. Hence they were instructed to base their judgments upon the contents as they appeared *during presentation.* To facilitate this mode of judgment, the exposure time of each interval was reduced from 2. to 0.5 sec. (measured by a silent metronome).

In Chart 3 are shown the curves for the 'simplicity' series. The 'simplicity' curves for G and B turned out to be nearly identical with their 'complexity' curves, and have therefore been placed as G(s) and B(s) on Chart 4 ('complexity') instead of on Chart 3 (the other 'simplicity' curves). The three curves of Chart 3 follow fairly closely the Stumpfian order of degrees of fusion, except for the rather high positions of the tritone and minor seventh, a possible explanation of which will be mentioned presently. The 'complexity' curves deviate from the 'simplicity' curves in a uniform and significant manner. Those intervals of which the components lie far apart on the tonal scale (sixths and sevenths) occupy the lower positions on the chart, whereas those intervals of which the components are proximate (thirds and seconds) occupy uniformly higher positions. The high positions of the octave, fifth, and fourth, are undoubtedly referable to the factor of 'fusion'[23] which cuts across the factor of sense-distance. The opposed positions of the seconds on the two charts demand a word of explanation, an explanation forthcoming in the introspections, which show quite definitely that two different attitudes are operative in the case of the seconds: a 'meaning' attitude and a descriptive attitude. The *O*'s whose curves for the 'simplicity' series are represented in Chart 3 judged certain of the intervals 'less simple' on the basis of contents which carried the meaning of non-simplicity rather than on the basis of relative degrees of simplicity of the auditory experience *per se*. The following quotations from their introspective characterizations illustrate the point:

Ob. F. "In some cases one of the complexes comes as very rough. It has the meaning of being readily analyzable at once. How this meaning is present structurally I cannot say, although the basis of it seems to be roughness. There is a verbal characterization of it as 'less simple,' but there does not seem to be any auditory attentional analysis."

Ob. L. "I tend naturally to judge as not simple a complex which produces roughness, or one where the 'noise' stands out prominently."

Ob. Y. "In such a case [comparison of a rough complex with a smooth one, the components of which stand out clearly] the rough one is usually cognized as a complex of two tones, more immediately than the other. Yet, since it was very difficult to split up the rough interval into two components actually, *i. e.*, psychologically, I judge it as simpler."

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[23] The word "fusion," when used in connection with the curves under consideration, bears no theoretical implications: it is merely a convenient term to apply to those intervals whose components either will separate out only with difficulty (=unanalysability) or do not fall apart during presentation (=unanalysedness).

Y's report shows that he was aware of his two attitudes, but his curve points to only a partial approximation to the psychological descriptive attitude: more often he judged the rough intervals on the level of cognizance and meaning. In the complexity series, however, he was able to abide quite rigidly by the descriptive attitude, as his curve indicates. L, too, was able to keep to the descriptive attitude fairly consistently in the 'complexity' series, although not to the extent of the other *O*'s; at least the low position of her curve at the seconds would seem to warrant such a statement. It is matter for regret that circumstances made it impossible for F to complete the 'complexity' series.

CHART 3

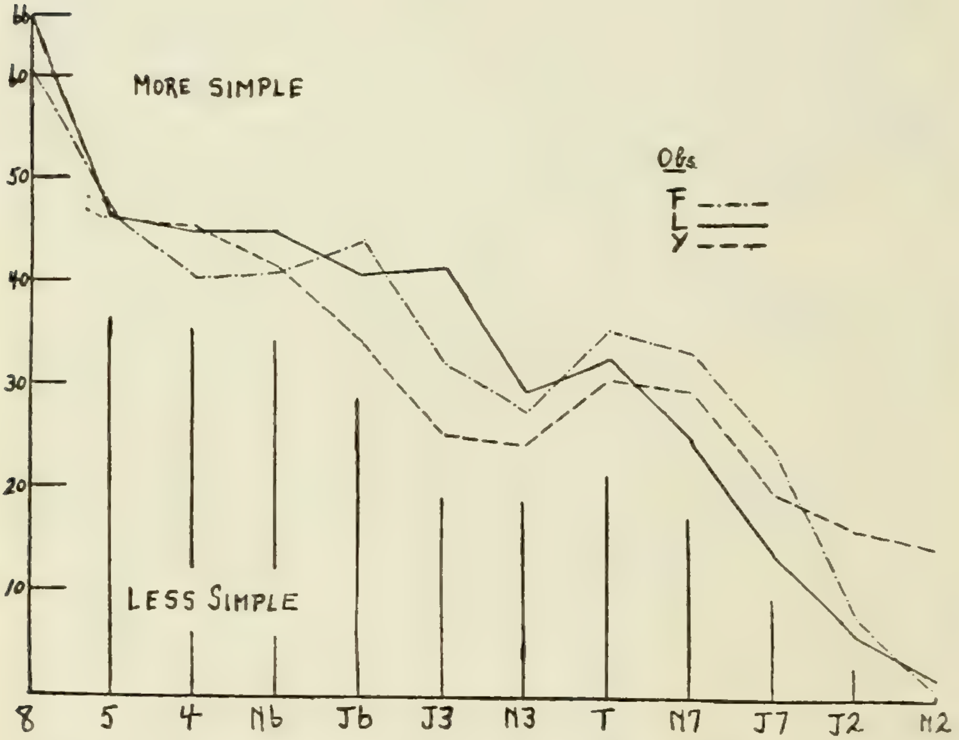

CHART 3. Rank-order of tonal intervals under instructions for 'simplicity' (see text). Curves for three *O*'s. 'Simplicity' curves for two *O*'s are shown in Chart 4.

Regarding the high positions of the tritone and minor seventh in the 'simplicity' series, adverted to above, it may be that this very factor of attitude is operative. In the 'smooth-rough' series the tritone and minor seventh occupy relatively high places, and since 'roughness' was part-mediator for the meaning of 'less simple,' it is reasonable to suppose that 'smoothness' may have entered in at times to support the meaning of simplicity, and thus give to the tritone and minor seventh values higher than would otherwise have been the case. It is not clear, however, why for two of the *O*'s the 'simplicity' series gave curves like the 'complexity' curves. A valid explanation can be sought only in repeated experiments under more stringent control of instructional determinants.

CHART 4

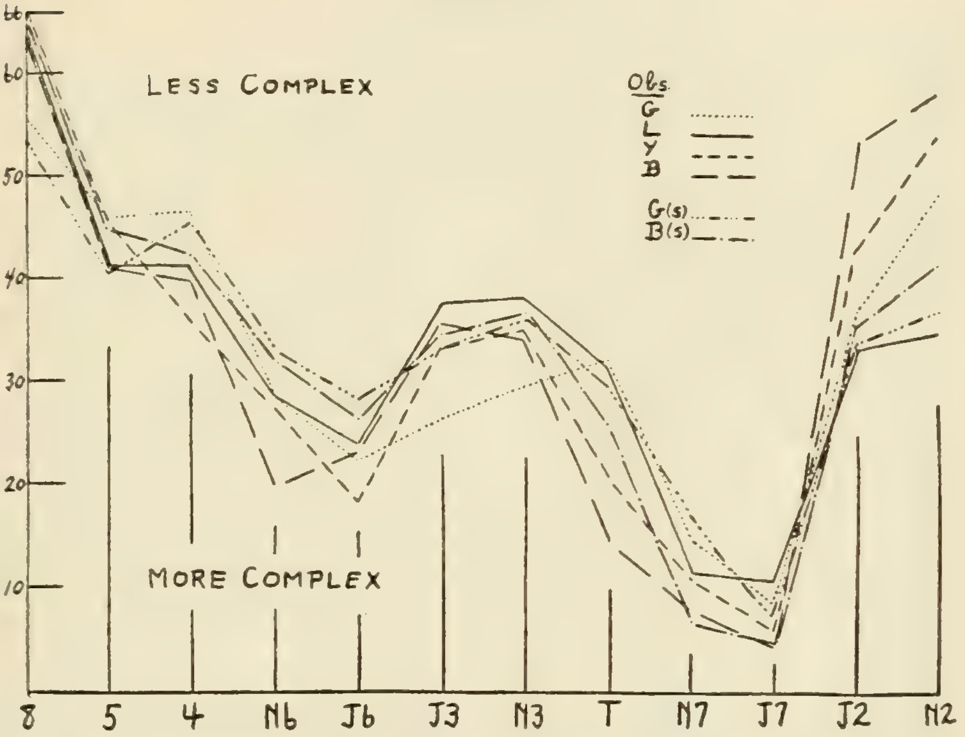

Chart 4. Rank-order of tonal intervals under instructions for 'complexity' (see text). The curves for G(s) and B(s) are 'simplicity' curves (see Chart 3). The four 'complexity' curves are inverted.

Pleasantness and Unpleasantness.—Although in the preliminary experiments, the *O*'s had stated that pleasantness and unpleasantness entered but seldom as disturbing criteria of judgment, it was deemed advisable to run through a series of *P* and *U* in order to discover just how judgments would come out under an affective determination. The instructions were similar to previous ones with the exception that *O* was requested to judge in terms of *P* and *U* only.

Under the conditions of the present experiment, *P* and *U* turn out to be meanings for all *O*'s,[24] or at least experiences not *sui generis.* For F and Y, the introspective reports state the potency of 'smoothness' and 'roughness' as mediating factors in the affective judgment, a fact borne out by the resemblance of the curves of F and Y (Chart 5) to the 'smooth-rough' curves. The agreement of these two curves and the agreement of the other three curves, and the marked difference between the two types, are worthy of especial note.

CHART 5

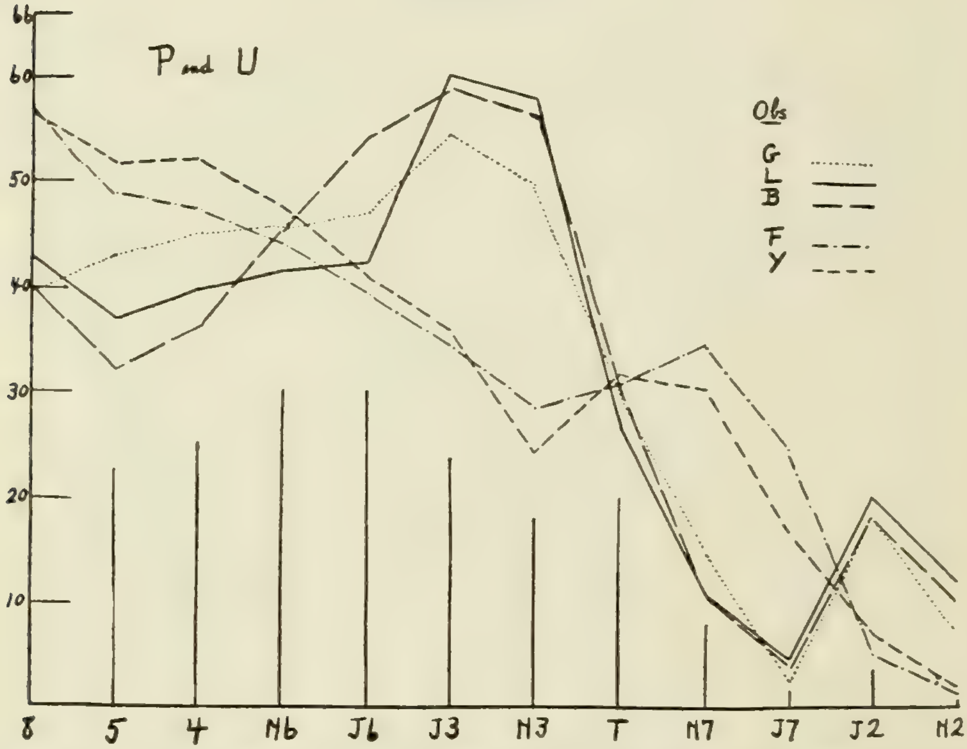

CHART 5. Rank-order of tonal intervals under instructions for *P* and *U*. Curves for five *O*'s.

For G, L, and B, whose curves represent the second type, the *P* and *U* judgments are explicitly reported as being on the level of meaning. Just what the sensory mediators for the judgments represented by these curves may be can not be satisfactorily gleaned from the introspective reports, although hints are not wanting to point to non-auditory concomitant processes of the auditory experience as playing a part. (See below: the "horrisonorous" curve for G, the "rotundity" and "hard-soft" curves for L, and the "kinaesthesis" curve for B.) Perhaps the most interesting feature of these curves is their consistency with musical usage: the sixth and thirds are very pleasant; the minor seventh and seconds are unpleasant and the major seventh very unpleasant; the octave, fifth and fourth are rather "indifferent."

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[24] For a recent exposition of the resemblance between affection and meaning, see M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, *Amer. J. Psychol.,* 1921, 32, 357ff.

So far the judgments under varying attitudes have come from instructions the purport of which was to determine *O* in the direction of purely auditory aspects of the total contents set up by the stimulus-situation. In the qualitative analyses of the preliminary experiments, however, there were numerous references to non-auditory concomitant processes, or to integrations of auditory and non-auditory processes, which presented themselves as important parts of the whole impression or as strong candidates for judgment in the case of unitariness. For the sake of greater completeness, therefore, although an exhaustive study of all possible factors in the *A, B, C*

CHART 6

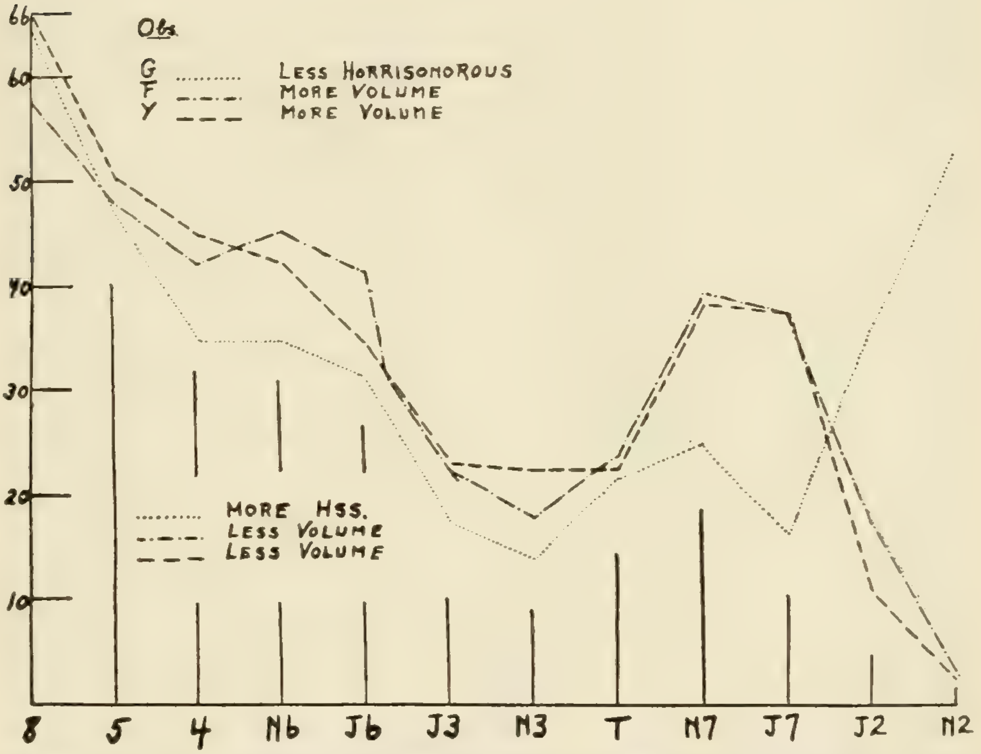

CHART 6. Rank-order of tonal intervals under criteria into which entered integrations of auditory and non-auditory processes (see text). Curves for three *O*'s. The curve for G is inverted.

. . . system was of course out of the question in the time at our disposal, it was decided to follow through certain of the more insistent contextual processes.

Integration of Auditory and Non-auditory Processes

The curves of Chart 6 represent rank orders from judgments based on an intimate integration of auditory and non-auditory processes. The criterion of this sort followed through by F was integration of the voluminous aspect of the auditory impression and a concomitant chest-kinaesthesis of varying degrees of expansiveness; a similiar criterion by Y, an integration of the same voluminous factor and a concomitant visual process between the two extremes of which a gap of varying width could be observed. In addition to the close uniformity of the two curves there should be noted also the factor of distance of components on the tonal scale, which is again operative: the low position of the thirds and seconds in which the paired components are close together, and the high position of the sevenths and sixths in which the paired components are farther removed from each other. Otherwise the form of the curves is similar to that for the 'simplicity' series. The criterion adopted by G in this group was an integration of auditory and kinaesthetic processes to which, for the sake of convenient reference, he originally gave the term "horrisonorous." By this term he meant the vibrant, rather unpleasant, factor in the auditory experience (*horridus*) together with the auditory fulness and richness of the impression. Although on closer observation G became convinced that the pattern of "horrisonorousness" consisted of an intimate integration of ear-kinaesthesis and auditory quality which could only be separated under careful attentive analysis, he still retained the term. The form of G's curve is similar to those for F and Y, and what has been said concerning these latter is equally true of the former. The one exception, in the case of the seconds, is explained in G's introspections by the fact that with the coming in of marked roughness the "horrisonorous" factor is 'swamped.' (The curve has been inverted on the chart.) Were it not for this rapid rise of the curve at the seconds, the resemblance to the other two curves would be still greater, inasmuch as a lowering of the curve at one point would result in an elevation at another, the essential event in this case for bringing about a greater agreement.

Concomitant Non-auditory Processes

Chart 7 gives the curves for judgments based on purely non-auditory concomitant processes. L, who is of a marked visual type, was able to make judgments on various aspects of what she called a 'pseudo-visual' image, *i. e.*, kinaesthesis of eye-movement which served as an effective surrogate for the actual visual experience. By 'rotundity' she meant the fulness and size of the visual image; by 'hard-soft,' the texture of the image; and by 'noise component,' the jagged or even contour of the image. Y followed through a criterion which he called visual *Zusammenfliessen,* by which he meant the pulling together or apart of concomitant visual images in the field of vision. By 'head-kinaesthesis' B referred to varying degrees of extension of concomitant kinaesthesis across the posterior portion of the head. It can be seen at a glance that there is less agreement among the curves of Chart 7; nevertheless a marked directional uniformity is apparent at the

CHART 7

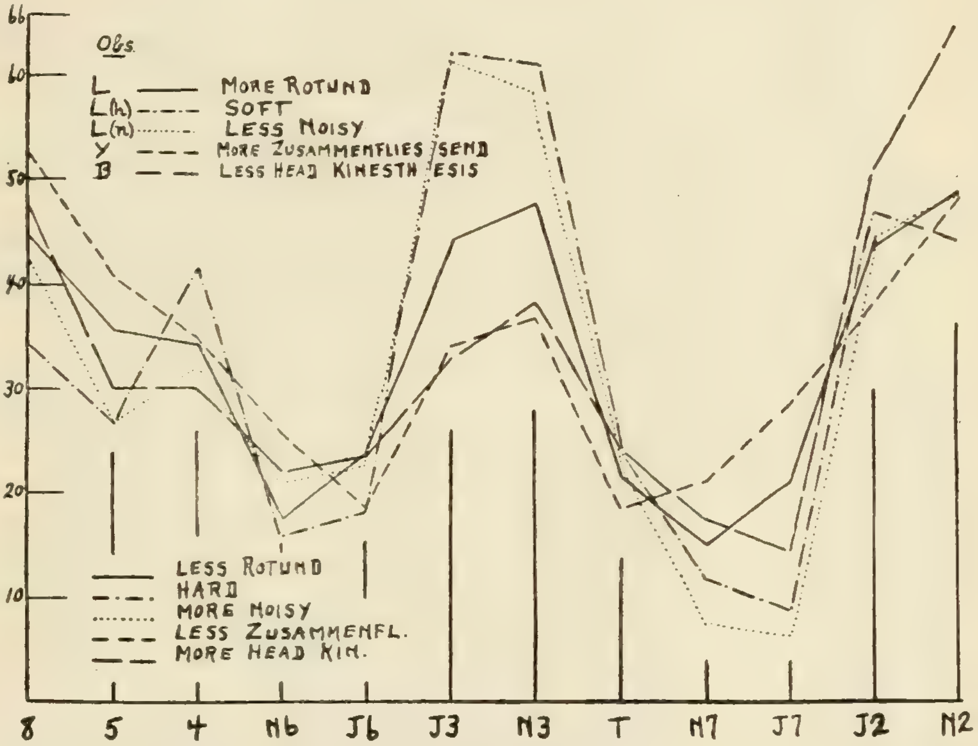

CHART 7. Rank-order of tonal intervals under criteria into which entered concomitant visual and kinaesthetic processes (see text). Curves for three *O*'s. The curves for L(h), L(n), and B are inverted.

TABLE I

Frequency of Judgments Upon Tonal Intervals Under Various Criteria. Method of Paired Comparisons. Column at Left Refers to Particular Criterion of Judgment. See Charts and Text. Abbreviations: 8 = Octave, 5 = Fifth, 4 = Fourth, J = Major, N = Minor, T = Tritone

Criterion	Obs.	8	5	4	N6	J6	J3	N3	T	N7	J7	J2	N2
Chart 1 Unitariness	G	61½	52	49	41	34	29	29½	28½	31½	20½	11½	8
	F	64½	55	47½	45½	41½	32	20½	27½	31½	22	7½	1
	L	66	54½	46	31½	31	40	39½	27½	30½	18½	8	3
	Y	66	55½	45	44½	45½	23½	23	25½	32	28	7½	0
	B	65½	54	50½	33½	41½	35½	34½	28	28½	16½	6	2
Chart 2 Smooth-Rough	G	48	44½	41	42½	44½	35	21½	40	39	31½	8½	0
	F	63	47½	46	45½	46	28½	15½	35	32½	25½	9	2
	L	64	51	48½	40½	40½	35	21	44½	24½	18	7½	1
	Y	64	52½	40½	47	40½	26½	13½	43	35	25	8½	0
	B	65½	51½	43½	45½	43	33	23½	39	27	17½	5	2
Chart 3 Simplicity	F	60½	46½	40½	41	44	32½	27½	35½	33½	24½	8½	1½
	L	66	46½	45	45	41	41½	29½	33	25	14	7	2½
	Y	66	46	45½	41½	34½	25½	24½	31	30	20	16½	15
Chart 4 Complexity	G (s)	53½	40½	45½	33½	28½	33½	36	29½	17	7½	34	37
	B (s)	65	45	42	32	26½	34½	36½	25½	7	5	35½	41½
	G	55½	46	46½	28½	22½	26½	29½	32	15	9	37	48
	L	64	41	41	28½	24	37½	38	31½	11½	11	33½	34½
	Y	66	45½	35½	27½	18½	33½	35	20½	11	6	43	54
	B	63½	41	40	20	23	35½	34½	14½	8	4½	53½	58

Chart 5 *P* and *U*	F	57	49	47½	44	39½	34½	28½	30½	34½	24½	5	1½
	Y	56½	51½	52	47½	41	35½	24½	31½	30½	16½	7	2
	G	39½	43	45	45½	47	54½	49½	29½	15	2½	18	7
	L	43	37	39½	41½	42½	60	58	27	11	4½	20	12
	B	40	32	36	45½	54	59	56½	30½	11	3½	18	10
Chart 6 Auditory and Non-auditory Integrations	G	64½	47	35	35	31½	17½	14	21½	25	16½	36	52½
	F	57½	48	42½	45	41½	22½	18	23½	39½	37½	17½	3
	Y	66	50½	45	42½	34½	23	22½	22½	38½	37½	11	2½
Chart 7 Non-auditory Processes	L	44½	35½	34	17½	23½	44	47½	21½	15	21	43½	48½
	L (h)	34	27	41½	16	18	62	61	24	12	9	47½	44
	L (n)	43	27	32	21	23½	61½	58	23	7½	6½	44½	48½
	Y	52½	40½	34½	25½	18½	34	36½	18½	21	28½	38	48
	B	47½	30	30	22	23	33½	38	24	17½	14½	51	65

position of certain of the intervals. As in Chart 6 the 'distance' factor is operative here, although in the opposite direction: the thirds and seconds (intervals whose components are proximate) rank relatively high, and the sevenths and sixths (distant components) low. Unusually low positions of the octave, fifth, and fourth are undoubtedly due to the violence with which the 'distance' factor intrudes upon the 'fusion' factor: note the extremes attained by the thirds and sevenths.

Conclusions

The numerical data from which the charts have been drawn are summarized in Table 1. The fractional numbers there derive from the usual procedure in the method of paired comparisons of giving equal value (*i. e.*, ½) to each member of a pair in an equality judgment.

The results indicate clearly the complexity of the tonal impression that is dependent upon the presentation of bitonal stimuli. Various factors which make for this complexity may be isolated by means of carefully controlled instructional determinants and examined apart. Such isolation and examination as the present study has undertaken bring to view certain of the phenomenological characteristics which constitute the foundation of bitonal impressions.

Smoothness-roughness.—The continuum to which the expression 'smoothness-roughness' applies is best described in terms of 'roughness.' By 'roughness' the *O*'s meant a temporal-intensive pattern in which various rates of intensive oscillations of the auditory experience are observable. 'Roughness' is inversely proportional to the rapidity of intensive oscillations. 'Smoothness' is the absence, or relative absence, of intensive oscillations. Absolute 'smoothness' is a continuation in time of the auditory experience without observable variations in intensity.

Simplicity.—By 'simplicity' the *O*'s intended the phenomenon of unanalysability. The introspective reports show that by unanalysability they meant the covariation in sensory clearness of the auditory components under an active attitude (=attentional analysis) by virtue of which isolation of one component becomes difficult, that is to say, occurs only incompletely or only after a considerable interval, or else, on occurring, persists only for a short time. The tonal complexes are judged increasingly less 'simple' as the variation in clearness of the components becomes more independent.

Complexity.—For judgments of 'complexity' the *O*'s assumed a more passive attitude, *i. e.*, an attitude for judging upon the 'complexity' of the interval as it appeared *during presentation,* rather than an active attempt at attentional analysis. From the introspective characterizations it becomes apparent that the *O*'s understood by 'complexity' the degree of disparity (nearness or remoteness on a scale of 'sense-distance') of the auditory pitch qualities as they appear in the tonal impression. Those intervals in which the components are readily perceived as disparate with respect to pitch quality are judged as 'complex,' whereas those intervals in which the pitch qualities are less readily perceived as separated or dual are judged as less 'complex.'

From a consideration of Chart 4 it becomes apparent that 'complexity' varies with physical distance: those complexes in which the components lie further apart on the tonal scale are judged more 'complex' than those complexes in which the components are proximate. Exceptions occur, however, in the case of the fourth, fifth, and octave. It would seem that the qualitative disparity of these intervals is obscured by a greater covariation in clearness of the components, the factor which makes for 'simplicity.' That is to say, processes which ordinarily lead to the judgment 'complex,' are cut across in the case of the octave, fifth, and fourth by more insistent processes leading to the judgment less 'complex'=more 'simple.' It should be noted that in the characterizations of 'simplicity' all the *O*'s except one had occasion to speak of those intervals in which covariation in clearness made analysis difficult as "stuck together," "glued together," "well knit," "well fused," "hanging together," *etc.* Such characterizations of the impressions, together with the fact that covariation in clearness cuts across and obscures qualitative disparity, make plausible the statement that covariation in clearness is related to a true perceptual 'fusion' of tones, *i. e.,* fusion is an integration of attributes of clearness rather than a qualitative integration.

Pleasantness and Unpleasantness.—None of the *O*'s based their affective judgments upon *P* and *U per se.* In all cases the judgments were of the nature of attitudinal or 'meaning' responses. For two *O*'s *P* and *U* turn out to have their sensory bases in 'smoothness' and 'roughness.' For convenience of reference the affective preferences of these two *O*'s may be designated as judgments of 'smooth' *P* and *U*. The other three *O*'s, whose affective preferences accord more with musical usage, were unable to give any thoroughgoing characterization of the sensory mediators for *P* and *U*. In lieu of a better term their affective judgments may be referred to as 'musical' *P* and *U*.

Volume.—Two *O*'s found it possible to characterize the intervals in terms of 'voluminousness' or 'fulness.' It is evident from their descriptive reports that 'volume' for them is not a purely auditory affair, but an integration of auditory process with certain contexts which attach to the intervals. For one *O* 'volume' is an integration of auditory process and chest-kinaesthesis; for the other, an integration of auditory and visual processes.

Horrisonorousness.—By this term one *O* meant an intimate integration of auditory quality and ear-kinaesthesis into a closely knit pattern of vibrant qualitative roughness. Originally, in the preliminary series, this *O* noted that 'horrisonorousness' was frequently affectively toned. The agreement of this *O*'s affective preferences with certain of his judgments on 'horrisonorousness' makes it possible that the latter and the sensory basis of 'musical' *P* and *U* have something in common.

Visual and Kinaesthetic Processes.—Three *O*'s followed through series in which judgments were made upon concomitant non-auditory processes alone. Terms such as 'rotund,' 'soft,' and 'noisy' refer to the size, smooth contour, and jagged texture of concomitant visual processes; 'zusammenfliessend' to the pulling together or apart of visual imagery; and 'head-kinaesthesis' to the spread of kinaesthetic process in the posterior portion of the head. (See Chart 7.)

It is now possible, with the aid of the foregoing terms, which stand for definite sensory experiences ('musical' *P* and *U* excepted), to characterize every interval separately with respect to various factors in the total impression which contribute to its constitution.

Octave.—The octave occupies an unique position among the tonal intervals in that it lies at one extreme of three of the attributive continua within which all intervals vary. It is the 'smoothest,' 'simplest,' and least 'complex' interval, in the sense of the discussion of the preceding paragraphs. With respect to 'smooth' affective judgments the octave is also the most pleasant interval. *O*'s whose affective preferences are of the 'musical' kind, however, find the octave about as pleasant as the fourth and fifth and less pleasant than the sixths and thirds.

By some *O*'s the octave was also characterized as giving a more 'voluminous' impression than the other intervals.

Fifth.—This interval resembles the octave in that it is marked by an almost complete absence of 'roughness.' One *O*, in fact, judged the fifth as practically equal to the octave in 'smoothness.' The components of the fifth admit of more independent variation in clearness than do the components of the octave, that is to say, the fifth is slightly less 'simple.' This factor of 'simplicity' tends to predominate over the wide 'sense-distance' included in the fifth, so that the interval appears as less 'complex' than any of the other intervals except the octave. Under a 'musical' affective determination the fifth is slightly less pleasant than the octave except for one *O* who finds it somewhat more pleasant than the octave. The fifth is also less pleasant than the octave for those *O*'s whose affective judgments are mediated by 'smoothness.'

The contextual aspect of 'volume' when reported was more prominent in the fifth than in any other interval except the octave.

Fourth.—With respect to 'smoothness' the fourth is on an equal plane with the sixths, which are a little less 'smooth' than the fifth. In this interval the 'simplicity' factor again appears to obscure the 'complexity' aspect, so that only the fifth and octave are judged less 'complex' than the fourth. The fourth, fifth, and minor sixth all appear about equally 'simple.' 'Musical' affective judgments place the fourth before the fifth and the octave in *P;* 'smooth' affective preferences place the octave and fifth before the fourth in *P*.

For G, who judged upon a qualitative pattern of integrated auditory process and ear-kinaesthesis, the fourth assumed a trace of 'horrisonorousness.' The 'voluminous' characteristic, when it was observed, entered into the fourth to the same extent as in the sixths, which are exceeded in 'volume' only by the fifth and octave.

Minor Sixth.—The limits which various characteristics may reach under attitudinal changes vary more between intermediacy and extremes in the minor sixth than in the three preceding intervals. The fourth and both the sixths are very 'smooth' intervals and are exceeded only by the fifth and octave in this respect. The remoteness of the pitch qualities in the minor sixth lends to the interval an aspect of 'complexity' which is more marked only in the major sixth and the sevenths. On the other hand, under an active attitude of attentional analysis, the relative lack of independent variability in clearness of the components leads to the judgment 'simple' in the case of three *O*'s, that is to say, perceptual 'fusion' is more insistent than qualitative disparity. 'Musically' the minor sixth is more pleasant than any of the preceding intervals. When mediated by ' smoothness,' however, it is less pleasant than the preceding intervals.

For two *O*'s, only the octave and fifth exceeded the minor sixth with respect to ' voluminousness.' G found ' horrisonorousness ' present in this interval to the same extent as in the fourth. For certain *O*'s contextual and kinaesthetic processes became palpable in the minor sixth to an extent which would tend, under an equivocal determination, to obscure the purely auditory aspects of the impression.

Major Sixth.—The qualitative disparity of the components of the major sixth is even more marked than in the minor sixth, giving thus to the interval an aspect of ' complexity ' exceeded only by the sevenths. Under the determination for ' simplicity,' however, three *O*'s find but little difference in ' simplicity ' between the major sixth and the minor sixth and fourth. The major sixth is on equal terms with the minor sixth and fourth with respect to ' smoothness.' Only the thirds are pleasanter than the major sixth under a ' musical ' affective determination. The other affective judgments place the major sixth just below the preceding intervals in *P*.

' Volume ' was somewhat less and ' horrisonorousness ' slightly more apparent in the major sixth than in the preceding intervals, for the *O*'s who found these contexts. As in the minor sixth, kinaesthetic and visual processes may have come in to obscure the auditory processes in the major sixth.

Major Third.—The relative position with respect to the other intervals of the major third, and also the minor third, varies considerably under attitudinal changes. Between the major third and the intervals already mentioned there is a marked difference in ' smoothness.' Only the minor third and the seconds are ' rougher ' than the major third. Only the octave, fifth, fourth, and seconds are less ' complex,' and the octave, fifth, fourth, and sixth more ' simple,' than the major third. The relative ' simplicity ' and ' non-complexity ' of this interval are presumably not due so much to the covariation of clearness and the parity of pitch qualities of the components as to the obliteration of these processes by the rapid intensive variations of the temporal ' roughness ' pattern. The major third is the most pleasant of all the intervals for those *O*'s whose affective judgments have a ' musical ' mediation, whereas it is " indifferently " toned for the two other *O*'s.

The auditory-kinaesthetic context of ' horrisonorousness ' was very prominent in this interval for G. For F and Y the ' voluminous ' context of the major third was almost lacking. The auditory factors of the major third may also have been considerably obscured by visual and kinaesthetic concomitant processes.

Minor Third.—With the exception of the preceding interval, the minor third is the most pleasant interval for those *O*'s whose affective judgments are of the 'musical' sort. The interval is rather unpleasant for those *O*'s whose affective preferences are mediated by 'smoothness.' One of the most striking characteristics of this interval is the prominence of the factor of 'roughness.' Only the seconds, and for two *O*'s the major seventh, are 'rougher' than the minor third. As with the major third, the temporal-intensive pattern of 'roughness' seems to blot out the separate auditory qualities with the result that the minor third is judged as relatively 'non-complex.' Three *O*'s find the minor third, however, less 'simple' than any of the preceding intervals because of a shift to a 'meaning' attitude in the presence of the 'rough' intervals; that is to say, intervals in which oscillations of intensity are very prominent are cognized as 'non-simple' by virtue of this very factor of 'roughness.'

'Horrisonorousness' was more prominent in the minor third than in any other interval for G. Concomitant non-auditory processes manifested themselves to a great extent in the minor third.

Tritone.—In none of the aspects which enter into tonal impressions does the tritone seem to occupy an extreme position with respect to the other intervals. Perhaps the most striking characteristic of the interval is its 'smoothness.' Only the group of intervals comprising the octave, fifth, fourth, and sixths, is smoother than the tritone. The qualitative disparity of its components is sufficiently marked to cause the interval to be judged as more 'complex' than the octave, fifth, fourth, thirds, and seconds, though not so 'complex' as the sixths and sevenths. For three of the *O*'s the tritone appears somewhat more 'simple' than the thirds. For all the *O*'s the tritone is "indifferently" affective.

G found 'horrisonorousness' rather well marked in the tritone, although not to so great an extent as in the thirds.

Minor Seventh.—The 'roughness' of the minor seventh is not very obvious, although, of course, the interval is not so 'smooth' as the tritone and the group of intervals already mentioned. It is about equal in 'roughness' to the major third. The qualitative disparity in pitch of the components of the minor seventh lends to the interval an aspect of 'complexity' only exceeded by the major seventh. Three *O*'s under a determination for analysability, however, find the minor seventh rather 'simple,' *i. e.,* the independent variability in clearness of its components is not very marked. The interval is unpleasant for *O*'s whose affective judgments are mediated by 'smoothness' as well as for those whose judgments are 'musically' mediated, although less pleasant for the latter than the former.

O's, who judged on the basis of 'volume,' found the minor seventh very 'voluminous.' Other contextual processes were also apparent to a noticeable extent for some *O*'s.

Major Seventh.—With the exception of the seconds, and for three *O*'s the minor third, the major seventh is the 'roughest' interval. It is also the most 'complex' interval. Only the seconds, when cognized as 'non-simple' under a 'meaning' attitude, are judged less 'simple' than the major seventh. The interval is very unpleasant for all *O*'s. For the 'musical' type it is the most unpleasant, while for the 'smooth' type the seconds are somewhat more unpleasant. It can be seen that the extreme position which the major seventh occupies within certain continua tends to give it a unique status antipodal to the octave.

Only the thirds exhibited greater 'horrisonorousness' than the major seventh. For some *O*'s visual and kinaesthetic processes and the contextual 'voluminous' aspect entered very prominently into this interval.

Major Second.—Unquestionably the most striking feature of the major second lies in the violent intensive oscillations constituting the 'roughness' pattern. Only the minor second possesses this characteristic in a more pronounced degree. For all *O*'s the major second is very unpleasant. Under a 'meaning' attitude the seconds are cognized as the least 'simple' intervals; but under a strictly descriptive attitude they scarcely submit to psychological separation at all, *i. e.,* the auditory qualities can not be singled out in the face of the violent intensive variations which dominate the seconds to so great an extent.

The contextual factor of 'volume' dropped out in the seconds, as did also 'horrisonorousness.' The interval was rich, however, in kinaesthetic and visual processes.

Minor Second.—This interval is the 'roughest' of all. Under an affective determination of the 'smooth' type it is the most unpleasant interval. With respect to other aspects the minor second is similar to the major second: under a 'meaning' attitude an *O* knows that a violently 'rough' interval must be 'non-simple;' but under a descriptive attitude he judges it as 'non-complex,' since the component qualities can not be separated out.

From the charts and the immediately preceding characterizations of the intervals it can be seen that the rank-order of certain bitonal complexes is more affected by change of attitude than is the rank-order of others. The thirds and sixths, *e. g.,* undergo greater alteration in position under varying instructional determinants than do the octave, fifth, and fourth. The seconds shift position very decidedly in the presence of a descriptive attitude as against a 'meaning' attitude; but since these differences of attitude are of a different order from those brought about by the more readily effective instructional determinants, the variation of the seconds may be left out of account in the present consideration. In the literature upon tonal fusions there is considerable agreement regarding the ranks of the octave, fifth, fourth, and seconds;[25] but the sixths, thirds, and sometimes the minor seventh, are variously placed by different experiments. In the present experiment the results of the 'unitariness' series (Chart 1) show a like agreement among *O*'s of the octave, fifth, fourth, tritone, sevenths, and seconds, and a considerable divergence for the thirds and sixths. The preceding discussion has shown that the sixths and thirds vary in rank more than the other intervals under change of instructional determinant. The difference becomes apparent if the averages of all the intervals of all the series, except the 'unitariness' series, are taken and their M. V.'s computed, and the intervals arranged in order in terms of decreasing size of M. V. This order is as follows: minor third, major sixth, minor sixth, major third, minor seventh, major seventh, octave, fifth, tritone, fourth, and seconds. It seems to indicate that the possibility of equivocal determination of judgment is greater for the sixths and thirds, and occasionally for the sevenths, than for the other intervals. This fact may explain the lack of agreement in the literature concerning the rank-order of these particular intervals, since the instructional limitation of an *O* to a particular sensory criterion is extremely difficult at best, and, in the absence of a previous knowledge of the phenomenological criteria available in tonal intervals, almost impossible.

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[25] See *e.g.,* R. Schulze, Ueber Klanganalyse, *Philos. Stud.,* 1898, 14, 483; Stumpf, *Tonpsychologie,* 1890, ii, 135; A. Meinong and St. Witasek, Zur experimentellen Bestimmung der Tonverschmelzungsgrade, *Zeits. f. Psychol.,* 1897, 15, 191, 193; A. Faist, *ibid.,* 109-113; Kemp, *op. cit.,* 186-190; Pear, *op. cit.,* 66.

ON ARTERIAL EXPANSION

By G. N. Hartman and D. L. McDonough, University of Pennsylvania

Such a great deal of experimentation has been published on plethysmographic work that some time ago Leschke[1] attempted to reach some conclusions by submitting the results of all these studies to a statistical treatment. All of this work, however, has been qualitative; the authors have sought to determine whether the volume of the blood at the periphery increased or decreased. We are unable to find any study in which the amount of increase or decrease has been quantitatively studied. Such a quantitative determination is the object of the present study.

A Lehmann plethysmograph was employed to the glass tube of which an arbitrary scale was attached. The volume of the body of the plethysmograph and of each division of the scale was determined. When the subject was placed in the instrument, the water was taken from a large graduated tube. The volume of the arm was thus determined by subtracting the volume of the water used from the total volume of the plethysmograph.

Three series of determinations were made on each of 24 adult subjects. (1) A Stanton sphygmomanometer was placed above the elbow of the arm used. The sac was then inflated until the diastolic pressure was obtained. In this case the veins were closed while the arteries remained open, and hence the blood backed up in the forearm expanding the arteries to the limits of their elasticity. After this determination the sphygmomanometer was removed. (2) The subject was then instructed to hold his breath for as long a time as possible and a second determination was made. (3) The third determination was made during mental work; mental multiplication of a three-digit by a two-digit number being sufficient, in most cases, to keep the subject concentrated for a period of several minutes. At the beginning of each determination, the reading of the height of the column of water in the tube was recorded, as was also the *greatest* extent of change during the experiment.

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[1] Leschke, E., Die körperlichen Begleiterscheinungen seelischer Vorgänge, *Arch. f. d. ges. Psychol.*, 21, 1911, 435ff. Die Ergebnisse und die Fehlerquellen der bisherigen Untersuchungen über die körperlichen Begleiterscheinungen seelischer Vorgänge, *ibid.*, 31, 1914, 27ff.

Subject	Volume of Arm in C. C.	Volume Change Diastolic in C. C.	Volume of Change in C. C. Holding Breath	Volume of Change in C. C. Mental Work	% of Change Diastolic	% of Change Holding Breath	% of Change Mental Work
1	708	12.5	0.0	0.0	1.77	0.00	0.00
2	732	7.5	—1.2	—2.5	1.02	0.17	0.34
3	1103	17.5	0.0	0.0	1.59	0.00	0.00
4	840	5.0	—5.0	—1.2	0.60	0.60	0.15
5	845	15.0	—1.2	—1.2	1.77	0.15	0.15
6	808	10.0	0.0	—1.9	1.24	0.00	0.23
7	888	16.2	0.0	1.2	1.83	0.00	0.14
8	1111	8.8	—3.1	0.0	0.79	0.28	0.00
9	1400	17.5	—3.8	—2.5	1.25	0.27	0.18
10	773	5.0	—1.2	—1.2	0.65	0.16	0.16
11	830	21.2	—7.5	—1.2	2.60	0.91	0.15
12	810	11.2	—2.5	—1.2	1.40	0.31	0.16
13	985	45.0	—1.2	2.5	3.17	1.25	0.17
14	782	15.0	1.2	1.2	1.90	0.16	0.16
15	788	12.5	0.0	—1.2	1.60	0.00	0.16
16	908	22.5	—1.2	2.5	2.48	0.14	0.28
17	1022	17.5	—0.6	1.2	1.70	0.06	0.12
18	812	7.5	—0.6	—1.2	0.92	0.01	0.15
19	1045	16.2	—1.2	0.6	1.56	0.12	0.06
20	838	22.5	—2.5	—2.5	2.68	0.29	0.29
21	955	20.0	—2.5	—3.7	2.01	0.25	0.38
22	1030	37.5	10.0	—1.2	2.75	0.67	0.08
23	1078	30.0	—2.5	1.2	2.78	0.23	0.12
24	1142	25.0	2.5	3.7	2.19	0.22	0.03
Ave.	926	17.9	2.2	1.5	1.76	0.26	0.16

The results are found in the accompanying table. In the first column are indicated the subjects by number. In the next column the volume of the arm under normal conditions is recorded. In the next three columns are the volumes of change in c. c. respectively under the conditions of diastolic pressure, of holding the breath, and of mental work. A *minus*-sign indicates a decrease in volume. Inasmuch as the volume of the arm varies considerably for the different subjects, the actual amount of increase or decrease loses in significance when the different subjects are compared. Hence the percentage of change was calculated for the different experimental situations. These values are found in the last three columns of the table. The averages for the values in each column are found in the bottom row.

It will be noticed that the changes under the conditions of diastolic pressure are very much larger, on the average, than those for either mental work or holding the breath. The average for diastolic pressure is 1.76 *per cent.,* the greatest change being 3.17 *per cent.* (Subject 13), the smallest 0.60 *per cent.* (Subject 4). In the case of holding the breath, the average change is 0.26 *per cent.,* the greatest being 1.25 *per cent.* (Subject 13); while for Subjects 1, 3, 6, 7, and 15 no change was noted. For mental work, the average change was 0.16 *per cent.,* the greatest change being 0.38 *per cent.;* while for Subjects 1, 3 and 8 no change was noted. The change for diastolic pressure was greater than that for holding the breath in every case but one (Subject 4) where it was equal but in the negative direction. The change for diastolic pressure was invariably greater than for mental work. In 10 cases the change for holding the breath was greater than for mental work, in 6 cases it was equal, and in 8 cases less.

SUMMARY

(1) This study suggests a method for dealing with volumetric peripheral vascular changes in a quantitative way.

(2) Under our conditions we were unable to get increases in volume of blood in the forearm which approached the limits of elasticity of the vascular system.

(3) The combination of the sphygmomanometer with the plethysmograph appears to give a better determination of arterial elasticity than does the present medical clinical method.

FUNCTIONAL PSYCHOLOGY AND THE PSYCHOLOGY OF ACT: I[1]

By E. B. Titchener

§ 1. In the dichotomy by Extension, physical phenomena receive positive and psychical phenomena receive negative determination. In another dichotomy, which is perhaps even more familiar, this relation is reversed: the universe of experience is divided into the Conscious and the Not-conscious, and psychical are identified with conscious phenomena, mind with the totality of consciousness. Psychology thus receives at length a positive definition.[2]

The word 'consciousness' is, however, notoriously ambiguous;[3] and the question whether the adjective 'conscious' suffices to mark off a special class of phenomena must therefore be discussed with great care. It must, moreover, be discussed in two separate contexts. For the psychologists who recognise the independent existence of 'phenomena of consciousness,' as the given objects of psychological investigation, fall at the present time into two principal groups. The one of these emphasises, in a psychology of 'function,' the biological aspect of empirical psychology; the other, in a psychology of 'act,' emphasises its intentional aspect.[4] Both alike may therefore trace their descent from Aristotle.[5] But in spite of much that they hold in common, the difference of motivation makes it necessary to consider the two groups separately.

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[1] In writing a chapter on "The Definition of Psychology: Subject-Matter," for the introductory volume of a Systematic Psychology, I have been obliged to take critical account of functional psychology and the psychology of act. Since it seems fruitless to publish conclusions without giving the evidence on which they rest, and since at the same time a book such as I have planned is not the place for these criticisms of detail, I print my comments in the Journal. The present article deals with functional psychology; a following article will deal with the psychology of act. The first section of the text follows a discussion of the definition of mind, negatively, by "absence of extension" (Bain).

[2] Moreover, the Not-conscious may be further subdivided, in the interest of biology, into a Living and a Not-living. There can be no doubt that, formally regarded, this classification is superior to that by Extension.

[3] The *locus classicus* is A. Bain, *The Emotions and the Will,* 1880, 539ff. Cf. also R. Eisler, *Wörterbuch der philosophischen Begriffe,* i., 1910, 177ff.

[4] Functional psychology, in this sense, is especially American, and the psychology of act especially German. Recent English psychology, through G. F. Stout (*Anal. Psychol.,* i., 1896, 36, 40), has been influenced by Brentano.

In dealing with 'functional' psychology, we shall first examine a single representative system, and shall then bring together, for critical review, what appear to be the major tenets of the school. Systems that are based upon the concept of consciousness, even if this be taken under a single aspect, will inevitably differ; and we find accordingly that the biological emphasis falls differently in different functional psychologies. For Ladd, the 'stream of consciousness' is a life, and mind, the totality of consciousness, is an organism, "a unique and living totality in a course of development,"—an organism living a life of its own, which is always connected with the bodily life, but yet is "in some sort independent of" the body; and functional psychology is the science of the functions or activities of this psychical organism.[6] Angell, on the other hand, regards consciousness as an organic function,[7] a phenomenon of control;[8] "mind," he declares, "seems to involve the master devices" whereby the "adaptive operations of organic life may be made most perfect."[9] Judd, again, appears to combine both views. At first, consciousness is represented as an organic function, as something which, like the digestive or locomotive function, plays its part in the economy of the organic life under the conditions of the biological struggle for existence; it is thus one of the 'attributes' of man, considered as the highest animal.[10] Later, consciousness is 'possessed' by a knowing self, an unitary being which finds its nearest analogy in the living organism.[11] Differences of this sort find their natural expression in the classifications and arrangements and relative emphases of the writers' systematic works. But they have little if any bearing on the question immediately before us; they do not destroy the essential unity of the functional school.

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[5] Intentionalism is ascribed to Aristotle on the ground of his doctrine of the relation of sensation to object of sense (*De anima,* 424*a*, 425*b*), and of thought to the object of thought (*ibid.,* 429*a*, 430*a*, 431*b*). Cf. also his statement that the objects of memory and imagination (*De mem.;* 450a) and those of thought and desire (*De an.,* 433*a*; *Met.,* 1072*a*) are identical.

[6] G. T. Ladd, *Psychology, Descriptive and Explanatory,* 1894, 638, 659ff.; *Philos. of Mind,* 1895, 400ff., esp. 405; G. T. Ladd and R. S. Woodworth, *Elements of Physiol. Psychol.,* 1911, 656ff.

[7] J. R. Angell, *Psychology,* 1904, 79. In 1908, 95 the phrase is dropped, but the meaning of the passage appears to remain the same.

[8] Angell, "The Province of Functional Psychology," *Psychol. Rev.,* xiv., 1907, 88.

[9] *Psychol.,* 1908, 8. In 1904, 7, "mind seems to be the master device." Cf. also 1904, 50, 86 with 1908, 64, 103. In both editions mind is "an engine for accomplishing the most remarkable adjustments of the organism to its life conditions:" 1904, 379; 1908, 436.

[10] C. H. Judd, *Psychology,* 1917, 4, 161; "Evolution and Consciousness," *Psychol. Rev.,* xvii., 1910, 84.

The psychology of 'act' cannot be treated in the same comprehensive way. For here, as we quickly recognise, individual differences are not only inevitable but also fundamental and constitutive, so that the principal systems and programmes must be separately considered. Brentano, of course, furnishes both our starting-point and our constant point of reference. Besides Brentano, we pass under review, either in their own person or in that of some member of their school, Meinong, Stumpf, Lipps, Husserl and Külpe. We then interrupt the course of the exposition, in order to compare and contrast two experimental text-books of intentionalistic psychology, Witasek's *Grundlinien der Psychologie* (1908), which systematises Meinong's views, and Messer's *Psychologie* (1914), which we may regard as, in large measure, a systematisation of the later views of Külpe. The digression will be useful: it will reveal likeness and difference, in kind and in degree, and will thus prepare us for a broader survey of the act-systems. But here, we repeat, it proves impossible to follow the lines laid down by our study of functional psychology. We shall rather enquire, first, into the significance for these systems of the classification of psychical phenomena, and secondly into their treatment of two modes of such phenomena, namely, sensation and attention. The choice of topics is not arbitrary. It is suggested by the foregoing review of the principal systems; and it leads us to certain general conclusions with respect to intentionalism at large.

Ladd's Systematic Psychology

§ 2. We may take, as broadly typical of the functional systems, Ladd's definition of psychology: "the science which describes and explains the phenomena of consciousness, as such."[12] Consciousness, Ladd says, cannot in strictness be defined. Its meaning may, however, be brought out by contrast. "What we are when we are awake, as contrasted with what we are when we sink into a profound and perfectly dreamless sleep, or receive an overpowering blow upon the head—*that* it is to be conscious. What we are less and less as we sink gradually down into dreamless sleep, or as we swoon slowly away: and what we are more and more, as the noise of the crowd outside tardily arouses us from our after-dinner nap, or as we come out of the midnight darkness of the typhoid-fever crisis—*that* it is to become conscious."[13] For the rest, he commits his case to the "reflective mind of all mankind." "The distinction between external facts and facts of consciousness, as actually made by every man, furnishes . . . the one peculiar and abiding standpoint of psychology."[14]

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[11] *Psychology*, 274ff. Cf. below, p. 532.

[12] G. T. Ladd, *Psychology, Descriptive and Explanatory*, 1894, 1.

It would be foolish, certainly, to require a definition of the indefinable. If we are to deal with the indefinable, we can ask only that it be exhibited. We must insist, however, that it be exhibited clearly and unequivocally, in such wise (to put the matter a little paradoxically) that we can assure ourselves of what the writer's definition would have been, had he been able to formulate it. Ladd, as we have just now seen, attempts to exhibit the conscious by contrast with the not-conscious. We note, however, with some disquiet that—while psychology is to deal with the facts or phenomena of consciousness—it is 'we' and not these facts that are conscious, and that consciousness is made susceptible of degree, of less and more. If there is an universally realised difference between "external facts and facts of consciousness," in their status as data for scientific treatment, then surely the difference should be demonstrable from the face of the facts themselves, and 'we' who discern it should be left aside. Moreover, the facts, if they are data for scientific treatment, must surely either be or not be "facts of consciousness," and cannot vary their nature by more and less. Man is not more biological than amoeba, or the mountain brook less physical than Niagara.

Ladd's illustrations are therefore by no means free from objection,—so far, at any rate, as regards a science of "the phenomena of consciousness, as such." We must, however, take them as they stand, and see what Ladd makes of them when he comes to technical exposition.

A man of ordinary education and intelligence, looking back on his recovery from swoon or fever-crisis, would, no doubt, be likely to report a gradual restoration of 'consciousness:' by which he would mean an increasing awareness, an increasing realisation and appreciation and command, of himself and his surroundings. It is precisely this varied awareness, now, that Ladd intends by his own technical use of the word 'consciousness.'[15] Every 'state of consciousness' with which a scientific psychology has to do is, he tells us, at one and the same time "fact of intellection, fact of feeling, fact of conation."[16] Intellection is evidently a mode of awareness: whether intellection mean that universal discriminating activity which has its root in the awareness of resemblance,[17] or whether it be taken to embrace the whole series of psychoses that runs from the abstract 'sensation' through perception and ideation to judgment.[18] In so far, then, as every state of consciousness involves intellection, consciousness is always, in this narrower intellectual sense, awareness. Feeling too, however, is a mode of awareness; it is our very being become aware;[19] and conation also is a mode of awareness, awareness of activity.[20] Every state of consciousness, therefore, is at once fact-awareness, value-awareness and activity-awareness, —the unitary awareness-resultant of three irreducible awareness-factors.[21]

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[13] *Ibid.*, 30.
[14] *Ibid.*, 3.

Ladd thus obtains a starting-point for his system: but he has not adhered rigorously to his illustrations. For these states of consciousness, which are the primary data of psychology, while they all belong to some 'self' or 'I,' and indeed cannot be 'thought of' out of that connection, yet do not necessarily carry the reference to 'self' within or upon them; their consciousness is not necessarily a self-consciousness.[22] The convalescent of Ladd's illustrations, on the other hand, would assuredly maintain that 'he' became increasingly conscious, 'he' and not his 'states.' It is the observer who may become increasingly aware: the 'state' or 'fact' or 'phenomenon of consciousness' is, as we saw, simply conscious, and cannot become increasingly or decreasingly what it essentially is.

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[15] The two terms are not seldom used interchangeably. See, *e.g.*, *op. cit.*, 166, 290, 293ff., 296f., 300, 310, 322, 328f., 331ff., 379, 422, 517, 523f., 530f., 636; Ladd and Woodworth, *Physiol. Psychol.*, 430, 512, 681, 685. They appear, indeed, from such passages as *Psychol.*, 11, to be strictly coordinate.

[16] *Psychol.*, 33, 58, 172, 264, etc.

[17] *Ibid.*, 33, 288ff., esp. 293.

[18] *Ibid.*, 93, 235, 251, 357, 430, etc.

[19] *Ibid.*, 170. Cf. the doctrine that feeling may precede or outlast ideation (181); the illustrations offered in support of a manifold of affective qualities (*e.g.*, 170); and the doctrine that feeling is an integral factor in (not merely a determinant of, or an influence upon) knowing (53, 510ff.; *Philos. of Knowledge*, 1897, 95, 124, 165f.).

[20] *Psychol.*, 83, 216, 219; *Philos. of Mind*, 1895, 87ff.

[21] 'Fact' must be understood, not in the sense of our own previous discussion [here not printed], but rather in the manner of common sense; cf. the illustrations, *Psychol.*, 17, 19, 50f., etc. For value, cf. *Philos. of Knowledge*, 124.

[22] *Psychol.*, 31f., 523.

We are, however, not yet at the end of Ladd's account. The state of consciousness which is to be the subject-matter of psychology must, he points out, do more than merely exist; it must become an 'object of knowledge.'[23] But it can become an object of immediate knowledge only by way of introspection or self-awareness;[24] and since this observing activity is itself a phenomenon of consciousness, the total state of consciousness, as object of psychological knowledge, is not just awareness, but rather self-awareness, awareness of awareness. The state remains unitary. If, however, we have recourse to logical abstraction, then the second awareness, the "phenomenon [of consciousness] known as fact," may be distinguished as 'content' from the activity of observation, the "knowing of the phenomenon [of consciousness] as object."[25] In this way the observer is brought within the conscious field, and degrees of consciousness are so far justified.

Still we are not at an end. The 'content' thus abstractly marked off from the activity of consciousness remains consciousness, and must therefore in its turn admit of the same distinction of activity and passivity. Sensations, ideas, feelings, conations may be regarded, passively, as 'content' of consciousness.[26] Their description and explanation make up half of the detailed psychological story. In the other half, consciousness (conscious content in the broader sense) is regarded actively: as intellection (or awareness of likeness and difference), as reactive feeling (mental tension, conviction), as attention.[27] Ladd is emphatic that "the task of a scientific psychology is as truly the description and explanation of the phenomena of consciousness, considered as forms of active functioning (of consciousness 'function-wise'), as it is the description and explanation of the particular qualities and quantities of the phenomena regarded as passive states (of consciousness 'content-wise')."[28] And he expressly applies this dictum to the "single state of consciousness, so far as we can catch it and separate it from the stream of conscious life," *i. e.,* to what he has called the phenomenon known as fact, no less than to the "stream of conscious life in which every such state occurs," *i. e.,* to the total consciousness which includes the knowing along with the known.[29]

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[23] *Ibid.,* 1f., 4, 7, 9, 32, etc.

[24] *Ibid.,* 9, 15, 523, etc. Introspection is also called self-consciousness and reflective consciousness.

[25] *Ibid.,* 32f.; cf. 37, 49, 289ff. Ladd varies in his use of the terms 'active' and 'passive:' see 46, 83, 96, 214, etc.

[26] *Ibid.,* 309. There seems to be inconsistency as regards feeling: cf. 19f, 163, 523.

[27] *Ibid.,* 288ff.; 308f.; 213, 289.

[28] *Philos. of Mind,* 86.

It appears, then, that Ladd operates with two distinct notions of 'consciousness:' the notion of consciousness as the sum-total of conscious states which make up the experience of an 'I,' and the notion of consciousness as the observing activity of this 'I' itself. If the states alone are conscious, there should be no more or less of consciousness: but Ladd, assimilating the conscious character of the states, their essential nature as awarenesses, to the conscious character of the observing self, is able to speak of degrees of consciousness at large. The complete datum of psychology, awareness of awareness, he regards as intrinsically unitary, but as separable by logical abstraction into awareness knowing and awareness known, activity or function and content of consciousness. The awareness known, the content-awareness, he then divides again, by the same abstraction and in the same terms, into activity or function and content. These divisions are confessedly artificial: there is no real line of division within the psychical fact: and we must suppose—since there seems to be no logical reason why they should not be repeated *ad infinitum*[30]—that they are carried only so far as is necessary for systematic exposition. Ladd himself justifies them, partly on the ground of convenience, but also, in the general portion of his treatise, because they indicate that psychology, the science of the phenomena of consciousness as such, is far more than description and explanation of merely passive 'content.'[31]

§ 3. In seeking to appraise Ladd's definition of the psychical, as subject-matter for a science, we shall confine ourselves to a few broad lines of criticism. And we begin by considering (1) the place or position to which the definition assigns psychology within the group of the acknowledged sciences.

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[29] *Psychol.,* 290f.

[30] Brentano (*PES,* i., 1894, 167) avoids the infinite regress by means of his *eigenthümliche Verwebung:* cf. my *Exper. Psychol. of the Thought-processes,* 1909, 47f. Ladd (*Psychol.,* 35) comments critically on Brentano's fourfold act, but does not appear to recognize the logical difficulty in which he is himself involved. I do not know that every logical regress is necessarily vicious. But in this case Ladd's system is not intelligible without the regress, while the nature of the regress itself is (as it seems to me) to be unintelligible.

[31] It follows, of course, that Ladd's 'physiological psychology' is, as psychology, only a portion of a science, and not an organized scientific whole: see Ladd and Woodworth, *Physiol. Psychol.* 381, 430, 542, 597, 625, 656, 664f.

Observation, the immediate awareness of fact, seems in Ladd's view to be identical over the whole range of science. He draws no distinction of kind between inspection and introspection.[32] When, however, we turn to the relation between this direct method of acquaintance and the object upon which it is directed, we find that psychology is "peculiar, and indeed unique." For observation, being itself a fact of consciousness, merges into, or fuses with, the facts of consciousness observed; "it is separable, neither in reality nor in time, from the phenomenon observed as fact."[33] Our own discussion of psychological method must be postponed. Here we note only that, as regards the relation of its primary method to its subject-matter, Ladd marks off psychology, as unique, from all the other sciences.

The subject-matter of psychology, in what (for want of a better phrase) we may call its logical constitution, is also, for Ladd, unique. The state of consciousness, it will be remembered, is always, at one and the same time, fact of knowledge, fact of feeling and fact of conation. "This unity in variety, which belongs to all states of consciousness as such, is of unique character—and this, whether we lay emphasis on the unity that comprises the variety, or upon the variety comprised in the unity."[34] This uniqueness of constitution, exemplified by the single state of consciousness, is attributed by Ladd to the entire course of the mental life.[35]

Not only in logical constitution, however, but also in its self-determination, is the subject-matter of psychology unique. Descriptive and explanatory psychology leads us "to recognise a unique and self-active being" as, within limits, "interiorly determining, in a quite inexplicable way, its own course."[36] The presence among its data of this incalculable surd—whatever the limits of its operation may be—again separates psychology from the other sciences.

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[32] *Psychol.*, esp. 17f. Even the fact that psychical phenomena are "alterable—swiftly and largely—by the very act of attention which makes them objects of knowledge" seems to be paralleled on the side of inspection: see 18 (§ 3), 305f., 318f., 367f., etc.

[33] *Ibid.*, 32f., 319, 530, etc. Cf. *Philos. of Mind*, 160: "The knowing subject and the object are . . . woven into a vital oneness of being."

[34] *Psychol.*, 36, 172f.

[35] There is, namely, a "principle of continuity which gives its unique character to what we can observe of mental development. In all forms of organic physical evolution . . . the factors and stages of the evolution have some existence and value considered *in themselves*, as it were. But the case of mental development is not so. . . . Each factor, faculty, and stage exists for consciousness as in and of its own continuously flowing life-movement;" *ibid.*, 659f. The unity and variety of states of consciousness "are illustrations of this very principle of continuity as lying at the base of mental development:" 661.

Here, then, are three points at which scientific psychology is wholly and necessarily out of touch with what should be its fellow-sciences. It is clear that the word 'science,' if it is to be retained at all, must be given an extraordinarily elastic meaning. In fact, the position which Ladd assigns to scientific psychology is a position, not within, but without the circle of the acknowledged sciences.[37]

(2) Ladd, however, has his own definition of science. "There is science," he says, "wherever there are ascertainable facts that may be described and explained in their relation to one another and to other classes of facts."[38] We must therefore consider, secondly, the nature of the 'facts of consciousness' that appear in his pages as the data of a scientific psychology.

Throughout his psychological writings Ladd is insistent that the phenomena of consciousness be regarded both content-wise, as facts of passive existence, and function-wise, as facts of activity.[39] The task that he sets himself is, accordingly, twofold: he must describe and explain both the content and the function of every typical psychosis; and, indeed, he must describe and explain the particular content as adequate vehicle of a particular function, and the particular function as correlated activity of a particular content. This of itself would be a sufficiently difficult programme; but Ladd is hampered in his undertaking by his double use (to which we have already referred) of the term 'consciousness,' which means both 'my' awareness and also the awareness intrinsic to a psychosis as such. Consider, for example, that "convenient abstraction," the sensation. Every sensation is at once active and passive, a "psychical activity" and a passively received "impression." As active, it should be actively aware, actively cognisant of the "quality belonging to the object of sense." In Ladd's account, however, it becomes, even while regarded as intrinsically active, an "item of information" to 'me;' 'my' sensations "become objectified, as my feelings and thoughts cannot, in the form of qualities of perceived *things*." The function which should be the sensation's own is thus transferred, from the sensation, to the 'me' to whom all sensations belong.[40]

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[36] *Ibid.*, 638, 662. We might append, as a fourth point, the fact that psychology sustains a "quite unique" relation to philosophy: *ibid.*, 638; *Philos. of Mind*, 71.

[37] Ladd can be sufficiently severe upon these 'acknowledged' sciences: *e.g.*, *Philos. of Mind*, 6ff. The point here, however, is that he distinguishes psychology by its three (or four) unique characters from the other sciences, not as seen by themselves, but as he sees them.

[38] *Psychol.*, 658. The 'affirm' of the following sentence should apparently be read 'deny.'

[39] Ladd observes that "a psychosis without content is equivalent to no psychosis at all; there are no phenomena of consciousness in general" (*Philos. of Mind*, 85; cf. *Philos. of Knowledge*, 200, and *Psychol.*, 30f.). Yet he frequently speaks, in the *Psychology*, as if consciousness were, after all, something 'apart from' and superadded to the 'actual psychic facts;' we read of 'conscious psychic activity' (214), 'conscious intellection' and 'conscious mentality' (296), 'the conscious mind' (305), 'conscious mental life' (321, 469), conscious feeling (583, 587), 'conscious ideation' (600), 'conscious conation' (623), 'conscious acts of will' (657), and 'conscious attention' (666f.). Similar phrases occur in Ladd and Woodworth, *Physiol. Psychol.*, 380, 463, 642, 671, 676, 679. These slips give further evidence of the instable nature of Ladd's concept of consciousness.

In this case, then, there is loss to be noted; in the case of primary intellection, on the other hand, we have a superfluity. Intellection, as actively discriminating consciousness, is "within," is "an integral part of," every state of consciousness, so that a concrete psychosis is by its very nature self-discriminating and self-discriminated. Yet if I make a state of consciousness the object of my regard, discriminating consciousness is found to "accompany" the now passive fact. It is perhaps intelligible that 'my' discriminating consciousness should be needed to discern the discriminating activity which is native to the psychosis (though it must be remembered that, as such, these two activities are identical); but it is surely not clear how this attendant discrimination of mine helps toward the discernment of the already self-discriminated 'content.' Two discriminating activities, of precisely the same kind, are here set to work in circumstances where it seems that one would be sufficient.[41]

In still other cases, the duplication of consciousness leads to sheer logical confusion. Suppose, for instance, that I, by way of primary attention, make a state of consciousness the passive object of my consideration: primary attention is then the degree of psychical energy "expended upon" the different aspects or moments of the state. Yet every mental state, as active, has its own degree of this same psychical energy, upon which attention, in its present turn, is constantly dependent. Ladd tries to save his logic by the remark that these two statements "only serve to approach the same truth from different sides." But the truth, as he leaves it, seems rather to be a matter of alternatives.[42]

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[40] *Psychol.*, 93ff.

[41] *Ibid.*, 33f., 288.

Difficulties of this sort recur again and again to baffle the student of Ladd's psychology. The 'facts of consciousness' with which the system deals are both ambiguous and instable. They are active and they are passive, they are forthputtings of a mind and data of a science, they are conscious and I am conscious of them. Moreover, they play their systematic parts with so little regard to omission and repetition and contradiction that the plot of the play tends to be lost.

(3) This plot itself, we must add,—the systematic working up of the psychological materials,—shows a like ambiguity. Ladd declares, for example, that the mental life, from its very beginnings, carries the plain promise of a plan, so that "no *science* of the life of mind is possible without recognising the presence of final purpose;" yet he affirms also that "in attempting a scientific account of the mental life psychology is justified in laying emphasis, at first, upon the passive and, as it were, externally determined side of the total development," as if no plan were visible or operative.[43] Here is no distinction of function and content within consciousness, but the recognition of two radically different psychological attitudes toward mind in general. The same attempt to have things both ways is seen in the treatment of the faculties: Ladd, in all literalness, both accepts and rejects the doctrine of faculties. He speaks, in formal reference, of the 'so-called' faculties; he explains that the term 'faculty' is both futile and dangerous; and still he employs the concept, repeatedly and constructively, in his psychological exposition.[44] And, as a final instance, the same criticism holds of Ladd's treatment of mental composition. He makes free use of the terms 'mix-

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[42] *Ibid.*, 74f., 78, 83, 621. On 'psychic energy' (of which Ladd gives no definition) see 39, 41, 44, 64f., 78, 83, 132, 261, 386, etc.

[43] *Ibid.*, 266f., 286f., 414, 664f., 668; *Philos. of Mind*, 203; *Philos. of Knowledge*, 473, etc.

[44] *Psychol.*, 33, 45f., 49ff., 60, 288, 317f. 380, 409f., 455, 490, 612, 659, 664. In particular: (1) Ch. iv. is headed "The So-called 'Mental Faculties.'" Ladd's use of 'so-called' is a mannerism, which began perhaps as a defensive reaction against the possible charge of looseness of language. Since it has come with him to mean anything from 'what is ordinarily called' to 'what is falsely called,' it does him little service. (2) The adjectives 'futile' and 'dangerous' are not too strong. For we are told (51) that words like 'faculty' do not explain; that they help but little in classification; and that their use, however guarded, is "likely to occasion. . . . a generally inadequate and misleading account of the development of mental life." (3) For the seriousness with which the concept is taken, see such passages as 380, 490.

ture' and 'blending' and 'fusion' and 'association,' while yet he assures us that this language is figurative, adopted only for the avoidance of "almost unending periphrases." But is taste, or is it not, a complex of "gustatory, olfactory and tactual elements?" And is there, or is there not, in stereoscopic vision, a 'combination' of visual with tactual and motor sense-complexes? The reader looks in vain for a single periphrasis to clear his mind, and is forced to the conclusion that Ladd's logic is trying to say both Yea and Nay of the same subject-matter.[45]

(4) Where both data and applied logic are thus ambiguous, it is perhaps unnecessary to urge that the resultant is not an organised system. Since, however, we are presently to discuss the working concepts of 'function' and 'content' in their general applicability to the subject-matter of psychology, we cannot afford to neglect this formal point of criticism. We note briefly that Ladd's system breaks bounds on every side.

Ladd starts out with the threefold unity of intellection, feeling and conation, all of which 'moments' or 'aspects' of consciousness are to be taken both as content and as function. These are the material postulates of his psychological system. But they prove to be inadequate. The study of intellection, for instance, brings us in time to the problem of recognitive memory; and here our scientific advance is arrested; recognition is "a form of mental reaction *sui generis,* which, while depending upon conditions . . . , has still a unique character that transcends the conditions on which it reposes."[46] In like manner the study of feeling brings us to the feeling of obligation and the sentiment of moral approbation or disapprobation. "These two forms of moral feeling are unique. Why they arise in the individual, and why they have that nature and connection with each other, and with the development of intellect, which they actually have—these are questions which psychology [even as explanatory] cannot answer."[47] Lastly, the study of conation brings us to volition, where "psychologically considered, it is no less true that I will the influential ideas, feelings and desires, than that the ideas, feelings and desires influence the final 'I will.'"[48] So that there is no single aspect of mind, no single strand of mental development, for which the complication of function and the compounding of content are sufficient. The system is disrupted on the side of intellect, on the side of feeling, and on the side of will.—

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[45] The following sentence (*ibid.,* 235) is characteristic: "The introspective and experimental analysis of modern psychology cannot be abandoned, because, in spite of repeated explanations, some readers will persist in misunderstanding our necessarily figurative terms." Cf. 18f., 23, 37, 38f., 89ff., 94, 102f., 106, 115, 118, 132, 141ff., 146, 160, 180, 186, 209, 253ff., 318f., 323, 349, etc.

[46] *Ibid.,* 382, 397, 399, 401.

[47] *Ibid.,* 581ff.

[48] *Ibid.,* 618f., 625f., 635, 638.

There can be little question, then, that Ladd has failed to erect a science of psychology on the basis of a special class or department of "phenomena of consciousness." We looked at his 'so-called' science in its general status and relations, and we found it to stand apart from all acknowledged sciences. We glanced over the contents of his exposition, and we found that his programme has not been carried out. We considered the logic of his system, and we found it no less ambiguous than the materials to which it is applied. We have now examined the result to which his systematic labours have attained, and we find him hopeless of a system. It remains to ask whether his failure is the failure of a particular author, or whether it is the failure of his psychological position 'as such.'[49]

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[49] The systems of Angell and Judd can be less certainly appraised, partly because they have so far been carried out only at the text-book level, and partly because their writers are more interested in function and genesis than in content. We may, however, note the following points.

For Angell, psychology is the science of consciousness, and consciousness is awareness (*Psychology,* 1908, 1, 222, 366, 442; cf. 5, 150, 185, 199, 228, 246ff., 337, 364f., 370, 373, 383ff., 399, 426ff., 430, 441ff., 444, 446). On its subject-side, consciousness is the observing activity of a self; 'we' are immediately aware of thoughts and feelings, of perceptions, images and emotions (2, 442f.; cf. 84, 302, 401, 408, 431, etc.). In its object-half, or on its content-side (content being taken in the broader sense), consciousness shows the two aspects of structure or content (in the narrower sense) and function (*e.g.,* 201). States of consciousness are functionally unitary, but by logical abstraction may be divided into cognitive and affective (302, 436f.). [The position of attention is ambiguous. Attention appears to be a function of subject-consciousness, with a structure on the side of object-consciousness; there is apparently no discriminable attentive function within object consciousness: 80ff.] Both the cognitive and the affective functions are, in their own right, modes of awareness: the cognitive are informative (109, 170, 198, 201, 222, 248), the affective are evaluative awarenesses (302, 320, 322, 327, 378, 382). In general outline, therefore, Angell's system is very like Ladd's.

For Judd, likewise, psychology is the science of consciousness (*Psychology,* 1917, 1, 5, 10, 12, 38, 145, 309; behaviour is to be studied for the understanding and explanation of consciousness), and this consciousness is awareness (2ff., 6, 12f., 142, 238, 329; cf. 27, 65, 73, 160f., 166, 169, 183, 190, 212, 233, 246, 270, 276, 291, 345). Awareness, however, is always the awareness of 'someone;' it is 'one' or 'the observer' or 'the individual' or 'the child' that is conscious (1, 141f.,

The Functional Systems

§ 4. We pass, accordingly, to a consideration of the features common to functional systems in general.[50] There seem to be four main tenets or tendencies which we may regard as characteristic of the school.

155, 169, 189, 241, 272, 301, etc.). 'Conscious processes,' which are classified according to nervous processes (64ff.), show the familiar duality. Over against sensations (73, 188), which are discussed content-wise, stand the attitudes of feeling and attention (66f., 146ff.), which are discussed functionwise. Perception includes, on the side of function, the activities of fusing, locating, distinguishing, recognizing, in a word of relating sensations (163, 166, 169, 186, 189, 191), and on the other side the product or resultant of these activities, the percept or relational fact, a content of a higher order (169, 175, 186f., 189, 191f.). The content of memory consists in images, which are primarily substitutes for sensations and percepts (241ff.), and its activity consists in recall according to the laws of association (244ff.). Ideation furnishes us with contents of a still higher order, difficult of precise description (246), and with such activities as conception, abstraction, generalization, judgment and reasoning (263ff.). Lastly, in voluntary choice we have a personality, an organized whole of mental activities (308), directed upon 'ideas' which are themselves organized "composites of experience" (306, 309).

There remains the question of the 'someone' who is thus in manifold ways and degrees aware. Judd's teaching is that we must conceive the knowing self, to which the study of conscious processes leads us, as an unitary being,—a 'conscious being' which, like the 'living being,' is an organized unity (274ff.; cf. 263). The self possesses and, by unifying, modifies conscious states (274). It is, however, difficult—since the terms 'self' and 'personality' are used interchangeably—to reconcile the statement that "the self is a being which perceives and forms concepts" (274) with the statement that "personality is the name of that individual nature which has been developed out of the play and interplay of impressions and instincts and conscious comparisons and imaginations" (308). See p. 521 above.

[50] This is not the place for a bibliography of functional psychology. I give a few early references, and name two papers of objective import.—W. James, "On Some Omissions of Introspective Psychology," *Mind,* ix., 1884, 18f.; *The Principles of Psychology,* i., 1890, 478; H. Ebbinghaus, *Grungzüge der Psychol.,* i., 1897, 161ff. (i., 1905, 176ff.; changed by Dürr, i., 1911, 175ff.); E. B. Titchener, "The Postulates of a Structural Psychology," *Philos. Rev.,* vii. 1898, 449ff.; "Structural and Functional Psychology," *ibid.,* viii., 1899, 290ff.; C. A. Ruckmich, "The Use of the Term '*Function*' in English Text-books of Psychology," *Amer. Journ. Psych.,* xxiv., 1913, 99ff.; K. M. Dallenbach, "The History and Derivation of the Word 'Function' as a Systematic Term in Psychology," *ibid.,* xxvi., 1915, 437ff.

Angell remarks (*Psychol. Rev.,* xiv., 1907, 63) that 'structural psychology was the first to isolate itself.' In a broad sense, that statement is true; functional psychology claimed the whole field. In historical detail, however, things are less simple. There had already appeared in America the two overtly functional systems of J. M. Baldwin (*Handbook of Psychology,* 1889-91) and Ladd (1894). Moreover, it was as early as 1887 that Ladd, in his *Physiological Psychology,*

(1) The distinction between the 'activity' or 'function' and the 'content' or 'structure' of consciousness is recognised, explicitly or implicitly, by all the psychologists of function. It reflects, of course, the biological distinction of organic function and organic structure, of physiology and morphology. We say 'biological,' in deference to current usage: in point of fact, the distinction is rather technological than scientific. It belongs of right to the great technology of medicine; it was accepted, and found useful, by human physiology; it remained serviceable in the beginnings of comparative physiology. On the strict ground of science, it is no longer adequate to our knowledge;[51] and if it still persists in biological textbooks, the reason is again largely technological: the distinction recommends itself for pedagogical purposes. Even in medicine, its encouragement of a rigid specialisation lays it open to criticism.

There is, nevertheless, in the 'biological' context, a certain appeal to common sense, a certain plausibility, about this correlation; heart and lungs and hand and brain, regarded as machines, are obviously 'adapted' to the 'ends' which they subserve. We search in vain for any such plausible coördina-

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sought deliberately to subsume the whole body of experimentally observed facts to a functional view of 'the nature of mind.' So that functional psychology had been a little uneasy, a little self-conscious, for a whole decade before the catch-word 'structural' came on the controversial scene. And the first note of revolt (E. W. Scripture's *New Psychology*, 1897) was struck in the physical, not in the biological key. Scripture's attempt proved abortive, partly because the physical concepts of time, space and energy were inadequate to the psychological data, and partly because he represented his new psychology as a straight development from the old: "there is no difference in its material, no change in its point of view" (453). 'Structural' psychology, as its name implies, recognized the existence of a correlative psychology of function; it isolated itself only to the degree that it demanded equal rights for content and activity, and by so doing protested against the autocracy of function. In the 'biological' atmosphere of its time the phrase did good controversial service. If the view of my book is accepted, both 'functional' and 'structural,' as qualifications of 'psychology,' are now obsolete terms.

[51] As appears, *e.g.*, from such works as H. S. Jennings' *Behaviour of the Lower Organisms* (1906), and J. S. Haldane's *Organism and Environment as Illustrated by the Physiology of Breathing* (1917); cf. also B. F. Kingsbury, "The Fitness of Organisms from an Embryologist's Viewpoint," *Science*, N. S. xxxviii., 1913, 174ff.; "The Interstitial Cells of the Mammalian Ovary; Felis domestica," *Amer. Journ. Anat.*, xvi., 1914, 79; "The Development of the Human Pharynx," *ibid.*, xviii., 1915, 374ff. It is obvious that those who identify biological phenomena with physicochemical processes have no need of the concept of function (see *e.g.*, J. Loeb, *Forced Movements, Tropisms and Animal Conduct*, 1918); but this view is not here under consideration.

tion of conscious content and conscious function. The "forms of receptivity," according to Ladd, stand in "bewildering and unclassifiable variety" over against the "relatively few forms of organising activity displayed in all mental states."[52] If content is to be made adequate to function, then (as in the instance of the 'representative image') function must be read into content, so that the content becomes an hypostatised function.[53] Nowhere is the attempt made to show that the strictly observable 'contents' are the suitable vehicles or instruments or substrates of the various modes of psychical activity.

Why, then,—it is natural to ask,—why, and with what hopes, did psychology submit itself at all to 'biological' guidance, and seek within its subject-matter for the analogues of organic structure and organic function? The answer refers us to historical conditions. Historically viewed, the functional psychology of which we have taken Ladd to be representative is a plea in avoidance offered before the court of science on behalf of empiricism. The continually growing body of experimental facts, menacing even while unorganised, had to be reckoned with; and the distinction of function and content afforded a means whereby it might be duly subordinated to the empirical system. If the worst should come, and experimental psychology should be able presently to organise itself as an independent science, still the empiricist had gained time, a breathing-space for adjustment, and had gained also a set of working concepts by whose aid the break might, so to say, be eased and graded. What he expected, however, as is clear from all of Ladd's work, was a new lease of life for the traditional empiricism. The particular line which his reconstruction followed was settled for him by the status of science at the time. Not mathematics, not physics, was the characteristic modern 'science,' but biology; and if biology thought and spoke in terms of structure and function, psychology need not scruple to think like thoughts and to use like language. So there arose a new empirical psychology, in all essentials at one with the old, but more or less effectively disguised under the cloak of contemporary science. After the event, we can see that psychology borrowed in haste, without assurance that the loan could be turned to properly psychological account. We can see, further, that the biology to which psychology appealed was not in truth a scientific biology, but a biology conventionalised and popularised. At the time, nevertheless, it was only natural that the contents and functions of this new empirical psychology should seem to bear the authentic stamp of science.

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[52] *Psychology,* 53ff., esp. 57.

[53] *Ibid.,* 234ff., 244ff., 376. D. S. Miller ("The Confusion of Function and Content in Mental Analysis," *Psych. Rev.,* ii., 1895, 536) meets the difficulty by a reference to the unlikeness of physical cause and physical effect. But content does not stand to function as cause to effect, and physical causes and effects are alike energetic.

(2) In the second place, functional systems are disposed to correlate the phenomenon of consciousness with unreadiness or inadequacy of the nervous system; "consciousness is only intense," remarks James, "when nerve-processes are hesitant."[54] As soon as the organism is confronted by a problem which its existing neural organisation is unable to meet, consciousness steps in to dispel the perplexity; "straightway appears consciousness, with its accompanying cortical activities, taking note of the nature of the stimulus and of the various kinds of muscular response which it called forth."[55] The point must be taken, it would seem, with a certain reservation: for it is not unreadiness or inadequacy in general that is responsible for the appearance of consciousness, but rather such unreadiness and such inadequacy as require, within the biological limitations of the particular organism, only a certain additional prompting or urging to change into readiness and adequacy. With this reservation more or less expressly made, some authors, like Angell, declare in round terms that "conscious activities emerge at the point where reflex acts are found inadequate to meet the needs of particular situations;" "if the reflexes and the automatic acts were wholly competent to steer the organism throughout its course, there is no reason to suppose that consciousness would ever put in an appearance."[56] Others, like Dewey, argue that difficulties in

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[54] *Principles*, i., 1890, 142. James, although he favoured a functional as opposed to a structural psychology ("The Energies of Men," *Philos. Rev.*, xvi., 1907, 2), does not give us in the *Principles* a system of functional psychology. He gives us a work on the principles of knowledge, written from a psychologistic standpoint. If his volumes are read with this interpretation in mind, the critics' charge of lack of plan (which James repelled: *Text-book*, 1892, iii. f.) will be found groundless.

[55] Angell, *Psychology*, 1918, 64.

[56] *Loc. cit.* Angell confines his discussion, in these passages, to the human consciousness. The question 'why' the human infant should be limited to a certain group of inherited coordinations he hands over to 'the biologist' for answer. So one might hand over many other questions: 'Why,' since in the last resort the environment is for all organisms the same, should any infant embark on a career for which his inherited coordinations are insufficient? and 'why,' having thus embarked, should he again meet with limitations to his conscious powers? and so on. The biologist, in so far as he is man of science, will maintain a discreet silence.

the way of nervous conduction heighten the clearness of consciousness, but do not commit themselves outright to a theory of its first appearance.[57] There are thus differences of detail.[58] It would, however, be widely agreed that, at any rate in the case of man, the 'condition' of consciousness is a temporary and corrigible imperfection of nervous organisation.[59]

A view of this sort seems, indeed, to be logically bound up with the view that consciousness is primarily and actively a matter of function, and only secondarily and passively a matter of content. For if consciousness is to do us some organic service, it will find its natural opportunity in some defect of our given (non-conscious or extra-conscious) organic equipment; and this, in the categories of our current thinking, means some defect in the functional capacity of the brain. Positive evidence, however, is found in the experience of every adult. We have learned to write, to use a typewriter, to ride a bicycle, to play a piano, to drive a motor car; and we know that, as the original problems cease to baffle us, and the skilled movements become habitual, the need of conscious 'control' grows less and less; we find that consciousness 'intervenes' less frequently and less imperatively, until it may finally disappear. Could there be more convincing proof of the instrumental function of consciousness? and is not the formation of a habit typical of the general course of the mental life?[60]

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[57] So at least I understand these discussions. See J. Dewey, "The Theory of Emotion," *Psychol. Rev.*, i., 1894, 553ff.; ii., 1895, 13ff.; "The Reflex Arc Concept in Psychology," *ibid.*, iii., 1896, 357ff.; "The Psychology of Effort," *Philos. Rev.*, vi., 1897, 43ff.

[58] The same general idea has been expressed in various places by W. McDougall. See, *e.g.*, "A Contribution towards an Improvement in Psychological Method," *Mind*, N. S. vii., 1898, 159ff.; "On the Seat of the Psycho-physical Processes," *Brain*, xxiv., 1901, 607f.; "The Physiological Factors of the Attention-Process," *Mind*, N. S. xi., 1902, 341f.; *Physiological Psychology*, 1905, 59f.

It is possible that Ladd is adopting a like view when he makes conation or conscious striving "the most fundamental of all psychic phenomena" (*Psychol.*, 219). Ladd and Woodworth, however, combat the theories of Dewey and McDougall (*Physiol. Psychol.*, 1911, 610ff.).

[59] Judd ("Evolution and Consciousness," *Psychol. Rev.*, xvii., 1910, 77ff.) forms a marked exception to the rule. Consciousness appears only when the organism has attained a certain stage of complexity of inner organization (94). The question of first causes—where this consciousness comes from—is not raised (92). Having appeared, however, consciousness functions as a centre of reorganization (93f.) The more highly organized, *i.e.*, the more self-sufficing the organism, the greater is the rôle and the higher the development of consciousness (80f., 88). In a word, consciousness is at once the product, the index, and the cause of organization.

We must here meet the psychologist of function on his own ground; we must, that is to say, grant him his assumption that consciousness, the subject-matter of psychology, is awareness. The question then arises whether our conscious life is, as a matter of fact, nothing more than a recurrent grappling with problems. Are we aware only when and in so far as we are searching, reconciling, reducing to familiar terms, trying to understand,—or whatever other words there may be that imply the setting of a problem and the urgency of its solution? Surely we are not; surely there is a contemplative as well as an acquisitive awareness. We are not always learning something new, or practising this new until it becomes old. There are times when we sun ourselves, as it were, in the full light of consciousness,—times when, without any "felt necessity for further nicety of adaptation to surroundings,"[61] we are none the less keenly and competently aware. There is a consciousness militant, but there is also a consciousness triumphant: more than that, there is a consciousness that, in the midcourse of its campaigning, rests quietly upon its arms, and surveys the terrain it has occupied. A narrowly instrumental theory of consciousness comes very near to a *reductio ad absurdum* of the biological 'struggle for existence.'

(3) The two characters that we have so far discussed—the distinction within consciousness of content and activity and the assignment to consciousness, especially in its active phase, of a value for organic survival—lie on the surface of the functional systems. Underneath, always and everywhere, runs the steady current of teleology. It is one of the ironies of the history of science that the great biological generalisation which was to free us of teleology in our study of the phenomena of life should give rise, in post-Darwinian days, to an unbridled license of teleological 'interpretation.'[62] Biology

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[60] Judd is here consistent. "Too often the psychology of habit has been guilty of the statement that habituation leads to unconsciousness. This is not the case" (*Psychol.*, 1917, 207).

[61] Angell, *Psychol.*, 1908, 74. Note the implication of the phrase! Consciousness, which might on Angell's terms be described, roughly, as the "felt process of adaptation," here anticipates its own function.

[62] "If we apprehend the spirit of the 'Origin of Species' rightly, then, nothing can be more entirely and absolutely opposed to Teleology, as it is commonly understood, than the Darwinian Theory:" T. H. Huxley, "Criticisms on 'The Origin of Species'" (1864),*Lay Sermons, Addresses and Reviews*, 1887, 264. It is a mind "stored with the choicest materials of the teleologist that rejects teleology,"—so J. Tyndall speaks of Darwin (*Report B. A. A. S., 1874*, 1875, lxxxvi.). Cf. E. du Bois-Reymond, *La Mettrie*, 1875, 23.

has suffered, and is still suffering, from that license. Biology, nevertheless, again furnished psychology with the obvious scientific parallel. Biologists spoke in terms of final cause, the whole system of biology was pervaded by teleology, and psychology again had no call to be more scientific than science. We have seen the outcome in Ladd's thinking: a teleological activity of organisation plays upon a causally (or quasi-causally) determined content, to the inevitable confusion of the concept of consciousness which is fundamental to his psychology at large. We find a like ambiguity in Angell's book. Mind, as we have said, there " seems to involve the master devices " through which the " adaptive operations of organic life may be made most perfect," and " the real business of consciousness is to be sought amid the adaptive responses of the organism to its life conditions."[63] But when we go in search of master-devices and real business we are told that the basal distinction is that " between certain kinds of nervous activity overtly involving consciousness . . . and certain other kinds not overtly involving it;" consciousness is only the " index " of " problem-solving adaptive acts." Nay more: if we speak "as though mind might in a wholly unique manner step in and bring about changes in the activity of the nervous system," we are employing " convenient metaphors," " a convenient abbreviation of expression," which must not blind us to " the fundamental facts which lie behind."[64] Such are the logical sacrifices that teleology demands of her children!

May it not be, however, that the fault lies with the children? May it not be that the psychologist of function takes his teleology a little carelessly? It has come down to him from the older empiricism; it is guaranteed by philosophy and technology; it is justified by biological example. Small wonder, then, that he should slip easily, even heedlessly, into the teleological attitude! But are we on that account ruthlessly to banish teleology from a scientific psychology?

One could wish, certainly, that the functional systems were somewhat less confident of their position. For it is one thing to affirm broadly that " the stream of consciousness appears . . . as a current designed from the beginning . . . to the fit performance of a certain work,"[65] and quite another thing to build psychological facts and laws, the details of psychology, into a coherent system of means and ends. The teleology which is used in psychology as a tool of scientific construction is, as a matter of fact, both clumsy and double-edged. The psychologist may insist on final causes as he will, but he never makes them adequate to the refinement of observation;[66] and he may answer any number of Whys, but he is still faced by unanswerable Why-nots that throw doubt upon his positive explanations.[67] Yet we must insist that final causes, if they are at all recognised in psychology, be recognised primarily for psychology's sake. It is not enough to infuse just so much of teleology into the psychological system as shall orientate us toward ethics or history, or place us within a generally biological context, or blur the difference between 'pure' and 'applied' science. Psychology has its own claims in behalf of every item of its subject-matter.[68]

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[63] *Op. cit.*, 8, 95.
[64] *Ibid.*, 59f., 300.
[65] Ladd, *Psychol.*, 668f.

There is, however, no reason to suppose that these claims could be met by any teleology, even the most self-conscious and the most persistent. The whole history of science argues to the contrary. We may freely grant that teleological ideas have the occasional heuristic value which certain investigators have claimed for them.[69] It remains true, notwithstanding, that these same investigators have only by exception had recourse to teleology, and have not been encouraged to adopt it as a guiding principle of research. It remains true that final causes have long been banished from the domain of the older sciences, and that they flourish only where (and in proportion as) exact knowledge is wanting. It remains true that interpretation by means and end tends to close enquiry and thus to bar the progress of scientific knowledge. In particular, it is highly significant that biology, after full trial of teleological principles, is in these latter days resolutely turning away from final causes to the laborious planning of experiments and the patient accumulation of observed facts, —so that 'evolution' and 'heredity' and 'adaptation,' once the means whereby we conjured ends, are now coming to be mere descriptive labels for laboratory note-books. The whole history of science thus goes to show that teleology is essentially non-scientific. And, if that is the case, there can be no room for "teleological import" within a "*science* of the life of the mind."[70]

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[66] "Fortunately for science," writes Judd (*Psychol.*, 90f.), "there have been a few cases in which the same person has been able to observe directly both the normal color sensations and the partially color-blind series." Fortunately for descriptive science, truly: but how fortunate for psychology in Judd's sense? How has the development of red-green vision aided man in the struggle for existence(4)? or what has man gained by the "unique compromise process"(92) which gives rise to the purple sensation? These and like questions are not touched.

[67] "It is because we have . . . no special organ affected by weak currents of electricity that men overlooked for so long a period both the prevalence of forms of electrical energy and the close relation between light and electricity" (Judd, *op. cit.*, 72). But, granted that the facts are as stated and granted that this furtherance of knowledge is useful, why have we not the special organ?—for it is surely evident that biological conditions, which have produced the 'electric fishes,' are also competent to produce an electrical sense-organ in man.

Again: "We do not have microscopic eyes like the fly. Nor . . . do we have distance vision like the eagle's. . . . The range of human vision has been determined by the range of possible human reactions" (132); cf. W. H. Hudson, *Idle Days in Patagonia,* 1893, 183 f. But has not man extended his reactions by microscope and telescope, and are not tools (249) sensory as well as motor? Cf. H. Spencer, *Principles of Psychol.*, i., 1881, § 164, 365; O. Wiener, *Die Erweiterung der Sinne,* 1900.

[68] How utterly this subject-matter may drop out of sight, under the prepossession of teleology, appears in the discussion by A. E. Taylor, *Elements of Metaphysics,* 1903, 306f.

(4) Finally, this teleological attitude threatens the stability of psychology as an independent branch of knowledge. The psychology of the functional systems appears as transitional, as a stage either upon the difficult ascent toward philosophy or upon the level road that leads to various application; always it appears as a half-way house on the journey to something else, and not as an abiding-place. The individual psychologist may look forward to the one goal or to the other, or may perhaps keep both in view; that is a matter of temperament and training. It is at any rate characteristic of the school that they are not content to rest in psychology. Their psychologising, with whatever pains and seriousness it is done, seeks to transcend itself, as if in the last resort it were done not for its own sake but for the attainment of some foreign end.

Ladd, for instance, bears witness on the side of philosophy. "The problems of philosophy," he tells us, "all emerge and force themselves upon the mind in the attempt thoroughly to comprehend and satisfactorily to solve the problems of a scientific psychology; and the attempts, along the different main lines of research in psychology, to deal scientifically with its problems all lead up to the place where this science hands these same problems over to philosophy."[71] Psychology is therefore of value as a propaedeutic to philosophy, in so far as philosophy is too difficult and too intricate to be approached directly and without an introductory easement; but the psychologist cannot, in scientific status, compare with the physicist or chemist or biologist. On the contrary! The longer he works in psychology, the more clearly does he confess his inability to tackle his problems at first hand; and the more able he is, the briefer will be his psychological apprenticeship to philosophy.

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[69] E. Mach, *Die Analyse der Empfindungen*, etc., 1900, 60f. The instance cited is not physical but biological. I do not find in the *Erkenntnis und Irrtum* (1906) any acknowledgment of the heuristic value of teleology in physics, though the book as a whole contains a good deal of teleological thinking.

[70] Ladd, *Psychol.*, 668.

Judd, on the other hand, lays stress on the importance of psychology for the understanding and consequent control of human life and human institutions. He devotes a chapter of his general *Psychology* to the maxims of 'mental hygiene,' by following which the individual may attain to the highest level of organisation, " when mental development becomes a matter of voluntary control." He devotes another chapter to the institutional applications of psychology, its usefulness in literature and the fine arts, in the social sciences and anthropology, and especially in education. He recognises, to be sure, that psychology's relation to philosophy is " closer than that of any of the special sciences," but the recognition is formal and carefully qualified. The main interest of the study of consciousness is that it gives the key to human nature, and thus enables us intelligently to guide the course of the individual and of society.[72]

There is no need of further examples, but there is great need that the reader clearly understand what these particular examples are meant to show. The point here at issue is not that the author of a scientific text-book should shut himself up within the four walls of his specialty, and look neither above nor about. Where we find such narrowness of vision, we are likely to find also confusion of thought. The point is rather that we note a marked difference of emphasis between text-books of functional psychology and text-books of other sciences. A text-book of physics will discuss various types of engines and machines, but it will discuss them as illustrative of physical laws. A text-book of physiology may discuss various types of pathological phenomena, but it will discuss them in connection with physiological methods and physiological principles. The text-books of functional psychology, on the other hand, tend—it is true, in varying degree—to make of psychology either an introduction to philosophy or an aid to individual and social welfare. In so far as these tendencies prevail: in so far as functional psychology, in its exposition of psychology proper, goes out to meet the problems of philosophy or of our customary human life: in so far there is real danger that the pains and seriousness which are the due of psychology as science are withdrawn from psychology and expended in those other fields.—

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[71] *Philos. of Mind*, 1895, 73; *Psychol.*, 12. Cf. J. R. Angell, "The Relations of Structural and Functional Psychology to Philosophy," in *The Decennial Publications of the University of Chicago*, iii., 1903, 55ff.; *Psychol.*, 1908, 9ff.

[72] *Psychol.*, 1917, 314ff., esp. 324, 344ff.; cf. 10, 208, 268, 299f., 309.

These, then, seem to be the four main characteristics of the functional systems. The subject-matter of psychology is duplicated, though function is preferred to content; consciousness is a solver of problems; the whole course of the mental life is regarded teleologically; and psychology is written as a preface to philosophy or to some practical discipline. We have already indicated that these characters are not logically coördinate. A logical arrangement might, however, have appeared to prejudge the case, whereas our topical presentation has required that every character be discussed on its individual merits. In any case it should now be plain that functional psychology has its roots in the Aristotelian empiricism, and that while it has taken color of modernity from the surrounding sciences it has not adopted the modern conception of science itself. For this reason Ladd's deliberate and sustained effort to maintain the continuity of psychology as science was foredoomed to failure. His loyalty to the past is incompatible with his open-mindedness toward the future. He recognises, frankly if a little anxiously, the gradual emergence of the scientific problem, yet he cannot bring himself to discard, even in what he defines as science, the constructions of prescientific thinking. He is thus betrayed into an illogicality which, in any other context, he would have been among the first to discern.

CHURCH HISTORY AND PSYCHOLOGY OF RELIGION

By PIERCE BUTLER, The Newberry Library, Chicago

The following paper is written by a student of church history, not by a psychologist. It attempts to emphasize, by citing certain specific examples, the importance and necessity of testing historical conclusions by the norm of psychological law. Furthermore, it ventures to criticise the present work of religious psychologists from the historian's point of view, and to suggest certain lines of investigation and verification in which he seems most to need professional assistance.

I

Formal instruction in religious psychology is given in most of our better theological seminaries; and more than this, it has become a commonplace in clerical circles that theologians ought to study psychology. There is thus an enormous amount of desultory reading of psychological treatises and text-books in ministerial studies. Unfortunately, however, a greater part of this study goes no further than a genetic investigation of the religious consciousness in a normal mind. It is Starbuck and his disciples whose writings are read rather than James, of whom indeed I believe it must be said that he writes altogether above the heads of many clerical readers. As a result, there is little reaction upon historical conceptions from the newer scientific knowledge of the human mind; only perhaps in his notions of the Crusades, or of the great religious revivals, is the student of ecclesiastical history directly influenced by his psychological readings. In these two matters his study of the mob-mind in such works as the *Social Psychology* of E. A. Ross inevitably becomes an element in the formation of his historical judgments.

But it is neither the religious experience of normal man nor great popular enthusiasms that most concern the student of ecclesiastical history; usually, the most difficult problem in his investigation of any period must be the understanding of the mind of a religious genius whose dominance over his fellows forms the figures which are printed upon the canvas recording the religious experience of the age. The reactions of normal men and women upon the teachings and examples of a leader constitute the warp and woof of the fabric, but this must usually remain unwritten and unwritable history; certainly it can never be reconstructed, be it never so tentatively, where one's understanding of the leader is distorted or untrue,—as it is quite certain to be when sympathetic insight into the mental processes of the religious genius is totally wanting.

Much time and labor are spent by modern students of the formative centuries of ecclesiastical history in tracing out, by philological evidence alone, influences and origins of constituent ideas and practices in various systems. Indeed, to some writers the ideal history of the course of religious thought seems to be a genealogical tree of theological "schools." Almost every German scholar who has attempted a *Dogmengeschichte* during the past seventy-five years is obsessed by this idea. Or perhaps a better illustration would be to select from the enormous literature upon Christian Gnosticism such work as is represented by the volumes of Bousset,[1] Anz[2] and Amélineau,[3] who demonstrate respectively that the system sprang from Zoroastrianism, Babylonian astral worship and Coptic magic, each arguing exclusively from the philological antecedent of certain specific terms used by Gnostic writers. All of these scholars know and mention explicitly the religious syncretism which marked the countries and age in which Gnosticism arose,—that strange coexistence and intermingling of all sorts of exotic cults which people frequented in seeming indifference to their inconsistencies and contradictions. Yet, in spite of the established fact of this religious climate, none of the three authors I cite has hesitated to ascribe definite lines of direct derivation where the evidence justifies no further conclusion than the contemporaneous existence of these other religions. With them it is not, as it should be, a commonplace in their historical canon that the religious genius habitually seizes out of the air, as it were, a name or phrase or idea that is current in his day, and weaves it into his own system without the slightest consideration of its origin and proper significance, and often indeed without any further acquaintance with the system to which it originally belonged.

Where the evidence is scanty enough, the fallacy of spinning an elaborate theory of genetic relationship out of philological coincidences is not ridiculously evident; but an attempt to apply exactly the same method to isolated identities, where the general evidence is more complete, will usually lead to all sorts of obvious inconsistencies. For example, in the *Book of Mormon* there are phrases which imply the technical terminology used in a contemporary controversy over baptismal remission which divided the denomination known as Disciples.[4] If there were extant only the *Book of Mormon* itself and a contemporary anti-Christian polemic writing which contained, as quotations, scattered fragments from sermons by Disciple ministers, a future Bousset or Anz, if he were true to his method, would have to describe Mormonism as a subcult or offshoot of the "Disciple School."

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[1] W. Bousset, *Hauptprobleme der Gnosis,* 1911.

[2] W. Anz, *Zur Frage nach dem Ursprung des Gnostizismus,* 1897.

[3] E. Amélineau, *Essai sur le gnosticisme égyptien,* 1887.

For another example of failure to consider in historical deductions obvious mental peculiarities of the religious genius, one may look into the literature which has sprung up around the interpretation of the fact that Jesus spoke of himself as the "Son of Man." All sorts of far-fetched theories are drawn from this phrase in sermonic literature; but the opinions of scientific historians are scarcely less numerous and divergent. Aramaic and Syriac lexicography has been ransacked, arrays of texts are marshalled, and statistical tables compiled therefrom; but all on the tacit assumption that the term in question was deliberately chosen in the first place to represent a clearly defined and fully developed idea, and that it was used in exactly that sense and no other up to the end. So firmly established is this assumption that none of the writers upon the subject, so far as I know them, has given more than passing reference to Ezekiel's use of this term. As he used it, the phrase is simply a circumlocution for "a prophet;" Jesus, during the period reported in the Gospels, undoubtedly meant by the "Son of Man" one greater than the prophets; hence Ezekiel is ignored in favor of Daniel, who uses the term with messianic significance in words quoted by Jesus himself. Yet it is an outstanding fact in the biography of almost any religious genius that his terminology grows with his religious experience. Words that were used early in his career with simple literal significance are gradually filled with new meaning, until they become so highly technical that only a mind saturated with the whole doctrine may fully appreciate them. In the same way it is not a rare phenomenon to find a religious innovator whose system is directly influenced by the choice of his terminology. If in the teaching of other men he discovers connotations and definitions of certain words that he himself has used almost by chance, he may unhesitatingly graft these new ideas into his own system.

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[4] Wm. A. Linn, *The Story of the Mormons,* 1902, 93 (footnote).

If in the light of these psychological peculiarities we suppose that Jesus, early in his career, still looking upon himself as one of the religious leaders known as prophets, adopted Ezekiel's habit of designating himself as the "Son of Man;" and then, in the development of his messianic consciousness, brooding over the Daniel prediction, gradually added to the simple significance the more complex and technical connotation; we shall have a theory which will not only meet the facts of the case but which has the additional strength of support by psychological parallels in the history of other religious founders.

But nowhere perhaps in historical investigation is there greater need of a correct canon of psychological interpretation than in those problems which are concerned with pseudographic writings. To speak of these as forgeries and plagiarisms is to introduce ethical considerations which any study of the abnormalities of religious genius will show are entirely lacking. A greater part of Biblical criticism involves questions concerning documents of just this kind; and it would sometimes seem that the most formidable obstacle in the path towards a scientific solution of the problem is that complex of prejudices and repugnances which arises in most minds the moment it is suggested that a particular document is not from the hand of the author which it claims. The naïve procedure of a pupil who cuts out of his Bible every word and passage that any writer has declared "not authentic" is scarcely less scientific than the refusal of 'conservative' writers on Biblical criticism and patrology to consider evidence and arguments that they would accept unhesitatingly were the documents in question of secular instead of ecclesiastical origin.

There is, for example, the problem of the *Second Epistle to the Thessalonians,* which is still accepted by many modern scholars as of Pauline authorship, though perhaps the majority have come to deny its authenticity, on the grounds of its formal verbal identities with the *First Epistle,* and its explicit denial of doctrine propounded in that document. The general tone of the *First Epistle* is benignant, enthusiastic and impetuous; and it speaks of the second coming of the Lord as imminent, during the lifetime of the writer and those he addresses. The general tone of the *Second Epistle* is chiding, repressive and authoritative; and it teaches that before the second coming of the Lord certain events must occur, and presumably that ages will first elapse. Furthermore, it denounces roundly those who refuse to perform their routine duties in life. Besides the fact that most of the introduction follows the *First Epistle* word for word, there is the significant statement that the letter is written lest " ye . . . be troubled, either by spirit, or by word, or by *epistle as from us,* as that the day of the Lord is just at hand . . ."

To reconstruct in the following terms the circumstances under which this second document was written, is not, it would seem, a gratuitous flight of the imagination, but as scientific a procedure by the laws of psychological analogies as it is for a palaeontologist to sketch an extinct animal from the evidence of some fossil bones. Certain early Christians were so impressed with the imminence of the Second Advent that they dropped the distractions of routine life and gave themselves entirely to contemplation and preparation for the catastrophe. In refutation of their fellows who disputed this theory, they exhibited as the words of Paul what we now have as the *First Epistle.* Among the anti-adventists who refused to be silenced by this argument was one who offered in refutation, not a rival interpretation of the passage, but a whole-hearted denial that Paul wrote the words at all; some evil-minded person had forged the whole epistle or had tampered with the text. Next, brooding over the matter, he undertook to rewrite the epistle as Paul probably had written it, copying exactly the insignificant introduction, but changing entirely the doctrinal portion, omitting the personal references that had only temporary significance, but warning explicitly against forgery as if it were foreseen, and railing at those who refused to work. With the revision once written, the next easy and final step would be the gradual conviction in the mind of the revisor that the document thus produced was wrought under divine inspiration or by the miraculous spiritual dictation of Paul himself. Later generations, ignorant of the local controversy of their fathers, would, of course, treasure both documents without distinction since each professed to be from the hand of the revered apostle.

II

These three examples of the ecclesiastical historian's direct application of methods and materials that are purely psychological are given, not so much for their intrinsic value, as in the hope that such a concrete presentation of sample problems may start some psychological reader upon investigations consciously directed towards the solution of other historical questions. For there are numerous matters in which generalizations derived casually from readings in religious biography cannot meet the needs of the historian; in these he must await the scientific pronouncements of the professional psychologist. It may not, therefore, be amiss if, before sketching the main outlines of a few of these open questions, I attempt to describe the kind of solution that will best meet the needs of historical investigation.

In the first place, it must always be borne in mind that history is concerned with immediate, not ultimate psychical causation. However important the primitive origin of any given psychical manifestation may be in the eyes of pure science, it is only those phases which emerge from the mental surface that belong to the field of history. Psychoanalysis may be able to trace the religious ideas of an individual behind the veil of his consciousness, but the historian has no right or authority to penetrate that dim realm. Roughly speaking, it may be said that the historical record of any religious figure is complete when it has accurately described what he believed that he believed, how he presented his belief, and the way in which his followers understood that presentation; in the main, the course of religious history consists of little more than an overlapping series of these triple elements.

It is extremely unfortunate, so far as the study of history is concerned, that modern psychology is so largely analytical and so sparingly descriptive. In a science so young it is natural, of course, that the general problems should be worked and reworked many times before specific manifestations can be brought to the front; but the religious historian awaits somewhat impatiently the growth of an adequate mass of studies of specific religious types. Though it be not altogether just, he is inclined to complain that in general, when psychologists do treat of religious phenomena, they use such matters only to illustrate some broader psychological hypothesis, or that frequently, being interested in the main thesis, their selection of fact is not a happy one.

Similarly, in the eyes of the historian, it is unfortunate that in discussing religious experiences the psychologist so frequently uses imperfect written records rather than first-hand, modern sources. Looking at the field in a broad way, one can not but be struck by the fact that there is a situation which may be caricatured in these terms: one writer describes the psychology of Joanna Southcott by analogy to that of Joseph Smith, another justifies his estimate of Joseph Smith by the presence of similar mental peculiarities in Joanna Southcott.

But it is inevitable that such cross-arguments must continue until we have adequate scientific studies by competent psychologists upon contemporary religious founders. The religious genius is always with us; probably, if one were to count them over, he would find no decade since 1800 which has not seen the establishment in America of a new religion which won more converts during the first twenty-five years of its existence than did Christianity in its first two centuries. No generation lacks its believers in a gospel written yesterday, or even in a contemporary incarnation of divinity. The historian cannot express too emphatically his hope that the religious psychologist will leave to him the biographies of Joanna Southcott, and will go out with a note-book to some dingy hall where a new gospel is preached. Nor can he repress a sigh of regret when he notices that James entered on his career in the Philosophical Department of Harvard one year before Mrs. Eddy opened her "Massachusetts Metaphysical College" in Boston, and then remembers how many of the "varieties" James chose to describe were drawn from the pages of eighteenth century Quaker biographies. Indeed, the same historian must not be judged too harshly if he sometimes declares that James' work, as a psychological treatise, is inferior to Howells' "Leatherwood God," despite the admixture of fiction in the latter.

III

The problems themselves that demand solution are obvious. Division of the individuals to be described into three classes according to their respective functions in the course of a religious movement will show (*a*) the religious genius, (*b*) his personal disciples, and (*c*) ordinary adherents to the system. The groups produced by this functional classification seem to show distinct types of temperament, which deserve separate study.

Of the first of these little more need here be said. The generalizations that the historian has made concerning the psychological abnormalities of the religious genius need verification and restatement; and undoubtedly there remain more traits and habits of mind common to this type which will be discovered only by careful comparative study of living subjects. It has already been pointed out that material for such first-hand investigation is usually available.

In the examples cited in the first part of this paper we have not mentioned the second group, the personal disciples of a founder. These disciples seem to have played an important part in the development of every religious movement. Though they lack the leader's originality, their influence may have been more or less decisive both by their polarization of his teachings as it passed through them to others, and by the reflex action of their understanding of his message upon the master's own final formulation of it. Usually historical sources preserve but little record of these disciples beyond their names, yet now and again one catches hints of the characteristics common to this type,—abnormal willingness to believe, fiery enthusiasm, unbounded liberality to " The Cause," and a lack of candor, often approaching deliberate trickiness, but without that common sense in practical values that sometimes stands out so amazingly in the genius himself.

In the third class the characteristics of the type will usually be found in the highest development in the official clergy; but any study of these professional religionists must be corrected by a corresponding examination of representative laymen, that due allowance may be made for the elements that belong, not to the religious experience, but to the office. That the different religions present specific types and sub-types must be the conviction of every curious observer; whether or not these types are persistent and amenable to classification is another question. Certainly the value of such classification, should it prove practicable, would be so great that one may hesitate to pass over as valueless even so frivolous an observation as Mozley's remark concerning " evangelical sallowness;" " How is it that goodness, poverty, and a certain amount of literary and religious ambition produce an unpleasant effect on the skin? "[5]

From the historian's side these characteristics, whether they be cause or effect, are of supreme importance; if certain temperaments are attracted by corresponding climates in religion there will follow, in the course of years, a reflex action upon the religion itself, tending toward more exact conformity with the religious taste of that temperament. If the existence of such a reaction can be definitely established, it will do much towards explaining on rational grounds the frequent recurrence in religious history of particular phenomenal series. Some writers are inclined to explain these on genealogical lines, bridging any chronological gap with a bold hypothesis of direct influence, while others, with less success, seek to explain them by laws drawn from a philosophy of history devised to meet the occasion; still other writers are content to remark that religious history persistently " repeats itself."

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[5] T. Mozley, *Reminiscences chiefly of Oriel College and the Oxford Movement*, 2nd ed., i., 1882, 243.

In the same way it is probable that the development of descriptive religious psychology will produce much material of importance in the solution of analytical problems. For example, those disquieting figures that appear in every religious body, who admit in private without scruple that they disbelieve in the possibility of any religion at all, and in public are the most zealous observers and advocates of their official creed down to its smallest denominational peculiarities, will probably be found to represent a distinct type of religious experience. Because these men appear most frequently in ritualistic circles, it is often said of them that " their religion is purely aesthetic." If this phrase, which sounds like an explanation, be true, it will be a fact of enormous significance in any attempt to define the religious sense. If on the other hand, as seems more probable, the religious experience of such men is really religious despite the denials of their materialistic metaphysics, that likewise will be a fact of far reaching implications.

Finally, it will be noticed that not until there has been accumulated an enormous bulk of descriptive investigations will it be possible to define the religious experience of normal human nature. Religion necessarily offers many attractions to the abnormal human mind; the high proportion of freaks in any group of religious has persuaded not only many laymen but some scientific men as well that all religious experience is fundamentally pathogenic. Whether this be true or not is, however, a matter of no great significance either for the historian or the psychologist; to both " religion is an incident in human history and a manifestation of human nature," and as such will remain an object for scientific study.

DEATH-PSYCHOLOGY OF HISTORICAL PERSONAGES

By Arthur MacDonald, Washington, D. C.

A summary of the last words of those distinguished people in history, records of whom have come down to us, is a psychology of their death.

Before presenting the results of such a summary, and in order to understand better the significance of words during the dying hour, it may be useful to note a few points as to death itself.

The Dying Hour

In another place, the author has treated in detail the physiology and psychology of death.[1] Here it may be stated that death is neither rapid nor sudden, but is preceded by a period of transition, which begins as soon as the reactionary forces of the organism have ceased and combat has ended.

The death-act is often confounded with the symptoms of disease which precede it. Dying begins after these symptoms have subsided; there is a pause in nature, the disease has conquered, the battle is over, and all is tranquil.

This transition-stage, or dying hour, may last for a longer or shorter time; in the great majority of cases persons are unconscious. Thus the natural death appears to be a brain-death.

But when there is consciousness during the dying hour, it depends upon nutrition and provision of the brain with blood. As there are three ways of physical death, (1) by brain, (2) by heart, and (3) by lungs, so there are three kinds of psychological death. The first is where there is little or no delirium, and intelligence not only continues to the end, but becomes very acute; physical prostration appears to be replaced by intellectual exaltation. Another kind of psychological death results from diseases only secondarily connected with the brain; the mind is in a mixed state between reason and delirium. The third kind of mental death includes all the lesions of the brain, which are almost always accompanied by loss of understanding; delirium is a symptom; there is a general obscuration of intelligence, and complete loss of consciousness.

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[1] *Medical Times*, N. Y. City, 1921.

Fear of Death

In life the fear of anything is often much worse than the thing itself. This is especially true in the case of death. When the dying hour comes, the fear of death disappears.

Whether it is the brain, heart or lungs which give the signal of death, the brain-forces are usually weakened or destroyed first, causing sensation to lessen or cease. Whether there be consciousness to the last, or only at times, depends upon the nature of the disease and the mental and moral character of the person dying; and this in connection with surrounding conditions. In old age, death is the last sleep, showing no difference from normal sleep. The general consensus of opinion based upon the experience of all ages is that the dreadfulness of death and its physical pain are for the most part in the imagination.

Psychological Summary of Death of Distinguished Persons in History

The average man usually dies unknown; whatever he thinks and says is soon forgotten. Fine and significant words may be lost. Now and then a physician may take the pains to note the last words of some of his patients. There may be a dozen cases, or more; but the outcome is small compared with the great number of those dying every day. Therefore almost all the last words recorded at death are those of eminent and distinguished persons.

This table represents a first attempt to summarize the mental condition, at or just before death, of distinguished persons from the beginning of history up to the present time. Only the most reliable sources have been utilized; and even here where there appeared to be any doubt the persons were omitted, so that we have remaining but 794 cases. It must be remembered, however, that very few death-bed experiences are published, and still fewer are described with sufficient accuracy to be made the object of scientific study.

Taking into consideration the very many and varied sources, in all periods of history, which have been consulted, the regularity of the figures in the table is remarkable. Such uniformity, coming out of most heterogeneous conditions, when put into statistical form, suggests that death is a great equalizer and leveler for all humanity. In a way, the conduct and last words of those facing death are a mental and moral test of their real character.

PSYCHOLOGY OF DEATH OF DISTINGUISHED PERSONS IN HISTORY

Profession or Occupation	Number of Persons	Average Age	Manner of Death						At Death Number		Mental State of Dying Persons													
			Violence								Last Words At or Just Before Death													
											Number of Persons							Average Number of Words						
			Executed	Killed in Battle	Suicide	Assassinated	Total	Disease	Little or no Pain	Pain	Religious	Sarcastic, Jocose	Request, Directions Admonition	Question, Answer, Exclamation	Contented	Discontented	More or Less Indifferent	Religious	Sarcastic, Jocose	Request, Directions Admonition	Question, Answer, Exclamation	Contented	Discontented	More or Less Indifferent
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Religious..........	192	56	42	0	0	4	46	146	12	3	39	6	26	10	66	17	30	23	10	17	2	25	22	19
Royalty..........	91	50	23	5	2	7	37	54	5	4	11	7	8	11	13	15	16	18	17	23	6	20	23	12
Military..........	75	51	21	21	1	6	49	26	31	2	2	1	20	8	15	13	17	11	0	16	6	11	16	17
Philosophers......	102	60	7	0	5	2	14	58	5	0	6	6	11	14	28	8	17	9	16	11	5	8	11	20
Literateurs........	106	64	6	2	1	2	11	95	10	0	9	24	8	9	23	10	27	18	12	14	5	14	15	16
Physicians and Scientists..........	54	64	3	0	0	0	3	51	4	2	4	1	8	3	18	3	10	9	0	8	5	23	11	10
Artists............	39	63	0	0	0	0	0	39	4	2	3	2	0	8	7	4	9	21	13	0	5	12	16	9
Poets.............	69	56	3	1	1	0	5	64	3	2	4	5	8	8	17	2	18	20	24	14	3	15	16	15
Statesmen........	96	65	18	1	2	6	27	69	6	4	8	5	14	10	26	14	17	14	45	18	3	10	14	12
Women...........	70	56	29	0	3	0	22	48	7	3	7	3	9	6	17	11	9	12	34	63	8	11	15	15
Totals Total Averages....	794	60	142	30	15	27	214	580	59	21	93	60	112	87	227	96	170	18	20	19	5	16	16	14
Percentages.......			18	4	2	3	27	73	63	35	26	17	31	26	46	19	35	Average Variations						
																		13	11	16	3	9	9	9

The persons whose records we have studied are classified, according to occupation, into ten divisions, as indicated in the first column of the table. Those whose profession was of a religious character are the largest in number (192), which is due doubtless to the power of religion throughout all history. Under "philosophers" are included mathematicians and educators. As the number of women was not large enough to make subdivisions, all women are placed together.

It will be seen from the third column of the table, which gives the average age, that the great majority of men who become eminent must live at least fifty years. Royalty and Military show the lowest average age, due in part to the large number of deaths by violence, which is the case also with Religious, Statesmen and Women. In short, all the ages in divisions where there are many deaths by violence would of course have a much higher average age had they lived their natural lives.

If we eliminate this factor of death by violence, the Poets and Artists die the youngest; thus Keats died at 26, Byron at 36, Burns at 37, Poe at 38, and Addison at 47. In the columns for pain or little or no pain at death, it will be seen that in only 80 cases out of 794 was any reference made to this matter, indicating that the question of pain at death is regarded as of little importance. In the last 14 columns of the table is presented the mental state at death or just before death, as shown by the last words. It will be noted, from column 13 at the bottom, that 17 percent were sarcastic or jocose, indicating a high degree of mental control. In fact some of the dying complained that it was taking too long and they were getting tired. A relatively large number (24, or 37 percent.) of writers and authors (literateurs) were jocose or sarcastic or both (column 13); they also were relatively the freest from pain (column 10).

The Military show much the relatively highest number of requests, directions or admonitions (column 14) in their last words. The Philosophers stand relatively high in questions, answers and exclamations (column 15). In general it will be noted (columns 12-15) that requests, directions and admonitions were most frequent (31 percent).

More than twice as many (46 percent.) were contented than were discontented (19 percent), as is seen at the end of columns 16 and 17; this accords with the fact that 65 percent had little or no pain, and 35 percent had pain. Thirty five percent were indifferent (column 18); but they all took about the same number of words to express their feelings (averages, columns 23, 24, 25). While relatively few of the Statesmen and Women were sarcastic (column 13), they took many more words to express themselves (column 20) than the others; the Poets also had as high an average as 24 words. In requests, directions and admonitions the Women show an average of 63 words, which is three times as great as that of any of the others, except Royalty, which has 23 (column 21).

As to expressing contentment or discontentment, the Religious and Royalty used the most words, except for contentment, where the Physicians and Scientists have an average of 22 words (column 23). The Artists and Scientists used the fewest words of all (averages 9, 10) to express their indifference (column 24).

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MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

Communicated by John E. Anderson

I. An Experiment in Time Estimation Using Different Interpolations

By Llewellyn T. Spencer

The problem of the perception of time has been given frequent and almost continuous treatment. Much has been done, especially in Germany, in regard to difference limina, increased uncertainty with increased length of interval, etc. In this country, among other investigators, R. McDougall[1] with thirty subjects found sex differences. He found that women are less accurate than men in their estimates of time-intervals and had a greater tendency to overestimate. Yerkes and Urban[2] conducted a group experiment on a far larger scale, in order to study this difference. Four intervals were used, 18, 36, 72, and 108 sec. The subject recorded his estimation of the interval in seconds. The interval was given by the signals " start " and " stop " spoken by the experimenter. With each of the intervals four different " fillings " or interpolations were used. These were idleness, reading (by the experimenter), writing (at dictation), and estimation (any method except the use of a timepiece). In the experiment approximately two hundred and fifty males and approximately two hundred and fifty females were used. This made a grand total of over 8,400 observations. Of all the observations for all the intervals and interpolations 2.39% were correct for males and 1.05% for females. This bore out McDougall's findings. They also found that the number of correct judgments decreases with the increase in the length of the interval. But upon examining their results, they discovered that in the judgments reported the final digit was most often a multiple of five; 65.9% of the male judgments and 80% of the female judgments ended in 5 or 0. This frequency is too great to be due merely to chance. They also found that even numbers were more frequent than odd numbers as the final digit. The order of decreasing frequency for the last digit was 0, 5, 8, 2, etc. In other words judgments " were strongly influenced by the thought of the conventional time unit, the minute, for in all quantitative work there are errors in favor of the standard of measurement and simple fractions thereof. . . . The longer an interval . . . the more frequently it is judged as the same as the unit or a simple fraction of that unit."[3] Inasmuch as the intervals used were not multiples of 5, it is clear that a certain influence not present in normal behavior affected the judgments of the subjects in the investigation of Yerkes and Urban. The present investigation was undertaken to determine whether a method of judgment which is not subject to this influence would produce results varying significantly from those obtained by Yerkes and Urban. The method chosen was that of reproduction.

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[1] *Science*, N. S. 19, 1904, 708-709.

[2] Harvard Psychological Studies, 2, 1906, 405-430.

[3] *Op. cit.*, 417 and 418.

The interval was given by a sharp rap on the table with a pencil or ruler as a signal of the beginning and of the end of the interval. The subject immediately reproduced the interval by starting and stopping a stop-watch, which was enclosed in a felt-lined case to prevent any reference to the rhythmical ticking. In this way the judgment was freed of any prejudice from "the thought of the conventional time unit." The judgments could be obtained to the fifth of a second. Four intervals were used, 15, 30, 60, and 100 sec. These closely approximate those used by Yerkes and Urban. The experiment was divided into two parts. In the first part the four intervals were given in mixed order, without any interpolation. This corresponds to the "filling" called "estimation" by Yerkes and Urban and will hereafter be designated by that name. The subjects were instructed to reproduce the interval immediately after the final rap, without using any

TABLES OF RESULTS

TABLE I.
15 SEC.

Estimation

Sec.	Cases
10–14	12
15–19	43
20–24	17
25–29	11
30–34	5
35–39	4
40–44	3
45–49	1
50–54	1
55–59	2
60–64	1

TABLE II.
30 SEC.

Estimation

Sec.	Cases
20–24	9
25–29	18
30–34	22
35–39	17
40–44	8
45–49	11
50–54	5
55–59	4
60–64	2
65–69	1
70–74	1
75–79	1
.....	..
105–109	1

TABLE III.
60 SEC.

Estimation

Sec.	Cases
30–34	1
35–39	2
40–44	2
45–49	7
50–54	9
55–59	27
60–64	11
65–69	7
70–74	7
75–79	6
80–84	7
85–89	1
90–94	1
95–99	4
100–104	2
105–109	1
110–114	1
115–119	1
120–124	1
......	..
150–154	1
165–169	1

TABLE IV.
100 SEC.

Estimation

Sec.	Cases
35–39	1
40–44	..
45–49	1
50–54	2
55–59	1
60–64	3
65–69	1
70–74	4
75–79	2
80–84	9
85–89	7
90–94	7
95–99	11
100–104	7
105–109	8
110–114	8
115–119	7
120–124	2
125–129	4
130–134	1
135–139	2
140–144	2
145–149	3
150–154	1
155–159	2
160–164	1
170–174	2
200–204	1

method of rhythmic comparison; that is, counting in the stimulus and response intervals, humming tunes, or thinking poetry, and thus gaining a basis for comparison, were forbidden. During the reproduction the subject was idle both in the first set of observations and in the second set, described below. Ten subjects were used throughout the experiment. These were men of the undergraduate body of Yale College and the Sheffield Scientific School and of the Yale Psychological Department. The four intervals were given twice at each sitting and only one sitting was held each day. No attempt was made to keep the sittings regular, but each subject had five sittings on each interval or interpolation, which makes a total of one hundred judgments on each interval or interpolation. As the subjects were ignorant of the success of their judgments, practice-effects could have small influence. Tables I, II, III, and IV give the distribution of judgments for these four intervals in classes of five.

In the second part of the experiment, the 30 sec. interval was used with the following interpolations: Reading of prose by the experimenter, Reading of prose by the subject, Reading of poetry by the experimenter, Reading of poetry by the subject, and Dictation. The interpolation was of course used only in the stimulus interval, and during the reproduction interval the subject spent the time in idleness as in the period of estimation described above. For the prose, selections from current periodicals were used and familiar and especially rhythmical poems were employed in the intervals devoted to poetry. Dictation was from the material of the reading. Table V gives the distribution of judgments for these five interpolations. For convenience of reference, the 30 sec. interval with estimation (idleness) is included also in this table.

TABLE V

INTERPOLATIONS FOR 30 SECS.

Judgment	Estimation	Prose By Experimntr	Prose by Subject	Poetry By Experimntr	Poetry by Subject	Dictation
5–9						2
10–14			1	1		10
15–19		5	1		2	9
20–24	9	4	6	9	8	10
25–29	18	18	15	5	10	14
30–34	22	24	16	22	16	18
35–39	17	11	19	18	15	8
40–44	8	14	12	24	14	9
45–49	11	9	7	7	13	11
50–54	5	7	11	6	7	3
55–59	4	6	6	3	7	2
60–64	2		2		2	
65–69	1	1	1	3	1	1
70–74	1		2	1	2	2
75–79	1					
80–84			1		1	
85–89					1	1
90–94					1	
95–99		1				
100–104				1		
105–109	1					

Of the nine hundred observations, thirty-five are correct. Responses were read to the nearest second, under the assumption that responses within two-and-one-half fifths either side of the unit second were equally distributed within those limits. This gives a percentage of 3.88% correct. Yerkes and Urban found the percentage to be 2.39%, as stated above.

The effect upon the percentage of correct responses of the prejudice in favor of the multiples of 5 can be approximately calculated. It is evident that this prejudice increases the returns for two of the possible final digits, at the expense of the other eight. It is assumed that this prejudice operates in a fairly regular manner. Such an assumption is always made in the description of any psychological trait. For example, we assume that every time the Müller-Lyer illusion is presented there will be a constant tendency to misjudge the distances. The percentage given by Yerkes and Urban of 65.9% of the responses ending in 5 or 0 represents the expected returns for those digits *increased* by a certain percentage at the expense of the other digits. Of all the responses only 20% should end in 5 or 0, on the basis of pure chance. Hence the difference between the 65.9% obtained and the 20% expected, viz., 45.9%, represents an increase of the returns for 5 or 0, due to the disturbing influence. This percentage should be redistributed among the other eight digits, so that 45.9% ÷ 8 = 5.73% approximately should be allotted to the percentage of cases of the other final digits. I say 'approximately,' for it was shown in their article that the other final digits were also affected by the prejudice, but not by any means to such a great degree. But this addendum of 5.73% refers only to the final digit in each case. We are interested in the correct tens digit as well. Only a portion of this addendum should be allotted to the particular percentage of correct final digits falling within the correct tens. There are obviously cases where the final digit is correct but the tens digit is incorrect. An examination of the data given in the article of Yerkes and Urban shows that 24.86% of the responses were of the correct tens digit. Therefore 24.86% of the addendum should be added to the percentage of cases in which a correct response occurred; and 24.86% of 5.73% is 1.41%. This percentage, then, should be added to the percentage obtained while the prejudice in favor of 5 or 0 was operative. The percentage of correct responses obtained by Yerkes and Urban was 2.39%; and 2.39% + 1.41% = 3.80%. This should give us the correct percentage of correct responses obtained when the prejudice referred to is not present. As a matter of fact, in the present experiment, the percentage of correct responses found was 3.88%. The difference of only .08% between the theoretically calculated percentage and the percentage actually obtained is so small that we are justified in considering that the amounts agree in a striking manner.

It may be objected that the present argument assumes that the judgments were distributed purely by chance, whereas the distribution clearly shows a tendency to judge the interval approximately correctly. It is certainly true that the argument rests upon that assumption. But such an assumption is limited to the distribution *within the tens*. In other words, we assume that, while the ability to judge time-intervals is sufficient to enable us to approximate the correct length, this approximation is only to the correct tens digit and not as close as the final digit. Within the tens the distribution of the responses is assumed to be fairly in accordance with the laws of chance. It is possible to discover from our data whether this is actually the case. An examination of the results gives the following distribution of responses in terms of the final digits within the correct tens.

Distance of digit from correct digit	—4	—3	—2	—1	0	+1	+2	+3	+4	+5
Number of cases	20	25	30	20	35	41	25	30	38	30

This shows that the distribution is approximately by chance and is by no means grouped about the correct digit. It is therefore justifiable to assume the chance distribution within the correct tens, which is required by the preceding argument. If the calculations are correct, this close correspondence between the theoretically calculated percentage and the percentage actually observed confirms the accuracy of the present experiment, and shows conclusively that the prejudice mentioned did decidedly affect the judgments of the subjects of Yerkes and Urban's experiment. The difference in the percentage of correct responses as reported by the two investigations is therefore clearly traceable to the difference in method.

Using the mean as a measure of central tendency, Yerkes and Urban found that the 18 sec. interval of "estimation" was overestimated (by 1.5 sec.). In the present investigation the 15 sec. interval in estimation is overestimated also (by 7.27 sec.). In this the two experiments agree. For the longer intervals there is a marked difference. For the 36, 72, and 108 sec. intervals of estimation they found an underestimation of 2.9, 8.8, and 0.2 sec. respectively. I find an *overestimation* in the case of the 30, 60, and 100 sec. intervals of 8.23, 8.55, and 4.31 sec. This difference in the direction of error may be a significant function of the methods used. Table VI gives the means mentioned, as well as the medians and measures of variability. All were calculated from the unclassed measures.

TABLE VI

Int. in Sec.	Interp.	Mean	σ	PEm	Mdn
15	Estimation	22.27	7.79	.52	19.5
60	Estimation	68.55	21.40	1.44	61.1
100	Estimation	104.31	26.35	1.77	100.5
30	Estimation	38.23	12.20	.82	35.2
30	Prose by Exp.	37.25	12.05	.81	34.8
30	Prose by Subj.	38.36	10.89	.73	36.6
30	Poetry by Exp.	39.07	12.25	.82	37.3
30	Poetry by Subj.	40.89	14.21	.85	38.7
30	Dictation	32.41	10.65	.67	30.3

In the course of the experiment it was noticed that a subject would occasionally lose all track of the interval, or would be disturbed by some thought or unavoidable distraction, so that certain responses were without doubt abnormally shortened or prolonged. The judgment of 105 sec. for a 30 sec. interval of estimation is certainly not free from some uncontrolled influence. In consideration of this fact, it seems that a measure, such as the median, which does not consider the value of the extremes, is a more accurate measure of the central tendency than the mean. Using the median as a measure, the 15 and 30 sec. intervals are seen to be overestimated. The 60 and 100 sec. intervals show no definite overestimation if the median is used.

In summing up, the following points are significant:

1. The method of reproduction produces more accurate results than the method of statement in terms of the standard unit. The error of the latter method in the case of Yerkes and Urban's results was theoretically calculated, and the corrected percentage of correct responses is shown to agree with the percentage obtained by the former method.
2. While the percentage of correct responses is greater when the method of reproduction is used, the percentage thus obtained (3.88%) is very low and shows that time-estimation is not very accurate.
3. Results obtained by the method of reproduction do not agree with those obtained by the method of statement in terms of the standard unit in showing that all intervals of greater length than 18 sec. are underestimated. On the contrary, all intervals were overestimated when the method of reproduction was used. If the median is used as the measure of central tendency, the overestimation of the 60 and 100 sec. intervals is not significant. This is true no matter what interpolations are used.
4. Poetry as an interpolation causes the interval to be estimated as longer than an interval with no interpolation (*i. e.,* estimation) or than one with the reading of prose.
5. The interval is estimated as longer if the subject reads the prose or the poetry than if the material is read by the experimenter.
6. Dictation causes the interval to be estimated as shorter than one with any other of the interpolations used, but not necessarily shorter than the actual interval.
7. The results of Yerkes and Urban agree with this study in the comparison of one interpolation with another, as to the effect upon the judgments, although they vary from the present findings in the relation of the judgments of the intervals to the actual intervals.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LIII. The Involuntary Response to Pleasantness

By G. H. Corwin

In a recent paper on *Pleasantness and Unpleasantness in Relation to Organic Response,* Young says:[1] "While U is associated with a large variety of reflex movements, P is passive and negative. Our data do not contain a single case of active reflex response to a P stimulus. With U there are withdrawing movements, frowning, straining, reflexes of expulsion, etc., while P is characterized by mere acceptance of the situation and the passive yielding to it. . . . Muscular strain (tension) is correlated with U, while its opposite, relaxation, is correlated with P.

"The traditional relation between P and seeking movements finds little support in our reports, while that between U and withdrawal is abundantly confirmed. The entire evidence for seeking movements, as 'expressions' of P, is found in 11 reports, in which the subject "sniffed," "took a deep breath in order to get more," "held the breath," etc. In every case these seeking movements are deliberate and never reflex, like most of the withdrawing movements of U. It is a question to what extent and in what sense voluntary deliberate behavior based upon the knowledge that P may be produced or prolonged, or U avoided, can be considered an 'expression of feeling.'"

This result, that the typical involuntary reaction to P is a relaxation and not a seeking movement, is contrary, as Young says, to psychological tradition; and it occurred to us that it might be occasioned by the method which Young employed. In fact, he himself implies that, under other conditions, another result might have been found. "It should be remembered that any result is a function of conditions. Our subjects were seated quietly in a Morris chair, instructed to be 'passive and receptive' and 'to let the experimental situation have its full normal effect.'" We have therefore undertaken in our experiment to put the *O* in a different situation. Instead of giving him a stimulus under conditions in which a seeking movement would be not only unnecessary but also impossible, and further instructing him to be 'passive and receptive,' we have tried to create a situation in which the *O* must seek if he desires to retain a P, and also to find an instruction under which he might move without disobeying it.

Our experiments fall into three series according to the stimuli used. We shall designate them as the olfactory, cutaneous and auditory experiments.

The Olfactory Experiment. The *O*'s were seated in an ordinary chair in a room especially prepared for the experiment. To the right of the *O* a Ludwig kymograph was placed upon a table. This was concealed by a grey screen. The revolving drum had been removed, and a wooden rod 58 by 2.8 cm. was screwed to the center of the revolving disk. A clamp was fastened to the free end of the rod in such fashion that the stimulus-vials could be easily inserted and removed. By means of this apparatus a vial could be placed on the end of the rod directly under *O*'s nostrils; when the kymograph was set in motion, the stimulus would recede from him at the rate of 1.7 cm. per sec. A small electric fan, which served the double function of keeping the air in motion and of eliminating the noise of the kymograph, was operated during this experiment.

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[1] P. T. Young, this Journal, xxxii, 1921, 38ff.

As a check upon *O*'s involuntary movements we took graphic records throughout this series. For this purpose a band was placed around *O*'s head and a cord extended to an ergograph which carried a pointer. By this arrangement any backward movement of the head was indicated upon the drum by a line in an upward, and any forward movement by a line in a downward direction.

The *O*'s were Assistant Professor L. B. Hoisington (H), Dr. H. G. Bishop (Bi), and Miss D. B. Dewey (D), a student taking her 'major' in psychology. Despite the fact that H and Bi worked with some knowledge of the problem, H assures us that his results are not influenced by knowledge, and that his reactions were entirely involuntary; whenever Bi suspected that knowledge might have influenced his results, he reported the fact. Every *O* worked two or three hours a week for periods of 40-60 min. The work was done during the spring of 1921. Five other (untrained) *O*'s were occasionally introduced into the experiment as a control. In every case we found positive results similar to, although not as extensive as, those reported by H, Bi, and D.

A preliminary series extending over two weeks was completed in order to determine the most effective stimuli and the best form of instruction. The following stimuli were employed: vanillin and cinnamon (3 to 1); Cashmere bouquet toilet water; whiskey; ol. rutae; carbon bisulphide; asafoetida; and empty vials. The instructions were: "You will be given a series of olfactory stimuli, some of which are *intensive* odors, some have no odors at all. At the signal 'now' smell the stimulus. You are asked (1) to report whether the immediate experience is pleasant, unpleasant or indifferent, and to indicate the *intensity of the feeling* (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong;') and (2) to report all muscular tendencies and organic sensations in any way related to the affective reaction."

From our preliminary series it had become evident that the very instruction 'to smell' might predispose the *O* to continue smelling, and might therefore occasion a pursuit for another reason than P. To offset this difficulty and also other predispositions, such as for example, curiosity, interest, the "laboratory attitude," etc., we introduced as stimuli several empty vials, and informed the *O* in the instruction that some of the stimuli would be *intensive* odors and some would have no odor at all. It turned out, however, that whenever an *O* actually moved or had a tendency to move from any of these extraneous motives, the fact was always reported in his introspections.

We give in Table I a summary of our results in quantitative form. It will be observed that for all *O*'s pursuit movements occurred in about one-half of the cases in which P was reported. Furthermore, for H and D, in addition to definite pursuit tendencies or movements, there was a much larger proportion of characteristic reactions which indicated their desire to prolong or 'get more of' the stimulus. H reports organic stir with P in 17 cases; it is almost never found in the reports of Bi and D. These *O*'s, however, were in 9 and 17 cases respectively actually seen by *E* to make gross pursuit movements.

TABLE I

Observer	H	Bi	D
Total No. Reports	73	46	60
Total No. Reports classified as P	36	26	42
No. of P reports characterized by definite seeking movements or tendencies to movement	15	13	18
No. of P reports characterized by secondary seeking movements, such as: sniffing, raising shoulders, change in breathing, extending arms, etc	34	11	28
No. of P reports with organic stir	17	0	5
No. of P reports with gross observed movement	0	9	17

Examples of the kind of introspection that we have classified in line 4 of the table (definite seeking movements) are:

D. "I felt attracted toward it and then repelled as it [the stimulus] became U." "Pushing of the shoulders and head forward." "Straining of the trunk upward." "Relaxing of my trunk towards the smell." "Moderately P, and I was inclined to follow it." "Tendency to go toward the stimulus not in a relaxed way but in a tense impulse." "I felt a tendency to move toward the stimulus."

Bi. "I felt a general pressure-kinaesthesis that would favor following up the odor, and when the stimulus moved away I felt also a decided pressure in my thigh-muscles. Knowing what I do about the experiment, I am very careful not to read any movement into it. This definite movement of pursuit, however, quite surprised me; it was just as sharply automatic as the movement which one makes when his hat blows off. When the stimulus suddenly disappeared I suddenly felt myself in pursuit." "That's asafoetida! I liked it at first, and then it became U. Both were weak. I seemed to follow it at first and then, when it became U, I stopped." [He followed it and then jerked back.] "I felt muscular sensation in my head as I followed." "I felt like staying with that, keeping right after it."

H. "A general tendency to strain forward which was different from the other sort of expansive leaning." "Tendency to lean." "Tendency to react forward." "As it weakened there was a tendency to try to get more of the odor and there was a tendency to lean over. I must have leaned." "There seemed to be a bodily set of getting the odor; of going after the odor." "Thought I noticed an impulsion to get more of the experience."

Examples of line 5 (secondary movements) are:

D. "Relaxation of the head." "Relaxation of the neck." "Increase in saliva." "My shoulders raised with the inhalation." "Felt my trunk relax toward the stimulus." "Took long breaths to get all the sensation possible." "Tendency to take quick breaths." "Tendency to sit up towards the stimulus."

Bi. " Certain tendencies to keep on sniffing." " Just an easy relaxed kind of P." " One continuous uniform inhalation." " Just a free comfortable inhalation." " Pleasant relaxation all through the body. I had the relaxed, unrestrained breathing that comes with P." " Whole lot of sniffing movements. Kinaesthesis in the throat. Free nostrils." " A feeling in my face, as when I am just about to smile. These facial reactions were directly related to the P." " Bodily relaxation and comfortableness."

H. "A rather expansive tendency, that is, to throw the diaphragm down." " Slight tendency to deeper inhalation." " That same tendency to deeper, prolonged inhalation and rather more rapid expiration." " Certainly a muscular relaxing effect, very different from the experience one gets with U." " Seemed to be a tendency to get more of the odor." " Deep breathing. This sets up certain kinds of pressure in the abdominal region; not very strong in intensity. They have a lively something about them." " There seemed to be a tendency to a general muscular, I was going to say, relaxation, yet not in the sense of going flabby. The tonus of the muscles seems to stay rather high and yet not to constrict." " A peculiar muscular tendency in the direction of relaxation but with a fair degree of tonus." " There is an expansive effect different from relaxation, for there is still a high degree of muscular tonus, the muscles do not go flabby, there is a sort of brightness, a kind of glow to it." " There was muscular relaxation; I don't like that word 'relaxation' because the experience is as much a contraction as it is relaxation; perhaps a different set of muscles is involved. It may be characterized as 'expansive;' a 'glow.'" " There was certainly a muscular tendency both to relaxation and expansion (not going flabby). There is apparently a qualitative difference between the pressures when muscles contract and when they relax. There seemed to be an urge to get more of the stimulus." [*E* asked H to explain his statement about the muscles. H replied that contraction was correlated with U, it had a dull, uniformly intense nature. The experience was more or less definitely localized and definitely limited. Extension was correlated with P, and had a bright quality. It was more generally diffuse, was very indefinitely limited, and its intensity fluctuated within narrow limits.]

Examples of line 6 (organic stir) are:

D. " Made me feel hungry, empty feeling in my stomach." " It made pressure on the wall of the stomach." " I felt contraction of the stomach."

H. " There was a weak organic sensation in the region of the stomach, still there, diffuse, not definitely localized." " A bit of fleeting organic." " Slight pressure in the abdominal region." " Something like organic that eludes me all the time. Rather a bright-like pressure." " Weak organic something in stomach region, very indefinitely localized."

The Cutaneous Experiment. This experiment was conducted in the same manner as the Olfactory Experiment except that the first sentence of the instruction was made appropriate to the change in stimuli; in place of odors, five cutaneous stimuli were used. These stimuli were a draught of cool air, cold iron, a board containing fine sharp points, a board covered with sand-paper, and a board covered with soft fur. These were moved slowly across *O*'s forehead or nose.

It will be noted from Table II that, although the series is short, H found decided movement or tendencies to movement with *every* P experience; also that Bi again reported no organic stir with P. It will also be seen that what we have classified as secondary characteristics of P were less marked and primary characteristics were more marked in this than in the olfactory experiment. There was only one case reported of a change in respiration.

TABLE II

Observer	H	Bi	D
Total No. of Reports	16	13	10
Total No. of Reports classified as P	9	8	5
No. of P reports characterized by definite seeking movements or tendencies to movement	9	4	4
No. of P reports characterized by secondary seeking movements, such as expanding, change in breathing, changing from nose to whole cheek, etc	6	1	2
No. of P reports with organic stir	5	0	2
No. of P reports with gross observed movement	4	3	3

Examples of the kind of introspection that we have classified in line 4 of the table are:

D. "Tendency to follow the stimulus in order to keep it next to the neck."

Bi. "I did have a tendency to put my cheek on it and follow it." "A little follow-up tendency."

H. "Then there was a positive muscular tendency forward." "There was a tendency to lean forward a bit so that the sensation would be less of a ticklish sort, but a rubbing with a fair degree of pressure." "I did not realize how P it was until it was over. When the support was removed I toppled over." "Same tendency to lean toward the stimulus and to maintain movement." "The stimulus is more at your command here. It is there, you know it is there, and you can do with it what you want. Quite a low level of attention anyhow."

Examples of line 5 are:

D. "Organism seems to expand and to grow tall towards it." "There was a tendency to take long breaths."

Bi. "I changed from nose to cheek."

H. "There was almost a tendency to lean and to expand upward." "There was a muscular tendency of relaxation; it becomes almost a desire to maintain the experience."

Examples of line 6 are:

D. "There was a general feeling of P throughout the entire body."

H. "There was a general liveliness of pressure-quality, an expansive tendency in the abdominal region; the whole muscular system from the hips up was involved in it."

The Auditory Experiment. The same instruction was used in this experiment, except that the first sentence was again made appropriate to the change in stimuli.

Two adjoining rooms were used. In *E*'s room there was a large funnel-shaped horn. From the small end of this horn a 1.25 cm. rubber tube extended through double doors into *O*'s room. While one *E* operated the stimuli, a second *E* moved the free end of the tube gradually away from *O*'s ear.

The stimuli were: pleasing musical selections (victrola records); an unpleasant record made by causing the record to revolve from a point other than the center; chords and discords played on tuning forks; the scratching of the finger nail on sand paper; and the dropping of brass rings.

During the last half of this experiment the *O*'s were definitely instructed "voluntarily to inhibit any movement or tendencies to movement." The introspections marked * were taken from reports under this negative instruction. It will be noted from Table III that Bi reported marked tendencies to movement or actual movement in 14 out of 16 P cases; it is also interesting to note that here he reports two cases with organic stir.

TABLE III

Observer	H	Bi	D
Total No. of Reports	35	31	35
Total No. of Reports classified as P	22	16	28
No. of P reports characterized by definite seeking movements or tendencies to movement	15	14	15
No. of P reports characterized by secondary seeking movements, such as change in breathing, expansion, etc	17	6	23
No. of P reports with organic stir	12	2	15
No. of P reports with gross observed movement	12	8	12

Examples of the kind of introspection that we have classified in line 4 of the table are:

D. "Inclination to follow stimulus." * "Inclination to follow stimulus was repressed because of instruction." "There was a tendency to move the trunk in order to follow the stimulus as it moved in different directions, going away from me and then returning."

Bi. "There was pursuit from two causes, from P of the music itself, and from the effort to hear every note in order to identify the selection." "I felt strain in my neck as if I were following the tube backwards and upwards." "That was very P. The only U part was being compelled to pursue the tube in order to maintain the P experience." * "I found myself wishing that I could stretch out my ear. I am sure that my eyes turned in the direction of the stimulus two or three times. I am sure that I found myself starting to move." * "I could feel a pretty marked incipient tendency toward movement in order to hear the tone better." * "Once or twice there was a definite tendency to incline toward the door. This was when the music became weak." * "I had a great many ideas of movement of pursuit or tendencies to pursuit. Probably these were ideated movements and are just pictorial of what I should have to do in order to follow the stimulus if actual movement were not inhibited."

H. "There was a special tendency to reach toward the stimulus when it became very weak." "The dying out of the tone brought an impulse to move head, this movement was noted after its beginning." "There was a muscular impulse to turn toward or adjust for the having of the experience." "I had an impulse to turn toward the sound source." * "There was a tendency to move my head which was corrected after the movement was made."

Examples of line 5 are:

D. "I took long breaths and relaxed." "There was a tingling in my finger tips. I closed my eyes." "I swayed." "There was a certain amount of rhythmical kinaesthesis." "There was a tendency to take deep inhalations. I always have this tendency with P."

Bi. "There was a tension in my neck of the kind that would turn my ear toward the tube." "There was a P kind of kinaesthesis." "Two or three times I found a tendency to rhythm."

H. "There was relaxation with slight expansion and deep easy breathing." "I breathed rhythmically." "There was an expansive tendency."

Examples of line 6 are:

D. "I experienced a sinking sensation in the pit of my stomach." "My organic system was very relaxed." "My stomach felt heavy."

Bi. "There was perhaps a slight organic 'stir' of P." "There was a kind of organic thrill."

H. "I experienced a diffuse quality that was bright and lively." "There was a diffuse bright pressure." "There was a diffuse expansive tendency in the abdominal region especially, but also in the body in general, a very weak bright-like pressure."

General Conclusions

We find that in 55.7% of the total number of cases classified as P all our *O*'s reported definite movement or tendencies to move; while in 66.6% of the cases secondary reactions characteristic of pursuit or desire 'to maintain the experience' were also recorded. When we note that 28.6% of the cases reporting secondary seeking characteristics were found in P cases other than those classified in line 4, we therefore determine that definitely seeking or maintaining reactions to P stimulation are found in 84.3% of the total number of P cases; whereas Young found no direct involuntary tendencies to movement, or actual movement, reports only 11 cases of the secondary type, and classifies even these as voluntary.

The results obtained from our introspections are completely verified by the graphic records taken during the Olfactory Experiment. We noted, however, that the upward direction (with U-withdrawal) took place immediately, and was steeper than the downward direction. The lines correlated with P were of a gradually sloping nature. This result verifies Young's conclusion that the reaction to U is quicker and more intense than that to P. As the *O*'s varied widely in the intensity of their reaction to the various stimuli, a quantitative statement of the amount of change would be inadequate. In general, the younger *O*'s made more pronounced reaction to both U and P stimulation than the older. Since these reactions were entirely involuntary, there can be no doubt that they are characteristic of P, and were not occasioned by instruction or by laboratory predisposition.

In the case of taste, it is an everyday fact that the stimulus is ejected if U and retained if P. Young does not tell us the results of his chocolate-drop experiment.

It seems from our experiment that, if 'contraction' is a better term for describing the involuntary response to U, 'expansion' is a better term for describing the involuntary response to P. With intense U we found a definite withdrawal, and with weaker U a 'shrinking' of the organism, a desire 'to roll up into a ball,' a 'contraction,' a 'dull pressure quality,' etc. These organic tendencies all carried the meaning of withdrawal. Likewise on the side of P we found that there was either (1) actual pursuit or tendency to pursuit, if the stimulus was sufficiently intensive and there was danger of losing it; or (2), if the stimulus was weak or there was no danger of losing it, an expansion of the organism, a desire 'to get more of the experience,' 'to relax *toward* the stimulus,' 'to relaxation, yet with a rather high degree of muscular tonus,' etc. H has characterized his organic sensations as bright-like, lively, glowing. All these organic sensations carry the meaning of pursuit. In both P and U the intensity of the affective quality is an important determinant of the expression which the response will show.

We believe, therefore, that Young's results are derived from the particular situation under which he placed his *O*'s. As they were seated throughout his experiment in a comfortable chair, they were already relaxed, and therefore it was easier for them to be 'passive and receptive' with P than with U stimuli. Their bodily comfort was a source of constant P stimulation, whereas our *O*'s were indifferent to their position.

A second and more important criticism of Young's method is its inadequacy for producing P responses. If an *O* has a P stimulus placed under his nostrils, there is no incentive for him to make seeking movements, unless *E* begins to withdraw the stimulus. Indeed, when we repeated the Olfactory Experiment under Young's conditions and instructions, we found no definite seeking movements or tendencies to move; though the relaxation reported always had an expansive component.

There is no doubt that the most natural response to U is a movement of withdrawal. The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion,* if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite activity of pursuit or of tendencies to pursuit.

LIV. THE INTEGRATION OF PUNCTIFORM WARMTH AND PRESSURE

By R. S. MALMUD

This study is a companion to the investigations made by Cutolo[1] and Tung.[2] Cutolo determined the psychological result of the simultaneous stimulation of warm and cold spots; Tung did the same thing for cold and pressure spots; and we have extended the inquiry to warm and pressure spots.

The method of this experiment was, in general, that of Tung, and falls into two parts. In the first part we stimulated a spot that was responsive both to warmth and pressure, and asked our *O*'s to report the course of the subsequent experience. In the second part we first aroused a warmth (without pressure) and then excited a neighboring pressure spot, and asked our *O*'s as before to report the course of the resulting experience. As apparatus for the former we employed the thermaesthesiometer used by Tung, but mounted on a mechanical stimulator instead of Thunberg's spring-balance device, and with warm instead of cold water running through the point. The source of the warm water was a tank about 1m. above the apparatus, heated by a Bunsen burner, and connected by a syphon with another tank of cold water; the syphon served the double purpose of permitting a smaller body of heated water, and at the same time of maintaining it at a fairly constant temperature by adding cold water at the same rate as the heated water flowed out. Thermometers were placed both in the heated tank and at the outlet; by these means we were able to determine the loss in temperature between source and outlet; and knowing the total length of the conduit and the position of the thermaesthesiometer within it, we were able to calculate accurately enough for our purposes the temperature of the water at the stimulus. This was on the average found to be 40.5° C, with a range from 38.5° C to 43° C. The temperature of the research room varied between 21° C and 24.5° C. We worked on the skin of the forearm; four or five reliable warm spots were marked by tattooing, and in an experiment the arm rested in a plaster cast. The instruction given to the *O* was as follows: "You will be given a cutaneous stimulus, and you are asked to report the course of the resulting experience particularly as regards its quality and intensity."

As apparatus for the second part of our experiment we used the electric thermaesthesiometer devised by Dimmick for his experiments on the localization of warmth without pressure.[3] This was fastened in the mechanical stimulator, and in an experiment was lowered over a warm spot to a distance of about 5 mm. above the skin; and then, when warmth was reported, was raised 5 or 10 mm. to avoid the arousal of heat. For the pressure stimulus we had intended to use a von Frey hair; but because the radiant heat so curled the hair as to make it useless, we substituted a wooden cylinder, 2.6 gr. in weight, and with a smooth spherical point about 0.5 mm. in diameter. This, held loosely in *E*'s hand, was applied over a hair-bulb adjacent to the warm spot. Since the crucial part of an experiment was the moment when pressure was added to the warmth, we first aroused the warmth, then gave the signal 'now,' and immediately thereafter applied the pressure stimulus. The instruction was: "After the signal 'Now' report any change in the quality or intensity of the experience." The *O*'s were Professor Weld (We), Mrs. A. K. Whitchurch (Wh), a graduate student, and I. Bershansky (B), an undergraduate specializing in psychology.

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[1] F. Cutolo, Jr., this JOURNAL, xxix, 1918, 442.

[2] S. Tung, *ib.*, xxxii, 1921, 421.

[3] F. L. Dimmick, this JOURNAL, xxvi, 1915, 142-157.

Results of the First Method. When a spot on the cutaneous surface that responded both to warmth and to pressure was stimulated with a warm-pressure stimulus, the general course of the experience as described by all *O*'s was, first, a weak pressure (contact), and then a diffuse warmth which with fluctuations gradually disappeared. At times the pressure faded out as warmth appeared; at other times pressure and warmth were present together. In the latter case the pressure was localized either as a core within an area of warmth, or as of the same area and place as the warmth. At times also one of the qualities was focal and the other marginal, and the one focal quality fluctuated in intensity independently of the other; at others both qualities were focal, and they fluctuated together in intensity. When the latter experience occurred, the two qualities were reported as fused, and were characterized as a warm-pressure, or a pressure-warmth. Since these fusions were found also in the second series of our experiments, we shall reserve their further description until we have discussed the results of that part of our investigation.

Results of the Second Method. The course of the experience when a warm spot was first excited without touching the skin, and then pressure added by stimulating a neighboring pressure spot, was in general the same as that obtained by the first procedure. The fact that in these experiments the warmth was already focal when pressure appeared seems to have had no influence upon frequency of fusion. Since, in Tung's experiment, the fusion of cold and pressure appeared more readily when attention was directed to the cold, this result of ours was contrary to expectation. Another difference in this series, and one resulting directly from the change in method, was that of relative localization. In some instances pressure was felt at the side of the warmth, and in others entirely without the area of warmth; in still others, however, it was localized, as in the first experiments, either as a core in the center of warmth, or as coincident in area and place with the warmth. In the first two of these cases fusion never occurred.

The Conditions and Nature of the Fusions. From an analysis of the introspective reports from both series of experiments it seems that fusion never takes place unless both pressure and warmth are of the same degree of clearness, and unless both are localized in the same area, although pressure may be less in extent than warmth. In some (and perhaps in all) cases of fusion, the components fluctuate or decrease together in intensity; a change in the intensity of the one or the other of the two qualitative components disintegrates the fusion. As we have seen above, the fusions are of two kinds: a warm-pressure, and a pressure-warmth. In the former, either a small core of pressure seems to radiate the warmth, or if the areas are coextensive the pressure is more intensive; it has, furthermore, the object-meaning of "being touched by a warm blunt object." In the latter, the areas are always coextensive and the warmth is more intensive than the pressure; and it has the object meaning "of warmth pressing down," or "a large, warm, diffuse, yielding body touching the skin." Typical descriptions of the two kinds of fusion follow.

Warm-pressure. *Wh.* "There was a time at the beginning of the experience when pressure and warmth fused; there was a core of hard pressure from which warmth radiated; it was as if a large hard thing were touching me." "Then they [warmth and pressure] fused; it was like a large warm blunt point pressing." "A big warm blunt thing pressing; then both warmth and pressure decreased in intensity, and then suddenly, instead of having the blunt warm thing touching, I had a warm area with a very weak pressure in the center."

We. "Thereafter the pressure was felt as a warm-pressure; by that I mean a pressure to which the warmth seems to adhere as its quality; both are there together, but the pressure is the more intensive." "Then the two qualities fused to a warm-pressure; the experience was as if I were touched by a warm object."

B. "Pressure and warmth were at the beginning fused; both had the form of a pin-head but were larger. After the pressure had disappeared the warmth became more filmy and diffuse, and without definite form."

Pressure-warmth. *Wh.* "Pressure and warmth were felt simultaneously; pressure seemed to cover the area of the warmth instead of being in the center; it was a fusion, as if warmth were pressing." "Pressure seemed as large as the warmth; there seemed to be a fusion of the two, and the intensity of both diminished and faded out together without fluctuation; the pressure that at the beginning was small fused with the larger warmth when the latter came."

We. "The warmth had an insistence about it that was like a deep pressure, weak in intensity; a pressing warmth." "I can characterize it only as like a warmth from a warm point; *i.e.*, a warmth that was pressury in quality; it was not a warm pressure, it was not as if I were touched by a warm object."

B. "Pressure seemed to fuse with the warmth which became intense but diffuse and filmy, and seemed to flow out of the skin." "The contact seemed like a big warm thing touching me; the pressure got weaker, but warmth remained."

Of equal importance for determining the conditions of fusion are introspections which describe cases of no-fusion. *Wh,* for example, says: "The pressure was spatially unrelated to the warmth; I could not localize the pressure within the warmth; they were two separate experiences, temporally but not spatially related;" and *We* likewise reports: "The contact was at one side of the area of warmth; it inhibited the warmth; but when I searched for the latter it was there at very good intensity; the two impressions were not experienced together;" and again: "Pressure is not felt as a warm-pressure, but rather as a pure pressure within an area of warmth."

The conclusion that the fusion of warmth and pressure is only a pressure-warmth or a warm-pressure was, at first, reached by us with some consternation. Since Tung had found the fusion of cold and pressure to be cold-wetness, we had confidently expected to find warm-wetness. This expectation was further justified by a result of Bentley's, who in experiments with areal warmth and pressure had obtained a few reports of warm-wetness.[4] In the face of our own finding it occurred to us that perhaps our *O*'s did not know the experience of warm-wetness. We, therefore, with two of our experienced *O*'s performed a few simple experiments like dipping the finger into warm water, or causing a drop of warm water to fall upon the back of the hand; but our *O*'s still failed to report warm-wet; they had, it is true, a perception of liquidity, they 'knew' that the finger was immersed in water and must therefore be wet; but there was no experience of wetness; and the only experience from the drops of warm water was warmth and contact. We then, with four *O*'s (Professor Hoisington was added to our group), repeated the significant parts of Bentley's experiment. His conditions were, we believe, duplicated in essential details. Our *O*'s were asked to close their eyes during the time the stimuli were in the room, and after signals to report the qualitative nature of the experience felt at each one of the three stages of the experiment. The results are contradictory. Two *O*'s, *We* and *H,* still fail to describe any experience as warm-wetness; the other two, *Wh* and *B,* report warm-wetness frequently. From a study of these reports it is plain, however, that the experiences which carried the meaning of warm-wetness, for the two latter *O*'s, are not the same. For *B,* when the finger is immersed in warm water (39° C.), warm wet comes immediately; is stable except with 'shift of attention,' when it changes to 'dry' warmth; is localized wherever warmth is localized; is felt when finger or stimulus is not moving; and is not distinguished from liquidity. Whereas for *Wh,* under the same conditions, warm-wet does not come immediately; it is uncertain, instable, has short duration, and fluctuates; it is localized under the finger-nail or on the ball of the finger, or again, as creeping up the finger; it is felt only with movement of the stimulus or of the finger; and, finally, it is distinguished from liquidity as 'less thick.' Our only solution of this diversity of report is that *B* has fallen into the stimulus-error, and that *Wh* gives the meaning of warm-wet to some peculiar or atypical experience. In view of the reports of the other two *O*'s, which are in complete agreement, and also in the light of our main experimentation, we incline to the opinion that there is no typical experience to which the meaning of warm-wet attaches, as there is for cold-wet.[5]

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[4] M. Bentley, this JOURNAL, xi, 1900, 414ff.

Summary. The simultaneous stimulation of warmth and pressure spots may or may not result in a fusion of the qualities of warmth and pressure. Fusion occurs when the two qualities are equally clear and, at the same time, are spatially coincident. The fusions are characterized either as warm-pressures or as pressure-warmths. The adjectival quality is the less intensive, and seems to adhere to (or to form a constituent of) the other.

There seems to be no typical experience of warm-wet.

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[5] Bentley's *O*'s did not, apparently, distinguish wetness from liquidity. See *op. cit.,* 424.

BOOK REVIEWS

Erlebtes und Erkanntes. By WILHELM WUNDT. *Stuttgart,* A. Kröner, 1920. Pp. xii., 399.

What a man has made of his life, Wundt says, is measured by the ratio of *Erkanntes* to *Erlebtes,* of the lessons learned from experience to the data of experience itself. The measure, taken in the rather special sense in which Wundt intends it, gives him a sufficiently large vital quotient; few of us would think of using a box on the ear, received in the first year of schooling, to illustrate the Law of the Continuity of Consciousness! Throughout the book, indeed, *Erkanntes* is far more in evidence than *Erlebtes,* and again and again the reader is diverted from some simple item of narrative to a technical disquisition which contains all the subtlety of Wundt's mature thought and shows all the intricacy of his mature style. Since we are familiar with the contents of Wundt's principal works, I shall pass over these scientific homilies and shall confine myself in the present notice to matters of autobiographical fact and to certain questions of genesis.

First of all, however, I must remark that whatever else Wundt learned in the course of his long life he had not learned fully to know himself. He tells us in the Preface that the cast of his mind was political, and its leading motive a desire to further the interests of state and society. There is, no doubt, a grain of truth in this characterization; Wundt was neither the typical man of science nor the typical philosopher. In the main, however, the statement derives from a very natural illusion. Wundt's boyhood was a time of marked political disturbance; he had worked off the superfluous energy of his early manhood in various social and political undertakings; and the closing years of his life were spent in the shadow of the Great War. The end thus harked back to the beginning, and the Karlsruhe episode took on in memory an undue importance. The illusion is, surely, natural enough,[1] and furnishes us with some thirty pages of interesting and instructive reading. It is, none the less, an illusion, and one to which Wundt himself elsewhere supplies the needed corrective. Every man, he declares, really lives a number of lives, which are more or less closely connected, but which run their separate courses to the end. His own political bent belongs, without question, to a partial and subordinate life whose brief periods of dominance are separated by long stretches of scholarly absorption.

In a recent paper I gave a scanty outline of Wundt's life. This outline can now be filled out, and at one point, where I made an unlucky guess, corrected. At the time when Wundt's schooldays began his parents were living at Heidelsheim. Here he passed two years in the *Volksschule,* and thereafter, until he entered the *Gymnasium,* was taught privately by his father's *Vikarius.* Wundt was a solitary and imaginative boy, averse from games and from school-work, much given to day-dreaming and, later on, to miscellaneous reading of poetry and historical novels. He was the fourth child in the family; but two children had died so early that he had no remembrance of them, and his surviving brother, eight years older than himself, was away at school, a member of his aunt's household in Heidelberg. He thus passed his early years "ohne Geschwister und ohne Mitschüler."

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[1] Readers of the Leibniz-brochure of 1917 will not think it fanciful if I suggest, as a secondary source of Wundt's belief, the parallel between himself and Leibniz.

His tutor, to whom he was so deeply attached that he followed him for a year to Münzesheim, seems to have been anything but strict and to have taught him but little. Wundt was able, nevertheless, to enter the *Gymnasium* at the neighboring town of Bruchsal, where he spent an unhappy year,—whence, indeed, he ran away, only to be escorted back by his mother. The misfit was so plain, however, that his parents determined to remove him to Heidelberg. Here (and not, as I had guessed, at Mannheim) he attended the *Gymnasium* from his fourteenth year until the time came to exchange school for university. The year of entry was overcast by the death of his father; the later years passed happily. Wundt was taken in hand by his brother, who saw to it that he worked with some steadiness; he made friends, and was pledged to membership in a student *Korps;* following the example of his uncle Arnold, a professor in Tübingen, he devoted himself enthusiastically to the study of the anatomy of the brain; he read widely, and on long walks with his fellows discussed what he had read; in a word, he lived the normal, companionable life of his age and environment.

Required, now, to make choice of a career, Wundt after some hesitation decided upon medicine. He had no special call; but he wanted above all things to get away from home; he had been interested, as we have seen, in brain anatomy; and as a beginning student of medicine he could, with some show of reason, migrate to Tübingen for his uncle's instruction. The year 1851-2 saw him, accordingly, a student at that university. In the summer of 1851 he returned to Heidelberg, with increased knowledge of brain anatomy but with very little beside, save the conviction that nature had not meant him to be a physician. He was forced afresh to a decision, and his choice fell on the new experimental science of physiology. His first Heidelberg year was spent upon preparatory studies in mathematics, physics and chemistry. Bunsen's lectures had well-night seduced him into preferring chemistry to physiology; but he remained true to his considered choice, and compromised only in so far that his first scientific publication—duly listed in the JOURNAL's bibliography under the year 1853—dealt with a problem in physiological chemistry. During the second year he studied clinical pathology and pathological anatomy. A prize-essay, in preparation for which he performed, at home and with the help of his mother, a series of vivisectional experiments on the rabbit, brought him a word of commendation from Johannes Müller. The third year —there must have been a third medical year, if later dates are to square—seems to have been devoted to the special medical subjects; I cannot make out that Wundt marks it off, definitely from the year preceding. At the end of this third year, as it would appear, he took his *Staatsexamen,* and came out first in all three divisions: internal medicine, surgery and obstetrics.

In spite of his success in examination, Wundt was still not disposed to attempt a medical practice. He accepted a semester's position as substitute assistant in Hasse's Clinic, and became so keenly interested in his work there that he again had thoughts of deserting physiology, this time for pathological anatomy. In addition to his regular duties, he prepared an experimental thesis for the doctorate of medicine at the University: it was published in 1856. At the expiry of his term of service he determined, with a little money in hand, to go on a physiological pilgrimage to Berlin, and there to utilize a final semester in work with Johannes Müller and du Bois-Reymond. He entered the University of Berlin in the spring of 1856; began at once a couple of experimental investigations, the results of one of which led to publication; and came back to Heidelberg, his student-days over, in the late summer of the same year.

Wundt's next step was to offer himself as *Privatdozent* in physiology to the medical faculty of his own university. There was very little formality in the matter: the doctorate thesis was accepted as *Habilitationsschrift,* and examinations were dispensed with. He straightway advertised a six-hour course in physiology, with demonstrations and experiments, which attracted an audience of four. But he had undertaken too much. A series of hemorrhages cut short the lectures and the beginnings of research; physicians and family despaired of his life; and it was a full year before his health was thoroughly restored. His experiences and reflections during this illness had a permanent effect upon his philosophy and his idea of the relation of philosophy to science.[2] When he was able to take up his work again, apparently early in 1857, he busied himself with an experimental study of muscular contraction, in continuation of an *Arbeit* suggested by du Bois and begun in Berlin. The book, Wundt's first book, came out in 1858,—dedicated to du Bois, published by Vieweg, "mit vorzüglichen Abbildungen der Apparate und Methoden ausgestattet;" outwardly as inwardly a joy to the heart of its young author. But books have their fates: and this book was destined, since it did not please du Bois, to issue stillborn from the press. At first, Wundt says, he was distressed; then, resigned. Later he began to see the humor of the situation, and later still he forgot the whole affair. Now, in his old age, he reviews his early work, and finds it, on the whole, very good.

When Helmholtz was called to Heidelberg in 1858, Wundt offered himself as assistant, and was accepted. Neither Helmholtz nor Wundt had any very precise idea of what the duties of an assistant might be; Helmholtz vaguely suggested microscopy, which Wundt did not affect. The Government presently cut the difficulty by requiring all students of medicine to take a semester's work in the physiological laboratory; and Wundt accordingly found his mornings, from 8 to 12, given over to elementary instruction and indifferent students. He held out for some years, but finally resigned the position (the story of his dismissal by Helmholtz is pure myth) to devote himself to the writing of his Physiology and Medical Physics. As to his relations to Helmholtz, it is hardly an exaggeration to say that they were non-existent. Helmholtz was as silent in fact as the great Moltke is in legend, and Wundt, the assistant, did not venture to ask his professor questions; and though Helmholtz unbent in society, he unbent for social relaxation, and not for the discussion of scientific problems. It was unfortunate, too, that both Helmholtz and Wundt were now occupied with the same subject, sense-perception. Wundt in two passages comments a little regretfully on this coincidence; and there can be no doubt that the community of interest, taken together with a radical difference of perspective, held the two men apart. So we have a few anecdotes, and we have a sort of apology for Helmholtz' liking of foreigners, and that is all.

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[2] The incident is discussed by W. Wirth, in the memorial article in *Arch. f. d. g. Psych.,* xl. I regret that I cannot say more of it in this place.

The biographies state that Wundt gave up his assistantship in 1864 and was promoted to *extraordinarius* in the same year. Unless I have somewhere missed a sentence, the date is not mentioned by Wundt himself. He tells us that the *Beiträge* were inspired, primarily by E. H. Weber and by his own experiences of cutaneous anaesthesia in Hasse's Clinic, secondarily and more remotely by Johannes Müller. The notion of creative synthesis came to him, in connection with the problem of visual space perception, in the spring of 1858. The genesis of the *Physiologische Psychologie* is less clear, and Wundt's account of the book itself is, I venture to think, somewhat colored by his later views,—though this coloring, since it shows us what Wundt would have planned if he were writing the work anew, is of importance in its own right. The one thing we see clearly is that Wundt, during the time of his connection with Helmholtz, was steadily reading the standard psychologies of the day. He got up at 5 o'clock in winter to study Herbart's *Psychologie als Wissenschaft;* and he familiarized himself with Lotze, Fortlage, George, Volkmann, and the rest, as well as with the older psychology of Wolff and Kant. If this wider interest appears, in the light of Wundt's whole career, as a natural consequence of the original interest in sense-perception, I must still believe, as I suggested in the paper already referred to, that it reflects also a direct reaction against the anti-psychologism of Helmholtz. Otherwise we should have heard more of psychological difficulties, and more of the actual motivation of the *Physiologische Psychologie.* The book came out, as we know, in 1873-4, and its successive revisions are described by Wundt as "ein beträchtlicher Teil meiner Lebensarbeit."

We have, I take it, a like admixture of *Wahrheit und Dichtung* in the account of the genesis of the *Völkerspsychologie.* We need not question the statement that, about the year 1860, Wundt conceived the idea of adding social psychology as a sort of superstructure to experimental psychology; the preface to the *Vorlesungen* stands as witness. But Wundt in 1920 had not gone over this work as carefully as he had restudied the *Muskelbewegung.* He forgets that the theory of thought is completed in the first volume, before social psychology makes its appearance; he forgets his express repudiation of Lazarus and Steinthal; he forgets his insistence that the book is a book of individual psychology, and that he introduces the facts of social psychology only to round out this individual psychology, especially on the side of morals and religion. The fact remains, of course,—there is *Wahrheit* amid the *Dichtung,*—that the second volume of the *Vorlesungen* is largely made up of material which was later psychologized in the *Völkerspsychologie,* and that it contains the germs of later theories. Wundt's first lectures on social psychology were, however, delivered at Zurich in the summer of 1875, after the publication of the *Physiologische Psychologie;* and the *Vorlesungen* probably drew less upon special studies than upon the contents of his Heidelberg lectures on anthropology. The new course seems also to have owed something to Wundt's recent occupation with Aristotle.

During his whole long stay at Heidelberg Wundt was busied about his titular subject of physiology. But he also became variously interested in philosophy, and as early as 1866 published his first philosophical book, *Die physikalischen Axiome.* Towards the last this interest must have been recognized as dominant by his colleagues; Kuno Fischer told him later that he had intended to recommend him to a philosophical chair in Heidelberg. The call came, in fact, from Zurich: at first, apparently after the publication of the first half of the *Physio-*

logische Psychologie, as a tentative enquiry, and a year later in definitive form,—a call to the chair of inductive philosophy vacated by Lange. Wundt was not fated, however, to remain long abroad. In May, 1875, he was offered a chair of philosophy at Leipzig. The faculty had first called Fischer, who was unable to accept. Then the idea arose, partly in view of the great age of Drobisch, of dividing Ahrens' professorship, and of securing at the cost of a single philosopher of repute two young men, the one of whom should represent the philological, the other the scientific aspect of academic philosophy. The choice fell upon Heinze and Wundt, whose youth and insignificance inspired a satirical journalist of the day to transform their names into the familiar Hinz and Kunz. One wonders how long that journalist lived! The moving spirit in the whole affair was, curiously, the astrophysicist Zöllner, a man known to psychologists both by his 'illusion' and by his unfortunate association with Slade. Zöllner had a strong leaning toward philosophy, concerned himself actively with the filling of the chairs, and was allowed by an apathetic faculty to pick his men. The Herbartians, Drobisch and Strümpell, had nothing whatever to do with Wundt's appointment,—and another historical anecdote turns out to be mere invention.

Wundt's arrival at Leipzig brings us near the end of his story. He was privileged there to enjoy the friendship of E. H. Weber, the father of experimental psychology, who had still three years to live, and of Fechner, the founder of psychophysics, who lived till 1887. From the first he aspired to organize and direct a laboratory. He started with a few rooms in the old *Konviktgebäude;* in 1892 he obtained more adequate quarters in the *Trierianum;* and in 1897 he moved back to the main building of the university, and took possession of the new-built Institute. In 1913 an extra story was added to the *Paulinum,* over a great part of the Institute proper, and was assigned to *Völkerpsychologie.* Other overhead construction was planned, with a view to a special section of psychophysics; but the war intervened, and psychophysics is now temporarily housed in some of the rooms intended for *Völkerpsychologie.* The three-fold Institute is therefore not yet complete, though it is all laid out and, apparently, promised; and if Wundt did not live to see his early dreams come fully true, we must remember that the scale of actual accomplishment far exceeds anything that he could have hoped for; the half has again proved to be greater than the whole.

In thus restricting myself to biography and to notes on Wundt's principal psychological books, I have left out of account what to most readers will be the really interesting parts of his last work. Beside the scientific homilies to which I have referred we have vivid sketches of men and their manners, of universities and cities; pregnant essays upon certain large questions of education; and a final section on the practical ethics of the Great War. Wundt is animated by a fervid and somewhat narrow patriotism. It is a thousand pities that he never visited England or America; the Anglo-Saxon genius is as foreign to him as it was familiar to Helmholtz, and he writes of the two countries with all the assured superficiality of the outsider who has facts but no perspective. His attitude to the future of Germany is that of chastened but convinced optimism. The *Volk* has been purged in three fires: the Reformation, the Thirty Years' War, and the Great War. It must and will, after the throes of present revolution, return to its native idealism and on that basis reconstitute the German State, which will then play its due part in the furtherance of civilization.—But this and many other passages of *Erlebtes und Erkanntes* demand fuller consideration than is allowed by the limits, however generously drawn, of a book-review.

Fuller consideration of the book as a whole, taken together with many other books, is also needed before *der innere Zusammenhang* of Wundt's life, which his preface leaves us to make out for ourselves, can be brought into clear light. We see that he was one of those not uncommon natures that combine a large capacity for day-dreaming with an obstinate persistence in the day's work. We see that he had a steady dislike of routine imposed from without, though he presently laid a scholar's regimen upon himself; he shrank from the life of a schoolmaster or medical practitioner; he escaped to Tübingen and clung to Heidelberg that he might work and play at his own sweet will. We see that his endowment was general and not special; with better schooling, he might have become a philologist; as it was, he might have been a chemist or a professor of pathological anatomy, or might have remained a physiologist and attained to eminence in that field; and if he was not in fact a politician, he would have made a very good minister in a bureaucratic government. Apart from these generalities three things, in particular, have impressed themselves on me as I have read. The first is Wundt's quite unusual combination of the experimentalist with the generalizing logician. Few men of science, even great men, hold the balance of interest as truly as Wundt held it. The second thing is his extraordinary power of 'cram.' He could keep available an extended and intricate subject-matter for as long as he needed it, and could then drop it, and turn fresh to a new topic. I know no better word than 'cram' to use for this capacity, but it was cram carried to the *n*th degree. The third thing—a character that I have called attention to in my biographical paper—is Wundt's imperative tendency to systematize the unripe. He was not a systematist of pure blood; for that, his thinking (if I may say so with all respect) was too turbid, too little clear; neither, of course, was he the mere encyclopaedist; he was continually essaying system. All these points are illustrated in his sixty years of psychologizing. They do not account for the psychological trend: if we are ever to understand that, we must know more of *Angelegtes*, and relate it both to *Erlebtes* and to *Erkanntes*. E. B. T.

L'année psychologique, vingt et unième année (1914-1919), ed. by H. Piéron. Paris, Masson et Cie., 1920, pp. xii, 522. Price 35 fr.

Psychology welcomes the reappearance of *L'année psychologique*. Founded in 1894 by Alfred Binet, it appeared regularly every year until the 20th volume had been published in 1914. Then the war intervened, and for the next six years publication ceased. Now it appears once more as a sign that French psychologists are again at their habitual work. The '*Année*,' as it is familiarly called, has from the first occupied an unique place among psychological periodicals. It was designed, as its title implies, principally as a year-book of psychology. It was the founder's original plan to divide the book into three parts: the first to contain original articles, essays and experimental investigations (chiefly from the laboratory of the Sorbonne), and general reviews of important topics written by experts in their special fields; the second to consist of digests of all the significant books and monographs of psychological interest which had appeared during the year; and the third to be a psychological index for the year. With this schema it was his policy to cover as much of the whole range of psychology as possible for the twelve-month period. Both schema and policy were maintained until 1904, when the policy was broadened to include occasional articles and reviews by specialists in fields adjoining psychology,—particularly in cytology, in the anatomy, physiology, and pathology of the nervous system, in mental pathology, anthropology, criminology, the pedagogy of the normal and abnormal child, ethics, and philosophy. In order to make room for this extension of programme the psychological index was dropped. Then, in 1908, came a still more marked change in policy; for in that year Binet announced his purpose henceforth to devote the major part of the *Année* to problems in practical and social psychology. This plan was put into effect and adhered to until his death in 1911. In 1912 the *Année* was edited by Larguier des Bancels and Th. Simon, and consisted entirely of *mémoires originaux.* The following year, 1913, the editorship was assumed by H. Piéron, Binet's successor at the Sorbonne, who has in general followed the policy laid down by Binet in 1904.

The high quality of the *Année* throughout its history is beyond question. Many of the research articles, particularly those written by Binet, alone or in collaboration with Simon, have had a decided influence upon the subsequent history of psychology. Furthermore, its pages have frequently been the means of publication for the best contemporary psychologists of France, for Beaunis, Ribot, and Henri (all three of whom were at one time or another co-editors with Binet), for Bourdon, Féré, Foucault, Piéron, Janet and Wallon. But the uniqueness of the *Année* lay in the nature of its general reviews, and in the character of its *analyses bibliographiques.* The former possessed an insight and an authority which still make them valuable as summaries of the period which they reflect; the latter had as ideal so thorough an abstraction of the principal features of method and result as to make it unnecessary for the reader to revert to the original articles. It goes without saying that this aim is in many instances impossible of fulfillment; but with the frequent reprinting of tables and curves, and of figures of apparatus, the digests, made for the most part by the editorial staff, attained as a whole a quite unusual completeness and reliability.

The present volume is a monument to the industry and enthusiasm of Piéron. He and his students have contributed three of the experimental studies, and he himself has written two of the reviews, nearly all of the 600 *analyses bibliographiques,* and the *chronique;* and as a result he, with the aid of his collaborators, has succeeded in producing a volume that is a worthy representative of the series. The first of the *mémoires originaux* is an experimental study of expectation (*problème de l' attente*) from the Sorbonne laboratory by Mlle. Morand; there follow experimental investigations of auditory space perception (*Recherches sur les perceptions spatiales auditives*) by B. Bourdon, of the persistence of an acquired ability (*La persistance des aptitudes acquises*) by M. Foucault, and of a comparison of the memory for meaningless forms with that for numbers (*Recherches comparatives sur la mémoire des formes et celle des chiffres*) by H. Piéron. Following these is an essay on the psychoneuroses (*Essai psychologique sur les psychonévroses*) by M. Mignard and A. Gilles; then comes another experimental study from the Sorbonne laboratory on the correlation of apprehension with memory (*Capacité d'apprehension; rapidité d'acquisition et puissance de rétention de souvenirs bruts: Recherches de corrélation*) by Ot. Vlaïcou; and finally an essay on the types of aesthetic ideas (*Les types d'idéation esthetique*) by M. Grzegorzewska. One cannot escape the impression that, with the exception of the essay on the psychoneuroses, and the investigations of Foucault and Vlaïcou which are perhaps still typical of research in educational psychology, these studies are a little out of tune with present-day psychology. Morand, for example, is content to rest the case of expectation with the conclusion that it is a conscious attitude of the intellectual sort; Bourdon does not bring his results into line with recent experiments on the localization of sound; and Piéron would hardly conceive of his problem today as he did in 1912 when his work began. It is only fair to say that, but for the war, the list would probably have appeared in 1915. There is, however, a result of Bourdon's that is more opportune. He concludes that, aside from intensity, the principal cue to the localization of sound both in distance and in direction is a character which he calls degree of *netteté*. He does not think that timbre in its own right can have much influence on localization. The degree of *netteté* is conditioned in part upon the distance of the sound, in part upon the position of the head relatively to the source of sound, and in part upon differences in timbre (the telephone click is more *net* than the sound of a whistle, and the whistle than the sound of a tuning fork). The psychological nature of *netteté* is, however, not made explicit; other descriptive expressions are clear, clean (*net*), pure (*franc*), distinct (*clair*); "the sound of a whistle," Bourdon says, "seemed to me less soft, less muffled (*moins flou, moins étouffé*) when it was near than when it was far." Although the general significance of these terms is obvious, it is also clear that further analysis is necessary. Whether Bourdon has discovered a phenomenological basis for the localization of sound more study alone can tell; but it would seem that he has shown a way to experimentation that should clear up a long-standing problem.

The Notes and Reviews of this volume of the *Année* consist of a short paper on the *correspondence of intelligence with instinct,* by E. Rabaud; a review of more than 100 titles on the *psychoneuroses of the war,* by H. Wallon; an account of the work done during the war by England, France, Italy and the United States on the examination of aviators (with a bibliography of 67 titles) by H. Piéron; and a report, also by Piéron, of the meeting of the newly-formed psychological section of the French association for the advancement of science held at Havre, July 27-29, 1914. The *analyses bibliographiques* represent a brave attempt to report the principal studies in psychology and neighboring fields during the years of the war. But they are limited principally to American, British, French, Italian, and Swiss sources; with the exception of 7 titles from Pflüger's *Archiv,* no Austrian, Danish, German, or Swedish papers or books are mentioned. The principal parts of the *Chronique* are a note on the part played by psychology in the war; the necrology for the six years (given, unfortunately, without dates); the events of the period, particularly as regards the vicissitudes of psychological periodicals during the war; and finally a refutation of the statement made by an American psychologist that psychological publications in the French language are, in the light of a statistical study, on the decrease. With an author's index the volume is brought to an end.

H. P. W.

Vom Organismus der Sprache und von der Sprache des Dichters. Zur Systematik der Sprachprobleme. By MARGARETE HAMBURGER. Leipzig, Felix Meiner. 1920. pp. vii, 186.

The author finds such days as these well spent in taking measure of the cultural attainments of one's country. Her own contribution is to be a study of speech, since a people's speech parallels its intellectual development; and her particular concern here is the aesthetic side. But two hundred words give small space in which to do justice to one's mother tongue in all its aesthetic aspects, especially when allowance must be made for the repetition and overlapping of a style which is not succinct and an organization by no means compact. And the author's range is ambitious: theories of the origin of speech, the history of its development, an estimate of it as art and as a medium of art, a formal criticism of poets' language are her main headings; and she touches upon other problems, as the possibilities of translation, the existence of synonyms, the nature of genius. Consequently the theories are not always given the evaluation which makes such summaries most worth while; problems are dismissed with the lining-up of authorities, or with a mere statement of opinion; and generalizations are couched in terms which must lead some readers to hope that they were written more easily than they are grasped.

However, the point of view is clear enough,—and incidental in its exposition are pleasing passages of literary criticism. Speech in its beginnings is more than a vehicle of information: it is always a sign that men are not wholly absorbed in satisfying practical wants. And it is, too, at that level, more than the mere play of imitating nature's sounds. The artistic impulses which make poetry are at work when the speech of a people is formed; the early speech is poetry. In it, and even here and there in its descendant, our own talk of every day, we may expect to find the very stuff which we are accustomed to attribute to the poet's peculiar gift: rhythm and rhyme, and the figures of speech. Metaphor, indeed always takes a large part in the making of a language. It intensifies the impressions of one sense by bringing to them those of another. It names new ideational experiences which have no counterpart in expression. And here it is in the nature of metaphor that it conveys the meanings in imaginal form, and keeps always in the hearer the sense of the image. But its intention never is to illustrate; its fitness consists in the unity of its effect: its various images do not become explicit and discrete, but rather give richly on the side of mood. "Grau, teurer Freund, ist alle Theorie, und grün des Lebens goldner Baum" is the example quoted from a later, individual maker of language.

But speech must grow along with the thought of the race. The demand which German thought in its development made of language was that it became less palpable and more abstract. The music was lost, and the metaphors faded to symbols. There is, of course, gain of a sort, and one which the author does not underrate, for the speech which carries evolved thought: it must become highly organized and variously shaded. But it comes a long way from poetry; and the poet meets a difficulty, which gives other artists little worry, in this medium which has lost so much of its vividness in serving non-artistic ends. In his remolding of it for his use, however, it comes to a new birth; and we are to consider that here the making of speech, which has been the work of the race, passes into the hands of creative individuals. Even the smaller men among them have a share in the achievement, for they help in restoring and putting into circulation the old sensory meanings of words, and whatever the centuries may have given in the way of emotional accruals. Or in depicting situations for which current German is especially inadequate, the dialects with their rhythms and moods may be adapted. But revival is not sufficient, and at various times, in protest against the flatness of common speech or heavy tradition of literature, new language has been made. Such a spirit, with the need for a new form to match their thought, inspired the *Stürmer und Dränger.*

This is the outline of Hamburger's view. One of the questions to which it brings her, naturally, is the degree to which speech created by the poet may influence, and come into, the speech of the people. Authorities say that the very color of Goethe's language made it less usable by others; words and structure might be paralleled, but the values were his alone. Other authorities say that Goethe gave German new vigor and new dimensions which are more precious than any material enriching through new colors and tones. The author herself brings instances of words which are the gifts of artists and philosophers; and concludes in general terms that, in so far as the poet crosses the limits of his individuality to find his highest development in the whole, there is harmony between his speech and the speech of all.

Vassar College J. GLEASON

Abzählende Methoden und ihre Verwendung in der psychologischen Statistik. By O. LIPMANN. Leipzig, J. A. Barth, 1921. pp. 78.

This monograph, as its title suggests, deals with the statistical treatment of mass-results rather than with the older established psychophysical methods. The view taken is from the standpoint of the individual within a group of performances, or of the group within different performances, or of different groups within the same performance. The treatment is almost wholly graphical or tabular in nature; very little importance attaches to any single representative value except as it serves as basis for the determination of some new curve. The idea of units of measurement is implied in the taking of the data, in the computation of the single values, and in the numerous interpolations; the actual comparisons are between curves or parts of curves. We have the empirically determined curve given as the standard of reference; hence the measure of any performance is a relative and not an absolute matter. Interest, therefore, seems to attach to differences rather than to uniformities; to the results as a whole rather than to an abstracted result; to relative rather than to absolute values.

The methods and practices are those of the natural sciences and of education,—of the biologist, the mental tester, and the worker in the field of experimental education. The psychophysicist will find the monograph interesting inasmuch as it presents a mode of treatment (it treats of psychophysical problems) which differs from that of the regular psychophysical methods; and he may be brought to believe that a relative judgment, after all, is as far as we may safely go in view of the present state of our knowledge about absolute units.

Because of the complexity and the unobservable nature of the conditions which obtain in psychological work, the mode of statistical treatment given to psychology by the natural sciences is inadequate to psychological data; variations and modifications must be made. The author emphasises the importance of these reforms. No less than fifteen varieties of curves are worked out and graphically illustrated, and more than a dozen others are suggested on the analogy of those given. Wealth in abundance! so one, at first, inclines to say; why, however, do we need a round two dozen measures for one and the same thing? To be sure, every curve or set of curves shows a somewhat different relation between the sets of data, or shows the relation in a somewhat different way.

The more fundamental forms of curve are: the curve of actual distribution, the curve of difference in actual distribution, the curve of percentage distribution, the curve of difference in percentage distribution, the curve of rank order, the curve of difference of rank order, the curve of rank order according to percentage, the curve of difference in rank order according to percentage, the zonal curve in which the point on the abscissa which corresponds to the value found for the measure of central tendency taken has zero-value in terms of the ordinate scale, the zonal curve for percentages, and, finally, curves of distribution of absolute or rank differences of scores in the total series or rank orders of these differences. It is not to be supposed that all these curves employ the crude data, or that the crude data belong exclusively to any curve or set of curves. The notion of the quartile finds extensive application. Since the significance of the difference between the upper and lower quartiles depends in part upon its relative magnitude, measures of variation take the quartile values in relation to some optimal value.

The longest section in the monograph deals with the subjects of correlation, coordination and contingency. As is to be expected, the curves of regression receive more emphasis than does the bare coefficient of correlation. Coordination resolves itself ultimately into equations expressed in terms of the quartile division. The test for contingency receives but little consideration, though it is perhaps adequate for the worker in the field. The monograph closes with a brief discussion of the problem of weighting results.

L. B. HOISINGTON

The Labor Movement: Its Conservative Functions and Social Consequences. By FRANK TANNENBAUM. New York, G. P. Putnam's Sons, 1921. pp. xviii, 259.

Only one chapter in this book falls by title within the province of the JOURNAL, that on "Labor Movement Psychology." The remaining chapters, with their doctrines of the overthrow of the "profit motive" in business, the "displacement of the capitalist system by industrial democracy" and the "rebuilding" of our "social structure" by the labor movement, the development of new industrial "political institutions" as the "inevitable" outcome of the worker's activity, all deal with matters which challenge the professional attention of others than the psychologist. If a reply is to be made to the author's persuasive prophecy that the machine will force us all to become workers, that the foes of the workers will force us all to become members of the union, and that all government will ultimately and rapidly become a form of glorified labor congress with the workers as congressmen, it will be better made by specialists in government.

In the chapter on Labor Movement Psychology, the worker is described as "propertyless," "a wanderer," "a seeker for better things" "a cog." In his work his "self and personality are not involved." His employment is "monotonous," "unbearable." He has neither the "satisfaction of a job well done" nor the joy of completing a "self-made plan." For him the present is "accidental, transitional," the future hopeful. His "drifting body tends to carry with it a restless mind," a "mind that knows nothing of the conserving, constructive experience" of those who are stabilized, for instance, by owning their own land. "Unemployment," "industrial irregularity," "occupational accidents," "the competitive and shifting market," the "adventure of ordinary business," the advent of the machine, have all resulted in "agitation of the mind and discomfort of the body" for the worker. The industrial revolution has equalized the "imagination" of men, and has increased their "irregularity of possession." Now the poor feel "spiritually" equal to the rich. The worker wants much but gets little, and is consequently dissatisfied. Equality in all but money makes money conspicuous, and so simple a difference between the poor and the rich makes the difference "more objectionable," and "desire for adjustment more vivid." Whatever unhappiness the worker feels is the stronger because it is constant and unescapable. He is driven, and driving "suppresses personality," and suppression of personality leads to "rebellion." The intellectual and critical nature of the labor movement in itself makes trouble. "Thoughtful workers" believe the world's ills could be cured "if only there were the will and the desire" to remedy them. "The strength of this conviction is in proportion to the revolutionary idealism of the workers. This conviction leads to the conclusion that the present system is not only bad but is kept so by the perfidy and selfishness of the powers who are benefiting by the present system. It thus adds to the hatred and to the instinctive opposition against being reduced to mechanical instruments by the machine, the belief in the villainous character of the capitalists as a class, a conviction that adds contempt to hatred and leaves a constant bitterness that knows no end." "Opposition by the capitalists—an opposition that is based often upon ignorance and generally upon selfishness and class standards—makes for a constant aggravation of this bitterness." His "psychological maladjustment" makes the worker a ready prey to "emotional appeals" which offer "an easy and ready rationalization of the world and its implications." "The labor movement provides an emotional outlet. It provides room for creative activity." But it does all this "in terms of the values, functions, and problems with which the worker is called upon to deal."

Obviously, such a chapter is not meant as a contribution to psychology. The author simply passes in review the events of the worker's day and the ideas which occupy him, and records the emotional value which he deems them to possess. Psychology has come to mean merely a plausible explanation of everyday experience. It is but fair to repeat that the emphasis of the book falls elsewhere.

Cornell University H. G. BISHOP

The Origin of Man. By CARVETH READ. Cambridge, University Press, 1920. Pp. xii, 350.

This book may be characterized as a psychological interpretation of the better known facts concerning Magic, Animism, Totemism, and concerning the priests and wizards who practice or encourage those rites and beliefs. The first two chapters trace the development of man from an hypothetical primitive stock, a cross between the ape on the physical side and the wolf on the mental, which the writer calls the lycanthropoid stock. These results are highly speculative, obviously. The chapter on Belief and Superstition is an interesting application of the psychology of reasoning, with illustrations from many sources. Much discussion is directed to the problem of the priority of Magic or Animism, with the conclusion that Magic probably comes first; but the interactions of the one upon the other are also traced through two chapters.

The long discussion of the origin of totems inclines to accept Andrew Lang's view that they were names given by outsiders to a group and gradually accepted by the members of the group themselves. Later savage explanations gave rise to many myths which vary from tribe to tribe. The writer believes that exogamy and other marriage customs grew up independently because of social needs and by natural selection, and that the connection established between them and the totems is more or less adventitious. A final interesting chapter traces the historical relations between magic and science, with the conclusion that magic did rather more to hinder than to advance the cause of science.

The book contains little new material, nor all of the very recent material, but is an important working over of anthropological results for the light they cast upon, and receive from, certain psychological doctrines. On many points it may well be recommended to the student of psychology.

University of Michigan W. B. PILLSBURY

The Psychology of Thought and Feeling. By CHARLES PLATT. New York, Dodd, Mead & Co.

This is avowedly a popular presentation of psychology from the point of view of a modified Freudian. It tends to become a discussion of things in general, with a few psychological principles serving as texts. As one might expect, instincts, emotions, and the unconscious form the body of the book. The chapters on Memory, Reason, and Habit contain pleasant descriptions of the superficial processes, with no reference to experimentally determined laws, nor attempt at fundamental analysis.

The last five chapters deal with applications: Education, Mental Ills, The Crowd, The Delinquent. In these, as in less degree in the earlier part of the book, the author appeals to the Unconscious to solve all difficulties. It is a *deus ex machina* which itself needs no explanation. The book is one of the best warnings so far of what we may expect if that tendency should become dominant in psychology. One may quote the writer's own dictum: "'Overbeliefs,' those which are held without the foundation of fact, are no more true when they come through the gateway of science than when they lack this prestige."

The book should appeal to the general public, as it is well written, with many interesting illustrations.

University of Michigan W. B. PILLSBURY

PSYCHOLOGICAL PERIODICALS

Zeits. f. Psychologie. Bd. lxxxiv., Heft. 1-3. P. Busse. 'Ueber die Gedächtnisstufen und ihre Beziehung zum Aufbau der Wahrnemungswelt.' [Reports experiments, performed in Jaensch's laboratory, on three types of image chosen from the memory-continuum (after-images, images of sense-memory, memory-images proper), with observers 11 to 17 years of age. Laws of the comparative behavior of these images are made out for change of the spatial relations of subject and object, for range of concomitant field of view, for clearness, for power of suppression and resistance; and the influence of sense-memory upon perception is demonstrated.] W. Fuchs. 'Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker: i. Verlagerungserscheinungen.' [Third of the series of papers edited by Gelb and Goldstein. Errors of localisation, both relative (tendency toward the point of fixation) and absolute (shift of visual space at large), are explained in terms of attention, which is however taken not as mental faculty but as 'structurally determined reaction' in Köhler's sense.] Literaturbericht.

Bd. lxxxiv., Heft 4-6. A. Gelb. 'Ueber den Wegfall der Wahrnehmung von "Oberflächenfarben:" Beiträge zur Farbenpsychologie auf Grund von Untersuchungen an Fällen mit erworbenen, durch zerebrale Läsionen bedingten Farbensinnstörungen.' [Fourth paper of the Gelb-Goldstein series. Reports cases in which there was loss of surface colors (Katz), and all visual objects appeared filmy or filmy-bulky. Persistence of object-consciousness assured the color-constancy of these films.] H. Friedlaender. 'Ueber Gewichtstäuschungen.' [Reports experiments on the resting skin. Illusions with stimuli differing in material are due to expectation; those with stimuli differing in volume to expectation *plus* the influence of 'seen' density. The 'idea of expectation' involves a complex play of the unconscious associative mechanism, and its effect may be a positive as well as a negative (contrast) suggestion.] K. Scholl. 'Vom absoluten Eindrucke bei Schallstärkevergleichungen.' [Attempts to prove the relative (empirical) nature of 'absolute impression' by the effect of series of single (loud or weak) or paired sounds interpolated between regular series. No clear result can be made out.] Literaturbericht.

Bd. lxxxv., Heft 1-4. Festschrift zum 70. Geburtstage von Prof. Dr. Georg E. Müller. J. Froebes. 'Aus der Vorgeschichte der psychologischen Optik.' [Outlines the views of the Greek, Alexandrian and mediaeval periods (Aristotle; Euclid; Alhazen, Vitello, R. Bacon).] E. R. Jaensch. 'Zur Methodik experimenteller Untersuchungen an optischen Anschauungsbildern: z. T. nach gemeinsam mit F. Reich durchgeführten Versuchen.' [The images of sense-memory follow the general laws of perception: identity of visual direction, retinal incongruity. The eidetic *Anlage* is found in two types of constitution, the tetanic and the Basedow; most cases are clinically quite normal.] D. Katz. 'Psychologische Versuche mit Amputierten.' [Summarises observations on the illusion of the phantom limb, on the sensitivity of the stump, and on the behavior of muscles after Sauerbruch's operation. The perception of weight arises, not in the joints, but in muscle and tendon.] O. Kroh. 'Eidetiker unter deutschen Dichtern: ein Betirag zum Problem des dichterischen Schaffens.' [Discusses the images of sense-memory in the cases of O. Ludwig, L. Tieck, E. T. A. Hoffman, J. V. von Scheffel, and Goethe.] G. Révész. 'Prüfung der Musikalität.' [Describes and evaluates tests of rhythmical sense, absolute and relative ear, recognition of octaves, analysis of dyads, triads, etc., the catching and repeating of melodies, playing by ear.] E. Rubin. 'Vorteile der Zweckbetrachtung für die Erkenntnis.' [A teleologically directed *Aufgabe* has the advantages of cumulative pleasure, analytic procedure, results from unsystematic work, availability of organised memory-material.] F. Schumann. 'Die Repräsentation des leeren Raumes im Bewusstsein: eine neue Empfindung.' [Reduces vision of empty space (as in stereoscopic combinations) to a true sensation, the 'glassy' sensation.] W. Baade. 'Zur Lehre von den psychischen Eigenschaften.' [Psychology has two parts, which deal respectively with the phenomena of consciousness and with the psychical attributes of the organism. These attributes are in turn either general, whose adaptive reactions bear witness to a principle of continuity, or circumscript, whose functions are approximately stereotyped. The writer advocates a special dynamology, or science of the circumscript attributes (Beneke's *Angelegtheiten,* Wundt's psychical dispositions, Müller's residues).]

Bd. lxxxv., Heft 5, 6. L. Bouman und A. A. Gruenbaum. 'Kasuistischer Beitrag zur Vorstellungspsychologie.' [The self-centred day-dreams of a patient who, under laboratory tests, shows small power of visualisation are made up of vivid visual, auditory, haptical images. There are thus objective and personal types of ideation.] J. Plassmann. 'Säkulare Veränderlichkeit des Dezimalfehlers.' [Continues records from 1917 to 1919.] H. Keller. 'Eine Verbesserung am Hippschen Chronoscop.' [Rotary dial.] Literaturbericht.

Bd. lxxxvi., Heft 1-3. W. Fuchs. 'Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker: ii. Die totalisierende Gestaltauffassung.' [Fifth paper of the Gelb-Goldstein series. Experiments show that, under certain conditions, central supplementation may give rise to the appearance of clear vision by the defective or injured parts of the retina. The conditions prove that the result cannot be explained by images or residua or attention; there is a 'totalising apprehension,' a touching-off of a central *Gesamtgestaltprozess.* This same process is normally at work in the filling-out of the blind spot, in twilight vision by the fovea, in certain observations of visual forms (Schumann, Zigler).] H. Henning. 'Ein optisches Hintereinander und Ineinander: Gemischte Farbenempfindungen.' [(1) A simple stereoscopic arrangement permits the seeing of two colored areas at different distances with corresponding (identical) retinal areas; the nearer color is a transparent surface. (2) A modification of von Karpinska's apparatus permits the seeing of differently colored lines (under certain circumstances, areas), with corresponding retinal areas, at identically the same place (*am gleichen Orte der Kernfläche*); the result depends upon accurate fusion of contours and energy of attention. (3) A *Nachtrag* discusses related phenomena and physiological conditions.] *Literaturbericht.* Gesellschaft für experimentelle Psychologie. [Notice of 7th Congress.]

Bd. lxxxvi., Heft 4. E. KAILA. 'Eine neue Theorie des Aubert-Försterschen Phänomens.' [Haploscopic experiments show that the Aubert-Förster phenomenon (optical figures that are small and near are recognized over a larger portion of the field of vision than large and distant figures appearing under the same visual angle) is independent of accommodation and convergence; Jaensch's theory is thus refuted. In point of fact, distant objects arouse more residua than near, and this arousal means mutual inhibition, which is therefore the key to the phenomenon. The Koster phenomenon (enhanced *Eindringlichkeit* in micropsia, and under conditions which destroy visual objectivity) is to be explained similarly, as due to the cutting-off of residua.] K. WINZEN. 'Die Abhängigkeit der paarweisen Assoziation von der Stellung des besser haftenden Gliedes.' [Varied experiments prove that, if two ideas are to be associated, and the one is more holdfast (*besser haftend*) than the other,—more familiar, more insistent,—it is of advantage for retention that the more holdfast idea come first and not second. Hence in learning vocabularies one should place the foreign word to the right, not (as is usual) to the left of the native word.] W. HELLPACH. 'Zu der Bemerkung des Herrn Brugmans in Heft 5/6 des 85. Bandes dieser Zeitschrift.'

Bd. lxxxvi., Heft 5, 6. F. SCHUMANN. 'Die Dimensionen des Raumes.' [Second paper of the series Untersuchungen über die psychologischen Grundprobleme der Tiefenwahrnehmung, ed. by Schumann.—Raises the question whether the truly plastic spatial impression, as distinguished from that of a pictorial representation of space, is in fact tridimensional. Recent experiments have made it certain that two colors may appear at different distances in the same visual direction; and this fact, together with the existence of the colorless glassy sensation, seem to assure tridimensionality. The conclusion, however, is so far only tentative.] E. R. JAENSCH und F. REICH. 'Ueber die Lokalisation im Sehraum.' [Second paper in the series Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter, ed. by Jaensch.—(1) The localisation of the images of sense-memory (*Anschauungsbilder*), whether with steady fixation or with shift of the point of regard, is determined by the region of attention (*Aufmerksamkeitsort*). (2) All the phenomena observable with the use of the three actual threads of the Hering-Hillebrand fundamental experiment on horopter deviation are also observable in the parallel experiment with images of sense-memory; the same three types of observer appear, and in general an observer retains his type in passing from the one to the other experiment. If fixation is long maintained, the phenomena of the fundamental experiment are typically complicated by the supervention of images. These results suggest the use of the image as an instrument of analysis of the fundamental experiment; and it is found that if, in the observation of two or three imaginal threads in the image of sense-memory, one or two are distinguished by the attention, then, as the attention is voluntarily shifted, the preferred thread or threads shift also in the same direction. Since a near-position of attention touches off a tendency to adjustment of attention for distance, and conversely, the whole group of phenomena falls under Hering's principle of the self-regulation of living substance (cf. Mach's antagonistic processes of attention). (3) Hillebrand's explanation of the horopter deviation is unsatisfactory because his experimental arrangements were only partial. Space-values are not stable. The cue to explanation lies in the gross phenomena of the parallel experiment; and the passage from eidetic (imaginal) through hemieidetic to rudimentarily eidetic endowment furnishes the needed continuity between the parallel and the fundamental experiments.] A. Hoefler. 'Meinongs Psychologie.' [An appreciation of the ten papers in the psychological volume of Meinong's *Gesammelte Abhandlungen.*] Literaturbericht.

Bd. lxxxvii., Heft 1, 2. E. Becher. 'W. Köhlers physikalische Theorie der physiologischen Vorgänge, die der Gestaltwahrnehmung zugrunde liegen.' [Popular paraphrase of the argument of Köhler's book "Die physischen Gestalten in Ruhe und im stationären Zustand" (1920), with comment interspersed in smaller type: a useful paper.] G. Marzynski. 'Studien zur zentralen Transformation der Farben. [(1) A given grey in a given illumination may be objectively lightened or darkened *either* by addition of white or black *or* by increase or decrease of total or partial illumination; the resulting difference of intensity of light may then be the same, but phenomenologically the change of brightness is not identical with the change due to total or partial 'transformation.' An examination of the experimental studies of Weber's Law shows that constancy of the differential sensitivity appears only where transformation is in play. (2) A shadowed and an unshadowed paper may be compared for objective, subjective and reductive equality. The shadowed paper looks darker under the subjective than under the objective attitude, but still appears less dark than the reductive procedure makes it. Schools of painting show characteristic differences in their treatment of shadows.] E. Gottheil. 'Ueber das latente Sinnengedächtnis der Jugendlichen und seine Aufdeckung.' [Second paper of the series Ueber die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens, ed. by Jaensch.—Tests made by way of after-image and memory-image bring out rudiments of images of sense-memory even in cases where a direct test fails. An eidetic phase of development is therefore in all probability normal.] E. R. und W. Jaensch. 'Ueber die Verbreitung der eidetischen Anlage im Jugendalter.' [Third paper of the series.—A statistical enquiry confirms the results of the preceding study.] A. Goesser. 'Ueber die Gründe des verschiedenen Verhaltens der einzelnen Gedächtnisstufen.' [Fourth paper of the series.—Experiments on after-images, images of sense-memory, and memory-images, as regards mode of appearance of background, freedom from background, influence of a rotating or uneven background and of a colored projection-surface, and assimilation to objects of perception, all alike indicate that as the memory-series proceeds the connection of the image with the objects of perception simultaneously presented becomes weaker.]

Bd. lxxxvii., Heft 3, 4. B. Herwig. 'Ueber den inneren Farbensinn der Jugendlichen und seine Beziehung zu den allgemeinen Fragen der Lichtsinns.' [This paper, the fifth of the series Ueber Grundfragen der Farbenpsychologie ed. by Jaensch, opens with a full discussion of the nature and distribution of the image of sense-memory, its differences from the ordinary after-image, the conditions of its appearance as positive or negative, etc. The author then shows that many sense-phenomena appear in the image in exaggerated and therefore more readily accessible form. The study of these imaginal effects is therefore instructive for the general psychology of vision: they may help us, *e.g.*, to distinguish color-weakness from color-blindness, inner from outer color-blindness, etc. The phenomena of induction receive special attention, and the normal color-blindness of the peripheral retina is referred to induction rather than to anatomical insufficiency.] E. R. JAENSCH. 'Ueber Kontrast im optischen Anschauungsbild.' [Sixth paper of the series.—Contrast-effects are enhanced in the image.] B. HERWIG und E. R. JAENSCH. 'Ueber Mischung von objektiv dargebotenen Farben mit Farben des Anschauungsbildes.' [Seventh paper. —Phenomena of mixture, less often of rivalry, occur. The results of mixture confirm the status of the image.] Literaturbericht.

Arch. f. d. ges. Psychologie. Bd. xxxix., Heft 3, 4. B. PAULSSEN. 'Einfache Reaktionen bei Variation und rhythmischer Gliederung der Vorperiode.' [The effect of equally spaced auditory stimuli in the foreperiod of auditory reaction differs with the length of the intervals and the tendency of the reactor to rhythmisation. The best result (unity of voluntary process, correct reaction) is obtained with two optimally spaced signals which are apprehended with the stimulus proper as forming an anapaest.] J. O. VERTES. 'Das Gedächtnis der Blinden.' [The immediate verbal memory of blind children is better, both in range and in time of reproduction, than that of normal children.] J. K. von HOESSLIN. 'Die Melodie as gestaltender Ausdruck seelischen Lebens.' [Working from the definitions of melody offered by Lipps and Siebeck, the writer finds that tones affect the inner life of mind both by their manifold interrelations and by the play of tensions which they touch off. The tensions are enough: we have space-melodies in the Gothic cathedral and thought-melodies in the poems of Goethe and Lenau.] E. STERN. 'Zur Frage der "logischen" Wertung.' [Experiments on touch, taste and smell (right-wrong, present-absent) confirm Haering's thesis that cognition is an evaluation.] W. WIRTH. 'Beiträge zur psychophysischen Anthropologie. i. Anomalien der Gesichtsfarbe als Begleiterscheinungen der Farbenblindheit.' [Deuteranopes seem to be markedly ruddy, protanopes pale.]

Bd. xl., Heft 1, 2. C. THEODORIDIS. 'Sexuelles Fühlen und Werten.' [The author starts from the thesis that moral ideas and social organisation have their root in sexual evaluation, and that universally and originally the sexual act is regarded as something mysterious, forbidden, impure: this is the view of the man, who considers that he injures or defiles the woman. After adducing evidence for his views, the author considers certain problems of social psychology, connected with the beginnings of society and with the growth of law.] N. ACH. 'Zur Psychologie der Amputierten: ein Beitrag zur praktischen Psychologie.' [Discusses the mental state of the patient, and his psychological treatment; the choice and use of the prosthesis; and the education of the will to work.]

Bd. xl., Heft 3, 4. W. WIRTH. 'Unserem grossen Lehrer Wilhelm Wundt in unauslöschlicher Dankbarkeit zum Gedächtnis!' [Memorial article with two portraits.] O. KLEMM. 'Untersuchungen über die Lokalisation von Schallreizen: 4. Ueber den Einfluss des binauralen Zeitunterschiedes auf die Lokalisation.' [Discusses the binaural temporal limen, the appearance of a simple subjective field of hearing, localisation and apparent movement in the sense of the temporal difference. Experiments with the Helmholtz pendulum give extraordinarily small times: thus localisation is possible for one observer with a temporal difference of 0.002σ.] E. SCHERER. 'Das Problem der anschaulichen Gestaltung in der Lyrik.' [The sensory and affective reality that is aesthetically characteristic of the best lyric poetry depends, not on the arousal of visual imagery, but upon verbal melody and rhythm: pitch, weight, volume of vowels, duration, accent of syllables. These elements, possibly reinforced by kinaesthesis, are responsible for the plastic effect.] J. E. LIPS. 'Die gleichzeitige Vergleichung Zweier Strecken mit einer dritten nach dem Augenmass: zum Drei-Reize-Problem in der Psychophysik.' [Under the experimental conditions the double judgment has a small advantage in accuracy over the corresponding single judgment. Under the same conditions the normal *DL* is approximately the same as compared with the current values of the simultaneous *DL;* new experiments, however, greatly reduce these latter.]

Psychological Review. Vol. xxvii., no. 1. C. E. FERREE and G. RAND. 'The Absolute Limits of Colour Sensitivity and the Effect of Intensity of Light upon the Apparent Limits.' [With stimuli of high intensity, the limits of R, B, Y coincide with those of white-light vision (the limits for G are narrower); for stimuli of equal energy at medium intensities, the limits of R, B, Y interlace. In the mid and far periphery of the retina, large differences in amount of light are needed to change perceptibly the limits of sensitivity.] H. A. CARR and M. C. HARDY. 'Some Factors in the Perception of Relative Motion: A Preliminary Experiment.' [Observations on two lights, set vertically, either one of which could be given a horizontal movement. Perceptive accuracy is increased by difference in size (or increase in combined area, or both); by inequality of brightness and decrease of combined illumination; by rate of motion; and (in general) by fixation of the stationary light. Extent of motion has no influence, and position of stationary light a variable effect.] S. D. ROBBINS. 'A New Objective Test for Verbal Imagery Types.' [Threefold presentation of specially prepared meaningless syllables. Consonants receive less attention than vowels, and short vowels much less than long. Vivid verbal imagery makes for efficiency.] 'Note on the Verbal Imagery of Stammerers and Normal Speakers.' [Stammerers pay more attention than normal speakers to consonants.] J. R. KANTOR. 'A Functional Interpretation of Human Instincts.' [We must distinguish between instinct (functioning of a connate potential reaction system) and instinctive behaviour (which comprises acquired tendencies of response). We then understand that all instincts are specific; that not a single act of an adult person is instinctive (an original response); and that the emotive situation, by dissociating reaction systems, may leave in function only some instinctive mode of behaviour.] J. P. M'GONIGAL. 'Immobility: An Enquiry into the Mechanism of the Fear Reaction.' [The expression of fear is primarily an effect of thyrine, as that of anger is an effect of adrenin.]

Psychological Review. Vol. xxvii., No. 2. W. D. SCOTT. 'Changes in Some of our Conceptions and Practices of Personnel.' [Emphasises individual differences, the non-rational aspects of behavior, the definition of education as profiting by experience, the idea of workers-in-their-work as opposed to that of pegs and holes, the scientific basis of vocational guidance.] J. J. B. MORGAN. 'An Analysis of Effort.' [Effort is fundamentally a reflex response to an inimical stimulus which the normal organic response has failed to meet.] J. F. DASHIELL. 'A Comparison of Complete *vs.* Alternate methods of Learning Two Habits.' [Experiments under varied conditions on maze running (rats, children, adults), card-sorting and adding (adults), show that learning by the complete method is the more economical procedure.] R. M. Ogden. 'The Tonal Manifold.' [Graphic representation of the system of pitch-brightness, volume and intensity.] C. Rosenow. 'Is Lack of Intelligence the Chief Cause of Delinquency?' [The correct conclusion from Goring's statistics is that in all probability factors other than intelligence are of greater importance as determinants of crime.]

Vol. xxvii., No. 3. C. Spearman. 'Manifold Sub-Theories of "The Two Factors."' [Expands reply to Thomson in *Brit. J. Psych.*, 1916. The fundamental theory has now been demonstrated with finality.] G. H. Thomson. 'General *vs.* Group Factors in Mental Activities.' [The proof of the Theory of Two Factors by hierarchical order falls to the ground, and Spearman's mathematical argument is finally proved invalid. A Sampling Theory of Ability, which considers any performance as carried out by a sample of group factors, is offered as alternative.] J. R. Kantor. 'Suggestions toward a Scientific Interpretation of Perception.' [Perception is the conscious behavior through which are developed the meanings of objects and relations which operate in the adaptation of the individual to his surroundings and in their control.] E. C. Tolman. 'Instinct and Purpose.' [Instincts are defined as (1) determining adjustments, often hierarchically arranged, which set in readiness particular groups of (2) variable subordinate acts. Purpose is the interaction of (1) and (2).] S. B. Russell. 'Brain Mechanisms and Mental Images' [Mental images occur only when there is coordinated molecular response in a brain centre conditioned by a former environment. The response is conditioned by means of registering mechanisms in the centre and association mechanisms.]

Vol. xxvii., No. 4. W. S. Hunter. 'The Modification of Instinct from the Standpoint of Social Psychology.' [The social significance of instinct depends largely on the modification (structural, temporal, adaptive) that instinctive forms of behavior undergo under the influence of intelligent behavior.] E. Isaacs. 'The Nature of the Rhythm Experience.' [Rhythm is the experience arising from the periodic, pendular, reflex response of characteristic organs to objective stimulation. It thus involves perception of stimuli, experience of periodic reflex response, accentuation and grouping by attention, and feeling due to repeated movement.] G. H. Thomson. 'A New Point of View in the Interpretation of Threshold Measurements in Psychophysics.' [Emphasises the influence of 'moral' characteristics or 'mood' upon the *DL*.] J. W. Bridges and V. M. Dollinger. 'The Correlation between Interests and Abilities in College Courses.' [On the face of the returns relative interests are an extraordinarily inaccurate symptom of relative capacities; but the problem is highly complex.] R. H. Wheeler. 'Visual Phenomena in the Dreams of a Blind Subject.' [After sixteen years of blindness the dreams show three peculiarities; the persistence of the synaesthesia of the waking life, the reduction of voices to the subject's own vocal-motor imagery and their differentiation by color, and a tendency to visualise himself at a distance.]

Vol. xxvii. No. 5. L. T. Troland. 'The Physical Basis of Nerve Functions.' [Discusses, on the basis of the work of Nernst, Lillie and Lucas, the general mechanisms of excitation and stimulation, the specific mechanisms of the threshold, impulse propagation, the energetics of nerve processes, the all-or-none principle, and the mechanisms of synapse and receptor; and makes psychophysiological applications.] R. H. Wheeler. 'Theories of the Will and Kinaesthetic Sensations.' [The historical divergence of opinions is due to the prejudice in favor of an unique mental process, to systematic bias and to lack of introspective data. The 'unique mental process' in will is simply kinaesthesis.] W. R. Miles. 'A Pursuit Pendulum.' [The instrument enables us to measure quickness, precision, steadiness of movement in reference to a moving object.] C. E. Ferree and G. Rand. 'The Limits of Color Sensitivity: Effect of Brightness of Preexposure and Surrounding Field.' [Color zones are widest when preexposure and surrounding fields have the brightness of the color. Difference of surrounding field may narrow a zone by 11°; of preexposure field, by 17°; of both together, by 20°.]

Vol. xxvii., No. 6. A. S. Otis. 'Do We Think in Words?' [Thought is restricted neither to the material of language nor to the action of the language mechanisms.] C. H. Woolbert. 'A Behavioristic Account of Sleep.' [Sleep is a matter of the efficient domination of the upper muscular systems by the lower, operating through the relaxing power of fatigue.] E. S. Robinson. 'The Compensatory Function of Make-Believe Play.' [Play, the more private forms of fantasy, much lying and story-telling, and the appreciation of stories all serve the same fundamental purpose as compensatory mechanisms.] E. G. Boring. 'The Control of Attitude in Psychophysical Experiments.' [Gives rules, in reply to Thomson, for the regulation of attitude in psychophysical experiments.] L. A. Jones and P. Reeves. 'The Physical Measurement and Specification of Color.' [Discusses uses of spectroradiometer, spectrophotometer, colorimeter.] S. L. Pressey. 'Suggestions Looking toward a Fundamental Revision of Current Statistical Procedure as Applied to Tests.' ['Reliability' simply means consistency, and 'validity' does not assure value. Tests should be built specifically for concrete problems, and their value measured by their efficiency in dealing with these problems.]

Vol. xxviii., No. 1. M. W. Calkins. 'The Truly Psychological Behaviorism.' [Neither Watson nor Warren, but only the self-psychologist, may lay claim to a genuine behavioristic psychology, a study of the totally integrated individual in the attitudes whereby it confronts its environment.] J. R. Kantor. 'An Attempt toward a Naturalistic Description of Emotions, i.' [The distinctive mark of emotion is the absence in the act of an organised response-system; emotion is a momentary condition of 'no-response.' Analysis reveals three phases: an act of simple apprehension, disintegration of the constitutive response-systems (emotive activity proper), and superseding organic or other activities. Since emotion appears only under definite external conditions the question of inheritance is futile.] H. Lundholm. 'The Affective Tone of Lines: Experimental Researches.' [Lines appear to imitate in their movement the motor expression of emotions, and therefore arouse feeling-tone.] S. C. Pepper. 'The Law of Habituation.' [Secular changes of appreciation, 'value mutations,' are accounted for by a law of habituation. Under repeated stimulation, the members of a linked affective series swing, continuously or cyclically, from dislike to liking.] W. Schilling. 'The Effect of Caffein and Acetanilid on Simple Reaction Time.' [The drugs, in 5-grain doses, retard reaction time and increase unsteadiness. Nothing positive can be said of an effect on pulse and breathing.]

NOTES

ON THE PLAN OF THE *PHYSIOLOGISCHE PSYCHOLOGIE*

The letter of Decr. 8th, 1872, in which Wundt offered the *Physiologische Psychologie* to the firm of Engelmann, contains an outline of the work, which shapes as follows:

I. Physiological Properties of the Nervous System
II. Sensation and Idea
III. Organic Movements
IV. Critique of Psychological Doctrines
V. General Theory of Psychophysical Occurrence

Since II and III comprise "the empirical material of physiological psychology proper"—"the inner or psychological and the outer or physiological consequence of the manifold interactions between our outer and inner experience"—the schema may be rewritten thus:

I. Physiology
II. Physiological Psychology
III. Psychology
IV. Theory of the Relation of Inner to Outer Experience

where the *Physiology* stands for relevant, *i.e.*, neural physiology; the *Psychology* stands mainly for the trends (Wolff and Kant, Herbart and Beneke) against which Wundt was struggling; and the concluding *Theory* discusses, in a wide sweep, "the relation obtaining in the last resort between the worlds of inner and of outer occurrence, and the explanation of the whole interconnection of psychological phenomena suggested by the survey of their borderland." The programme is clear and logical.

The published book is built, however, on a different plan. I shall first give its contents *with one omission.*

1873. Introduction, pp. 1-20.
I. Physiological Properties of the Nervous System, pp. 21-272.
[A. Inner Aspect of Physiological Psychology]
II. Sensation and Sense-Feeling, pp. 273-463.
1874. III. Idea and Aesthetic Feeling, pp. 464-706.
IV. Association of Ideas and Emotion, pp. 726-819.
[B. Outer Aspect of Physiological Psychology]
V. Movements, pp. 820-858.
Conclusion, pp. 858-863.

It is clear that what I have called the inner aspect of physiological psychology is represented, not at the one combined level of the original schema, but at three successive levels, at each one of which the 'objective' experience has its 'subjective' pair. It is clear, also, that the increased space required for this elaboration has meant the curtailment of Parts III, IV and V of the original outline: movement gets only a scant 40 pages, as against over 600 for its co-ordinate Part; the psychological criticisms are packed away in brief appendices to the successive chapters; and the wide-sweeping Conclusion is reduced to exactly five pages! No doubt, the publisher had his say; Wundt had estimated the size of the work at 640 to 800 pp., and it runs to 872. At all events, the mould was now set. Movement never recovers its lost importance; psychological criticism remains to the end a matter of appended paragraphs; and the full Conclusion appears only in the fifth and sixth editions.

So far, then, we have the familiar picture of a work which, through wealth of material, has grown under its author's hands until it outruns the appointed limit; the earlier parts are disproportionately long, the later disproportionately curtailed. But I have oversimplified; I have omitted pp. 707-725, the initial section of Part IV. These twenty pages, which are entitled *Bewusstsein und Aufmerksamkeit,* are of extraordinary significance. Their intervention, between the doctrine of idea and the doctrine of association of ideas, means that a second systematic thread, in addition to the thread of sensory integration, appears in all the following exposition.[1] We should expect a reference to it in the first, physiological Part; but there is no apperception-centre in 1873. We should expect, going further back, a reference in the Introduction; but there is no hint there of consciousness and attention. The fact is that Wundt, when he began to write the book, had no notion that he should presently introduce them. It was only when he came to work up the material of his nineteenth chapter, on the course and association of ideas,—his own experiments on reaction and complication, the results of Donders and Vierordt and the rest,—that, as the Preface informs us, he saw the possibility of further systematisation, of a theory of consciousness and attention which should, at any rate provisionally, round off an important division of physiological psychology. The opportunity thus presented was of precisely the sort that Wundt's genius welcomed. He set to work on the new (eighteenth) chapter; he found plenty of cues to back-reference, where there had been none to reference forward; and the doctrine of apperception was incorporated in the *Physiologische Psychologie.* It came in, nevertheless, by way of after-thought; and though it grew to overshadowing predominance,—the stages of that growth are another story,—we may doubt whether it was ever very firmly rooted; whether (to change the figure) it was ever really at home in the general systematic setting of the book.

E. B. T.

EXPERIMENTAL PSYCHOLOGY IN ITALY

In his inaugural lecture at the University of Palermo Professor F. U. Saffiotti sketches the development of experimental psychology in Italy.[2] The pioneers were three Sicilians, G. Sergi, G. Buccola and S. Corleo. Sergi as early as 1876 advocated the establishment of a laboratory; but nothing came of his efforts before 1889, when a laboratory was founded at Rome as a section of the Institute of Anthropology. Buccola, who died young, worked from 1880 to 1895 with A. Tamburini in the hospital of Reggio Emilia and with E. Morselli in the hospital and the psychiatrical clinic at Turin. Corleo, who died in 1891, started in 1889 a small laboratory, afterwards allowed to lapse, at the University of Palermo. Psychological work, during these early years, was done in hospitals (at Reggio Emilia, for instance, Tamburini and G. C. Ferrari founded a laboratory in 1896) and in physiological institutes (A. Mosso at Turin, M. L. Patrizi at Modena); lecture-courses at universities appear to have depended altogether on the initiative of the professor (Sergi at Messina in 1878-9, Ferrari at Bologna). The first independent university laboratory was opened in Florence, 1903-4, by F. De Sarlo.

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[1] Cf. this Journal, xxxii, 1921, 116 f.

[2] La evoluzione della Psicologia Sperimentale in Italia, *Rivista di Psicologia,* xvi, 1920, 129 ff. Cf. this Journal, xv, 1904, 515 ff; xvi, 1905, 225 ff.

In 1905 the fifth international congress was held at Rome. A direct result was the establishment of three chairs of experimental psychology: S. De Sanctis was put in charge of the laboratory at Rome, F. Kiesow of that at Turin, and C. Colucci received the call to Naples. In 1908 a laboratory of scientific pedagogy at Crevalcore was transformed into a laboratory of pure and applied psychology and, as a communal institute of Milan, placed under the direction of Z. Treves. At the director's death in 1911 this laboratory seems to have declined; his personally owned apparatus were bequeathed to De Sanctis' laboratory at Rome,—where, curiously enough, the psychological section of the Anthropological Institute was still maintained under Sergi's guidance. In 1912 a psychological laboratory, named in honor of Treves, was started by Gonzales and Corberi in connection with the provincial hospital of Milan at Mombello. Finally, Saffiotti was called in 1918 to Palermo, and V. Benussi, apparently in the same year, to Padua.

The *Revista di Psicologia* was started by Ferrari in 1905; it is the organ of the *Società Italiana di Psicologia,* which took shape in 1911. In 1920 appeared the first numbers of the *Archivio Italiano di Psicologia,* edited by F. Kiesow of Turin and A. Gemelli, a pupil of Külpe and Kiesow, now in charge of the laboratory of the Instituto Nazionale Medico-Pedagogico at Milan. Volumes of experimental studies have been issued from Reggio Emilia, Rome, Florence and Turin. E. B. T.

THE PSYCHOPHYSIOLOGY OF THE CONDEMNED

Dr. L. Gualino, director of an Italian war-hospital, has published a paper on the psychophysiological characters of soldiers condemned to be shot for breach of discipline.[1] Pulse is accelerated to 100 at the moment of sentence; is thereafter variable; and sinks to 60 at the place of execution. Sweating is profuse, but a thermal anaesthesia prevents reaction to cold. Salivary secretion is lessened; the voice roughens or fails; tears cannot be shed. Breathing is of the Sikorsky type: the thorax is violently filled or emptied, and the succeeding respiratory movements are superficial and hardly if at all perceptible. There is no impulse to defecate, and no tendency to frequent and scanty urination; a vesical anaesthesia leads, however, to unnoticed overflow of the bladder's contents. The most characteristic physical symptom is a paresis or paralysis of the lower limbs. The face assumes a set, mask-like expression. The pupil alternates between dilatation and normality; in both conditions it reacts normally to light. There is trembling, but (the writer thinks) no true tremor; muscular contractions appear irregularly at various parts of the body, and are probably due to "multiple fibrillary myoclonias." Reflexes are never normal; they may be hightened or diminished. The various modes of general sensitivity show a loss of acuity which may reach actual anaesthesia. The specific sensitivities are rather heightened than impaired.

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[1] Psicofisiologia dei fucilandi: Annotazione obbiettive, *Rivista di Psicologia,* xvi, 1920, 42 ff. The editor, Professor G. C. Ferrari, adds a brief note (101 ff.) entitled Psicologia dei moribondi.

The mental functions, on the other hand, maintain their integrity to the very end. Attention is lively, prompt and well-sustained. Memory is fully efficient. Thought, conception, judgment, reasoning present no change, qualitative or quantitative. The insistent idea is not that of death, or of what is to follow death, or of injustice, but of the unimaginable pain that may attend the moment of dying; the condemned are eager for fact or argument that may reassure them. Nor are the emotions blunted. The principal egoistic emotion displayed, apart from the overshadowing fear, is vanity; the condemned wish to make a good appearance, and are annoyed at the invading paresis; the chief altruistic emotion is a strong and variously directed sympathy. There is little sign of religious emotion. The religion of the condemned, like that of soldiers in general, is strictly utilitarian; their religious images are made, as circumstances suggest, the object of prayer or cursing; they seldom avail themselves voluntarily of the ministrations of the chaplain; and though at last they usually agree to receive the sacraments, this is due more to outside pressure and to the vague notion of a safeguard of the future than to any real religious feeling.

In his conclusion the author points out how far removed from the truth is the common belief that the condemned man is, to all intents, dead before he mounts the scaffold or takes his place on the field of execution. In natural death, it seems that the brain first succumbs, and that spinal paralysis follows later; in these cases of violent death, on the contrary, the cord appears to renounce its functions while the brain remains intact. E. B. T.

LOCOMOTION OF INSECTS

The locomotion of insects when walking with their six legs intact is roughly by three legs at a time, one set of three supporting the body tripod-wise while the other three are advancing. For example, while the right fore leg, the left middle leg and the right hind leg form the tripod of support, the left fore leg, the right middle leg and the left hind leg are advanced; then the last three form the support while the first three are advanced, and so on in alternation. This fact has long been known. Von Buddenbrock set himself to discover what happens when some of the legs (say the middle leg on each side) are removed by accident or amputation, using a walking-stick insect for his experiments.[1] If the original leg-partnerships were to continue, the insect would have a gait like a pacer, moving the fore and hind legs on the same side at the same time. As a matter of fact this is not what happens; but instead it uses the left fore leg with the right hind leg and the right fore leg with the left hind leg after the manner of a trotter, and does so whenever a leg on each side is removed, irrespective of their relation to each other. Removal of a single leg makes no change in the plan of locomotion.

The change of leg-usage when two legs are lacking is advantageous for an insect that must move about back-downward on the underside of leaves and the like, and raises the interesting question how the change is brought about. Is it due to the operation of some general center of locomotor control; is it caused in a purely mechanical way by the changed distribution of the weight of the insect upon the legs that remain; is it a matter of a local neural mechanism? Von Buddenbrock concludes (on the basis of an extended series of experiments which involved, beside the amputations and ingenious tests of other leg-conditions, the sectioning or hemisectioning of the main ganglionic chain at various levels) that the last suggestion is the most probable. The change appears to be in reality no change of locomotor plan at all, but merely the operation of the original neural mechanism within which the relative excitabilities have been altered by the amputations.

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[1] v. Buddenbrock: Der Rhythmus der Schreitbewegungen der Stabheuschrecke, Dyxippus; mit 2 Abbildungen. Biologisches Zentralblatt, Bd. 41, Nr. 1, Januar 1921, pp. 41-48.

For details the reader will wish to consult the original. It may be interesting, however, to note the following additional points with reference to the probable distribution of function within the nervous system of this insect. The *regulation* of the movements of locomotion seems to depend upon the three pairs of thoracic ganglia, but its *inception* upon the suboesophageal ganglion; the beheaded insect remains standing stock still. The head-ganglion or "brain," the receiving center for excitations from the organs of special sense, operates *inhibitively* for the most part, causing normally a quasi-cataleptic rigidity characteristic of the adult insect during the daytime; while removal of the "brain" results in almost continuous locomotor activity.

E. C. S.

GEORGE TRUMBULL LADD

Professor Ladd was born at Painesville, Ohio, January 19, 1842. and died at New Haven on August 8 of the present year. He graduated from Western Reserve College in 1864, and from Andover Theological Seminary in 1869. From 1869 to 1879 he filled pastorates at Edinburg, Ohio, and Milwaukee, Wis.; in 1879 he was appointed professor of philosophy at Bowdoin College, and in 1881 he was called to Yale, where he remained in active service till 1906. He was a lecturer at Andover Theological Seminary in 1879-81; conducted a graduate seminary at Harvard in 1895-96; lectured in Japan in 1892 and 1899, and in India in 1899-1900; and was president of the American Psychological Association in 1893. His books, on theology, philosophy, and psychology, make a long list: his chief psychological works are *Elements of Physiological Psychology*, 1887 (second edition, in collaboration with R. S. Woodworth, 1911); *Philosophy of Mind*, 1891; *Primer of Psychology*, 1894; *Psychology, Descriptive and Explanatory*, 1894.

Ladd was not, in my judgment, a great psychologist; but he was competent and assiduous; and his publications were of very distinct service to the cause of psychology in this country. His *Physiological Psychology*, in particular,—I well remember my excitement on finding this book in the library of the Oxford Union, and the shock of disappointment at reading that mind was a real unit-being!—helped toward the establishment of laboratories and the recognition of experimental psychology as an academic study. Coming, as it did, from a professor of philosophy at Yale who had been a Congregational minister, it gave the young science an air of respectability (I can think of no better word) which was of high advantage in its struggle for life. Aside from this special value, however, the book was important as our one English text-book and book of reference. Those were primitive times: James' *Principles* were still three years away; the translation of Ziehen's little *Physiological Psychology* appeared in a first edition in 1892; and the first instalment of Sanford's *Course* came out in 1894. Even in Germany Wundt's *Physiologische Psychologie* was only just attaining its third edition, and Münsterberg's *Willenshandlung* had not yet seen the light. Ladd had no model save the Wundt of 1880, and his volume embodies an amount of hard work for which we owe him grateful thanks.

By a coincidence which I wish might have been avoided this number of the JOURNAL contains a detailed criticism of Ladd's systematic psychology. I had looked forward to a trenchant rejoinder.

E. B. T.

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